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A  
**DEFENCE**  
OF THE  
**EXPERIMENTS**  
TO  
**DETERMINE THE COMPARATIVE VALUE**  
OF  
THE PRINCIPAL VARIETIES  
OF  
**FUEL**

USED IN THE UNITED STATES, AND ALSO IN EUROPE.

CONTAINING

*A Correspondence with a Committee of the American Academy of Arts  
and Sciences;*

**Their Report, and Remarks thereon;**

*And Animadversions on the Manner in which the Trust confided to the Academy  
by Count Rumford has been managed.*

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By **MARCUS BULL,**

MEMBER OF THE AMERICAN PHILOSOPHICAL SOCIETY, &c.

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## INTRODUCTORY REMARKS.

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WHEN I determined to commence a course of experiments to discover the relative value of different kinds of fuel, and during the long period in which I was occupied in its prosecution, I know of no one thing which would so completely have abated my ardour, and paralyzed my efforts, as a conviction that they would eventually engage me in a controversy, similar to that which I now deem necessary to my reputation. I was stimulated to the performance of the task, which I voluntarily imposed upon myself, by the hope of doing good, and of acquiring some reputation for having done so; other incentives I neither felt, or needed, nor was I aware that any other was within my reach. The time and manner of my being informed of the existence of the Rumford premium will be seen on a perusal of the subjoined pages.

It is unnecessary for me to detain the reader with the history of the circumstances connected with my application to the American Academy, as these are fully made known in the correspondence with the committee, their report, and the remarks thereon, together with the animadversions on the manner in which the trust confided to the Academy by Count Rumford has been managed; but it appears to me proper to assign my reasons for appealing to the press, notwithstanding an unfeigned aversion to authorship.

The account of my experiments on fuel, which gave rise to the correspondence, was read before the American Philosophical Society, on the 7th of April, 1826, and was immediately published by them in their Transactions, and also in the Franklin Journal, and was extensively circulated both in this country and in Europe: it has since been copied in whole or in part in some of the English, and has also been translated and published

in several of the French Journals : and the reviews and notices relating to it, have, it is believed, been uniformly commendatory. Gentlemen of the first eminence in the physical sciences in this city, and in other parts of the Union, have concurred in testifying to the accuracy and importance of my investigations, and the value and novelty of the results obtained. Thus circumstanced, I may justly feel that the arduous labours in which I have been engaged, have acquired for me a reputation which I ought to defend.

Had I not been thus sustained, the opinions expressed by the committee of the American Academy, would have formed the closing scene of my appearance before the public, and I should have hoped that the curtain which had thus fallen, would for ever hide my labours from public inspection : but whilst my own conviction that a partial and incorrect view has been taken of my experiments, is confirmed by the opinions of those who are well qualified to form a judgment, and who have no interest to serve upon the subject, but that of science ; I shall not be accused of temerity, for attempting to prove that the charge of inaccuracy, brought against my experiments, is altogether unfounded ; even though this charge may have emanated from a body so highly respectable as the American Academy.

MARCUS BULL.

*Philadelphia, March 10th, 1828.*

## CORRESPONDENCE.

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No. 1.

*Dr. Bigelow to Mr. Bull.*

BOSTON, June 3, 1826.

DEAR SIR—Your letter of May 8th was duly received; likewise, nine pamphlets by mail, containing the account of your experiments. I lost no time in placing one of your pamphlets on the Academy's table, and another on that of the Athenæum; the remainder were distributed among members of the Academy and scientific institutions.

The Academy met on the 30th. Several claims for the Rumford premium were submitted on behalf of candidates, yours being the first proposed. A committee was appointed to examine these claims, and report at the next quarterly meeting. This committee, after going through the examination of the communications referred to them, are of opinion, that no one of them contains any "discovery or improvement on heat or on light" sufficiently important to entitle its author to the premium.

Your experiments are considered by the committee as deserving great credit, for the ingenuity and perseverance exhibited in their performance. But the following circumstances, among others, are considered at variance with the correctness of the results.

1. The double room employed in your experiments, although it might *retard*, could not *prevent* the escape of heat from the inner room to the atmosphere. Hence the same experiment would not give the same result in a warm day as in a cold one, or in a windy day as in a calm one.

2. A more or less perfect combustion of the fuel, must affect the degree of heat produced in the experiments. This is influenced by circumstances difficult to regulate, such as the shape, position, and subdivision of the fuel; the rapidity of the current of air passing through it, and the amount of combustible surface in contact with fresh air. The smoke, or volatile combustible matter, would in one instance be burnt, augmenting the heat; in another it would not be burnt, but deposit soot.



I will not trouble you with any further objections from the committee, while these are unremoved. In the mean time, they will not report to the Academy until August next, previously to which time their objections will be open to your remarks, should you think proper to address any to them or to me.

I am, dear sir, your obedient servant,  
JACOB BIGELOW.

MARCUS BULL, Esq.

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No. II.

*Dr. Bigelow to Mr. Bull.*

BOSTON, June 26, 1826.

DEAR SIR—On my return yesterday from a journey in the state of New York, I found your two letters of the 9th and 12th inst.\* I regret that my absence should have occasioned any delay in the answers you request.

In these letters you request to be furnished with the remaining objections against your experiments, and also with an explanation of the first objection contained in my letter of the 3d.

As the last inquiry is perhaps the most important, it may properly be first considered.

It is, *what effect we suppose would be produced upon the result of an experiment, whether the day was warm or cold, windy or calm.* The following statement will perhaps make the meaning of the committee intelligible.

Suppose that at the time of beginning your experiment the temperature of your inner room is at  $100^{\circ}$  of the thermometer, the outer room at  $90^{\circ}$ , and the atmosphere  $80^{\circ}$ . (These numbers are hypothetical, others will do as well.) Suppose that, during your experiment, the atmosphere falls to  $50^{\circ}$ . It must follow, that the outer room will give off to the atmosphere a greater quantity of heat, than it would have done in the same time, had the atmosphere remained stationary at  $80^{\circ}$ . Now, to supply this loss of heat, the outer room will immediately draw heat from all warmer bodies in its neighbourhood. These are two, viz. 1st, its own stove; and, 2d, the inner room. It follows, then, that the inner room will *lose* a certain number of degrees or *measures* of heat, which it would *not have lost* had the atmosphere remained at  $80^{\circ}$ .

This may explain why the result of an experiment will be likely to differ in a cold day, from that of the same experiment in a warm one. In like manner, in a windy day, the number of atmospheric particles coming in contact with the building being greater, they will conduct off more heat from the rooms than in a calm day.

\* The purport of these letters being recited by Dr. Bigelow in his reply, their insertion is not deemed necessary.



The foregoing statement is made, to present the subject in a more intelligible form. You will probably be able to infer from it, that the sustaining of a *relative* temperature in two apartments, does not afford a correct indication of the *positive* amount of heat produced in one of them.

In regard to the other objections of the committee, they are principally these. That the subject does not admit of the philosophic accuracy in its conclusions, which you appear to attach to it, and that therefore no two experimenters would be likely to produce the same results. The same species of wood differs according to the season in which it is cut, the dry or wet soil in which it has grown. Heart wood differs from sap wood; young, from old; and wood in which decomposition is begun, from that in which it is not, &c.

The committee appointed at the meeting of the Academy in May, to report on applications for the Rumford premium, are Mr. Daniel Treadwell, Dr. John Ware, and myself. You will excuse the brevity of this letter, as it is written under the pressure of urgent engagements.

With respect, your obedient servant,

J. BIGELOW.

MARCUS BULL, Esq.

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### No. III.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, July 10, 1826.

DEAR SIR—Your letter of the 26th ult. which explains, in a very clear manner, the meaning of the committee as to the first objection made against the accuracy of my experiments, together with their remaining objections, has been received.

To place the subject in the most eligible point of view, I shall transcribe the several objections, and annex my answers.

1st objection. "The double room employed in your experiments, although it might *retard*, could not *prevent* the escape of heat from the inner room to the atmosphere. Hence the same experiment would not give the same result in a warm day as in a cold one, or in a windy day as in a calm one." "Suppose that at the time of beginning your experiment the temperature of your inner room is at 100° of the thermometer, the outer room at 90°, and the atmosphere 80°. (These numbers are hypothetical, others will do as well.) Suppose that, during your experiment, the atmosphere falls to 50°. It must follow, that the outer room will give off to the atmosphere a greater quantity of heat, than it would have done in the same time, had the atmosphere remained stationary at 80°. Now, to supply this loss of heat, the outer room will immediately draw heat from all warmer bodies in its neighbourhood. These are two, viz. 1st, its own stove; and, 2d, the inner room. It follows, then, that the inner room will

lose a certain number of degrees or measures of heat, which it would not have lost had the atmosphere remained at  $80^{\circ}$ . This may explain why the result of an experiment will be likely to differ in a cold day, from that of the same experiment in a warm one. In like manner, in a windy day, the number of atmospheric particles coming in contact with the building being greater, they will conduct off more heat from the rooms than in a calm day."

In reply to this objection I have to remark, that the conclusions to which the committee have arrived, are drawn from false premises; as they overlook the fact that the temperature of the outer room is maintained by heat which may be said to be *entirely independent of that generated in the inner room*, as in no other case could the outer room be of any service whatever. In detailing my experiments, I have stated that I took advantage of the heat transmitted from the interior to the exterior room, and made it subservient, as far as possible, for the purpose of regulating the temperature of the latter room; but as this heat had been measured in its proper place, (within the interior room,) whether it was then permitted to escape, or was made use of for the purpose described, would be entirely immaterial to the result of the experiment, and may be considered as *entirely independent*, as the same amount of heat generated from fuel in the stove of the exterior room.

We will now examine the case supposed by the committee, in which the temperature of the atmosphere, at the commencement of an experiment, should be  $80^{\circ}$ , and subsequently be depressed to  $50^{\circ}$ . The effect would undoubtedly be to depress the temperature of the exterior room, but this could only take place by supposing the operator to neglect his duty, and this would immediately be indicated on the scale of the differential thermometer in the interior room, and also by the thermometer of the exterior room, which we will suppose to have sunk to  $89^{\circ}$ .

Now, the only method which could possibly be taken to restore the relative difference of  $10^{\circ}$  between the two rooms, and maintain the interior at  $100^{\circ}$ , would be to increase the fire in the stove of the exterior, as any attempt to raise the temperature of the latter room by increasing the fire in the stove of the interior, must obviously only increase the difficulty, in proportion to the difference in the content of the conducting surface of the two rooms, or nearly as two to one. As every experiment must be considered a failure, unless completed at the same temperature at which it was commenced; we must now suppose that I have applied additional fuel in the stove of the exterior room, and again elevated its temperature to  $90^{\circ}$ , as this stove, although its office appears to have been overlooked by the committee, is fully competent to counteract the effects of a change even greater than they have supposed. The temperature of the exterior room being restored to  $90^{\circ}$ , I would respectfully inquire of the committee, whether they can suppose, that the transmission of heat from the interior room would not also be restored to its former rate, supposing the atmosphere to remain at  $50^{\circ}$ ? If the interior room, at  $100^{\circ}$ , is constantly surrounded by a thick interstice of air at  $90^{\circ}$ , this must possess the same power of con-

ducting heat, whether the atmosphere is  $50^{\circ}$  or  $80^{\circ}$ , and the only difference in performing the experiment would be, that if at  $50^{\circ}$ , I should be obliged to use more fuel in *the stove of the exterior room*, an important circumstance, which appears not to have been sufficiently noticed by the committee, as they appear to suppose, that part at least of the extra loss of heat, must *necessarily* be drawn from the inner room, as they say—"Now, to supply this loss of heat, the outer room will immediately draw heat from all warmer bodies in its neighbourhood. These are two, viz. 1st, its own stove; and, 2d, the inner room. It follows, then, that the inner room will *lose* a certain number of degrees or *measures* of heat, which it would *not have lost* had the atmosphere remained at  $80^{\circ}$ ." The tendency which bodies of unequal temperatures evince to equalize their heat, when *brought in contact*, is admitted; but I am not aware that cold bodies possess any influence to "draw heat," or increase its radiation through a stratum of *intervening warmer air*. The amount of radiated heat from the same surface, and at the same temperature, I have supposed to be always equal, and that the heat transmitted by *contact* through an air medium, would be always proportional to the *difference* in the temperature of the *bodies in contact*, other things being equal. Now, if the exterior room, by the *aid of its own stove*, is maintained at  $90^{\circ}$ , and no other supposition can be made, whence comes the *necessity* or even *possibility* of the inner room losing a greater portion of heat by radiation or contact, than would have been the case had the atmosphere remained at  $80^{\circ}$ ?

During the great length of time consumed by my experiments, it will be obvious that they must have been subject to all the changes of our variable climate; and as experiments were made upon the same kinds of fuel at *every* season of the year, and with the *same results*, I know not how to offer for the consideration of the committee, more satisfactory practical evidence, as to the correctness of the means made use of to ensure uniform results.

2d objection. "A more or less perfect combustion of the fuel, must affect the degree of heat produced in the experiments. This is influenced by circumstances difficult to regulate, such as the shape, position, and subdivision of the fuel; the rapidity of the current of air passing through it, and the amount of combustible surface in contact with fresh air. The smoke, or volatile combustible matter, would in one instance be burnt, augmenting the heat; in another it would not be burnt, but deposit soot."

In reply to the 2d objection, it will be apparent, that perfect similarity in "the shape, position, and subdivision of the fuel," among articles so dissimilar, would not only be "difficult to regulate," but entirely impossible and unnecessary, and indeed injurious, could it have been done. The object of my experiments being entirely practical utility, to attain that object it was necessary that the different kinds of fuel should be consumed as near as possible in the manner in which this takes place in the ordinary processes to which fuel is applied. "The shape, position, and subdivision of the fuel," so far as it related to *each kind*, was as similar as possible; and as to "the



amount of combustible surface in contact with fresh air, and the rapidity of the current of air passing through it," these would entirely depend upon the kind of fuel experimenting upon; for instance, the quantity of anthracite coal would be larger than would be required of any other article, and the quantity of air admitted was proportional to the heat required to be produced, and its "rapidity," it is presumed, was proportional to the heat, both of which are supposed to have been equal in every experiment. That part of the objection which relates to the different degrees of heat which would be produced by consuming the smoke in one instance, and not in another, will now be answered. As none of the chimney fire-places, grates, or stoves, made use of in this country, do, to my knowledge, possess the necessary requisites for consuming their own smoke, and as the anthracite coals do not present any smoke to consume, I had supposed the only method of making a *fair* comparison would be, to produce as perfect combustion as is ever done in the large way; but\* to have consumed the smoke from the woods and bituminous coals, would certainly have exposed my results to an objection, when compared with anthracite coal, an article of fuel, the value of which, it will be admitted, is of great importance to have accurately ascertained.

3d objection. "That the subject does not admit of the philosophic accuracy in its conclusions, which you appear to attach to it; and that therefore no two experimenters would be likely to produce the same results. The same species of wood differs according to the season in which it is cut, the dry or wet soil in which it has grown. Heart wood differs from sap wood; young, from old; and wood in which decomposition is begun, from that in which it is not."

In reply to this objection, I am quite prepared to agree with the committee, that experiments upon this, as well as upon every other philosophic inquiry, must be supposed to bear the common impress of *fallibility*; but I must protest against the inference to be drawn from this objection, that, because we cannot arrive at *perfection*, we should do *nothing*.

I am not prepared to agree with the committee, that the same *weight* of any particular *dry* wood, would not be likely to produce the same results, by different and equally judicious experimenters; even supposing it to have been cut at different seasons, and to have grown in different soils; as I presume the component parts of the wood would be nearly the same, although I admit that it would probably possess different specific gravity. My experiments on the woods were made with great care, so as to include a proper proportion of *bark*, *sap*, and *heart*, and as some difference might exist between wood of different ages, I selected a medium between the two, ("old and young,") as usually sold.

The largest portion of wood sent to market being *sound*, I did not suppose it necessary to experiment upon wood which had undergone the various degrees of "decomposition" of which it is susceptible; nor am I in possession of any method, if these experiments had been made, by which I could have pointed out to the public the manner of determining the "season" in which wood has been cut, the "soil"



upon which it has grown, its "age," or degree of "decomposition" which it may have undergone.

I remain, very respectfully, your obedient servant,

MARCUS BULL.

TO JACOB BIGELOW, M. D. &c. &c. &c.

#### No. IV.

*Dr. Bigelow to Mr. Bull.*

Boston, August 4, 1826.

DEAR SIR—Your letter of July 10th was received three days since, and has been submitted to the committee. They remain of opinion that their former conclusions, although pronounced by you to be "drawn from false premises," are nevertheless correct and true.

As unnecessary prolixity is, in scientific matters, a great evil, I beg leave to call your attention at once to a remark in your last letter, in which you state, that you are "not aware that cold bodies possess any influence to 'draw heat,' or increase its radiation, through a stratum of intervening warmer air." Now this is the precise point in which the committee and yourself disagree. They have always understood that cold bodies *do possess* an influence to draw heat, or increase its radiation, through a stratum of air, whether warm or cold. They have yet to learn, that radiation can be prevented by the intervention of an atmospheric medium of *any temperature whatever*. And until they are instructed how this may be accomplished, they will retain their former belief, that the results of your experiments must *vary* with changes in the temperature of the atmosphere.

In regard to their second objection, the committee will explain further. Suppose that hereafter two persons should repeat either of your experiments, and that one of them should divide his fuel into ten parts, and place them disadvantageously; while the other should divide the same fuel into twenty parts, and place them more advantageously for the circulation of air. In one case more smoke would be burnt than in the other, more heat produced, and a different result afforded by the experiment. In your letter you state, that "the object of" your "experiments being entirely practical utility, to attain that object it was necessary that the different kinds of fuel should be consumed as near as possible in the manner in which this takes place in the ordinary processes to which fuel is applied." Now, in ordinary practice, there is no very uniform mode of burning fuel, since every man builds his fire differently from his neighbour. If there is any thing in which most of the practical world agree, it is in never using fuel in the state in which you use it, viz. that of absolute dryness.

In replying to the third objection, you protest against the inference "that because we cannot arrive at perfection, we should do nothing." On this subject the committee think, that where we cannot arrive at

perfection, we should take care to do nothing which will lead others into error. In science, a state of ignorance is better than a state of error. No lover of truth can willingly adopt as laws in philosophy, conclusions founded on results which, from their nature, are likely to be overturned by the first succeeding experimenter.

The committee have no further wish in this business, except that you and your scientific friends, in common with themselves, may arrive at a joint understanding of the truth. They are in no haste to make up their report, but will patiently wait for any further remarks you may choose to offer on the subject.

Very respectfully, your obedient servant,

JACOB BIGELOW.

MARCUS BULL, Esq.

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No. V.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, August 11, 1826.

DEAR SIR—I received your letter of the 4th on the 8th inst. The committee will, I hope, excuse the “prolixity” of my last letter. Replications can seldom be as concise as objections.

I must plead great carelessness, as my only excuse, for having stated in my last letter that ‘I am not aware that cold bodies possess any influence to “draw heat,” or increase its radiation through a stratum of intervening warmer air,’ and I feel no reluctance in agreeing with the committee, that radiation cannot be prevented by the mere temperature of an intervening atmospheric medium.

I should have said, after the word *through*, ‘a *surface* uniformly heated by a stratum of intervening air, warmer than the cold bodies.’

The committee will probably agree with me, that the quantity of radiated heat, lost by a heated body in a given time, will be uniform, provided the temperature of its *surface*, and the *surface* of the recipient or colder body in opposition, remain the same; or, in other words, that radiated heat has only to do with the temperature of the *surface* of solid bodies.

Now supposing the internal *surface* of my exterior room to be maintained at a uniform temperature, I contend that a cold body, or the colder parts of the wall *on the outside*, cannot operate to “draw heat,” or increase its radiation from the inner room.

The process of transmitting the heat I conceive to be changed from the radiating to the conducting state, at the moment of absorption at the *surface*; and whether this new process then becomes more or less rapid, is not material, provided we possess at an intermediate point, viz. at the interior surface, a recipient of uniform powers. This I suppose to be effected by the body of warm air in contact with the walls, which is constantly exerting itself, not only to preserve the surface at the same temperature, but to extend its influence even through

the walls, so that every change in the atmosphere is met near the *outside*, and is constantly lessening in effect as it approaches the internal surface.

From the bad-conducting materials of which the room is composed, I am led to believe that no appreciable difference would be found to exist in the temperature of its internal surface, under the ordinary changes of our climate. In this opinion Professors Hare and Silliman both agree.

In confirmation of this opinion, I beg leave to repeat the fact, that the *same results* have been obtained from the *same kind of fuel*, at *every season* of the year.

It appears to me, that the only point of difference between the committee and myself, is, upon the question whether the surface of the walls of a room similar to that used by me, and as uniformly heated, will not be of the same temperature as the air of the room, or of a uniform difference of temperature, under the ordinary changes of our climate. Any person may readily satisfy himself on this point by a simple experiment.

In reply to the further explanation of the second objection, I remark, that it would be very difficult to place in my stove the small quantity of fuel used at one time, in a disadvantageous manner for combustion. If we suppose a second experimenter to divide his fuel in every instance as uniformly, but more minutely than I have done, and to obtain more heat; yet still our *comparative* results must evidently *be the same*.

The quotation from my letter, 'the object of my experiments, &c.' was intended to apply simply to the states of *aggregation* in which the fuel was used. This, it appeared to me, the committee supposed should be the same, whether the article was *wood* or *coal*.

The committee will not, I think, upon reconsideration, object to my having used the fuel absolutely dry, this being the only state to obtain it uniform, and the precise state recommended by Count Rumford, in whose place the committee may be supposed to stand, so far as relates to their official capacity in this business.

Although "in science a state of ignorance is (may be?) better than a state of error," supposing the latter to be material, yet I presume the committee will agree with me, that perfection is rarely if ever attained, and that near approximations are in most cases *all that utility* requires.

As I am not a chemist by profession, I took the liberty of sending the objections of the committee to Professor Silliman, requesting his opinion upon them, which I received a few days since, and take the liberty of transcribing.

I remain, very respectfully, your obedient servant,

MARCUS BULL.

TO JACOB BIGELOW, M. D. &c. &c. &c.



*Professor Silliman to Mr. Bull.*

YALE COLLEGE, July 17, 1826.

DEAR SIR—I have twice perused with attention your communication of the 6th instant, covering the report of the Committee of the American Academy of Boston, upon the subject of your experiments upon the heat evolved in combustion, &c.

In reply to your request, that I would give you my opinion of the objections made by the committee, and of your reply to them,\* I proceed to remark :

1st. I conceive that the exterior room, being sustained at a given temperature by a source independent both of the inner-room and of the external air, is as good a non-conductor as can be provided, and that the inner room is as effectually guarded as possible from any influence from the external air, and that it is sufficiently guarded to prevent any appreciable inaccuracy from that source.

2d. There being no *visible* smoke from the anthracite coals, and scarcely any volatile combustibile matter, that is not immediately consumed by the fire, there is, in the case of this fuel, no room for the combustion of the smoke; and as the object of the experiments was to show the comparative quantity of heat evolved in the *usual* modes of burning fuel, in domestic economy and in the common arts, and not the whole possible amount, it did not come within your plan to compass this object, nor does it appear to be necessary for the purpose in view.

3d. The spirit of these remarks is applicable to the third objection: your selection of fuel appears to have been sufficiently precise to furnish the *average* result of the good fuel in market, and this was all that the case required.

For my general opinions of the value of your paper, I beg leave to refer you to the American Journal, vol. xi. page 98, just published, where, under the date of May 11th, you will find my impressions concisely but fully expressed.

Entertaining the greatest respect for the Committee of the American Academy, and having myself the honour to be a member of that body, I trust they will receive with candour the opinions which I have expressed, and which would have been communicated with equal frankness had I been so fortunate as to coincide with them.

I remain, dear sir, yours very respectfully,

B. SILLIMAN.

MR. BULL,

\* Letter No. III.



## No. VI.

*Dr. Bigelow to Mr. Bull.*

BOSTON, August 20, 1826.

DEAR SIR—The correction contained in your last letter does not appear to throw any additional light on your subject, since it is founded on a fresh mistake, that of supposing surfaces to partake the temperature of the contiguous atmosphere, more than that of the solids to which they belong. The two surfaces of your outer room cannot be of the same temperature, for the same reason that the surface of a stove containing a fire, is not of the same temperature as the surface of the apartment in which it is placed.

The committee do not find in your letter any further reasons, from any source, requiring answers other than those already put into your possession.

In regard to the opinions which you adduce of Professors Silliman and Hare, and also the statement of the mode in which Count Rumford employed fuel; the committee consider these as entitled to no further weight, than that of meriting a respectful consideration. The business of the committee is with the merits of the question, and not with the authority of names.

The committee having now given what they consider a patient and ample hearing to your claims, feel themselves called on to report; that your experiments do not contain any discovery or improvement sufficiently important to entitle them to the Rumford Premium.

Very respectfully, your obedient servant,

JACOB BIGELOW,  
*For the Committee.*

MARCUS BULL, Esq.

## No. VII.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, August 24, 1826.

DEAR SIR—Your letter of the 20th inst. was received this morning.

The "fresh mistake" with which the committee think proper to charge me, appears to rest with themselves, as I apprehend it would be difficult for them to prove that two surfaces of solid bodies, (bad conductors, such as my walls, exposed to different temperatures,) *do not* "partake the temperature of the contiguous atmosphere, more than the solids to which they belong." The admission of such a state of things by the committee, must prove fatal to their first objection.

If the cold air on the exterior surface of the wall, did not possess more influence to lower its temperature, than the interior strata possess by their reaction to maintain it at *their* temperature, I would ask

the committee in this case, how the wall when once heated, could ever become cool? and when once cooled, could ever be heated by the action of the contiguous air?

The committee will permit me to observe, that I was not prepared to expect at this stage of the business, when the principal objection urged by them is reduced to a point capable of being proved or disproved by actual experiment, which I had determined to institute,—that they should think proper to foreclose any further hearing from me on the subject, and particularly as you state to me in your letter of the 5th inst. that “The committee have no further wish in this business, except that you and your scientific friends, in common with themselves, may arrive at a joint understanding of the *truth*. They are in *no haste* to make up their report, but will patiently wait for any further remarks you may choose to offer on the subject.”

I have respectfully to request, that the committee will delay their report upon my application for the Rumford Premium, until an opportunity shall be given me to institute the experiments necessary to determine the point in question between us.

I remain, sir, very respectfully, your obedient servant,

MARCUS BULL.

TO JACOB BIGELOW, M.D. &c. &c. &c.

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## No. VIII.

*Dr. Bigelow to Mr. Bull.*

BOSTON, September 2, 1826.

DEAR SIR—The passage in my last letter reads thus, in the copy which I preserved of it: “a fresh mistake, that of supposing surfaces to partake the temperature of the contiguous atmosphere, more than *that of* the solids to which they belong.” In your quotation of this passage, I find the words “*that of*” are omitted, and thus a foreign meaning given to the sentence. Without these words, the sentence is irrelevant to the question; with them, it agrees in connexion with what follows.

If the argument in your last letter is founded on this mistake, it is only necessary to refer you to the *true* meaning of the committee; if otherwise, it is difficult to comprehend its application to the case.

The committee remain of their former opinion, that the opposite surfaces of your exterior room cannot be of the same temperature. If they were of the same temperature, they could reciprocally neither give nor receive heat, and the question would remain for yourself to answer, how the inner room “when once heated, could ever become cool?” &c.

The time of the August meeting having passed, the committee can-

not present their report to the Academy till the next meeting, which is in November.

Respectfully, your obedient servant,

JACOB BIGELOW.

MARCUS BULL, Esq.

No. IX.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, September 7, 1826.

DEAR SIR—I have the pleasure to acknowledge the receipt of your letter of the 2d inst. and am glad to find that the committee have deferred their report until November.

The omission to which you allude, in the passage quoted in my last letter, from yours of the 20th ult. was entirely accidental, and a departure, as I find, from the first sketch of my letter.

The words "*that of*," which were omitted by me, appear clearly to refer as *relatives* to the antecedent word *temperature*; and if so, I do not perceive that their *omission* could occasion any different, or "foreign meaning" to be applied to the plain and obvious import of the sentence in your letter; which I presume would accurately convey the sense of the committee, if it were stated thus, viz. That I had committed "a fresh mistake, that of supposing surfaces to partake the temperature of the contiguous atmosphere, more than" (the temperature of) "the solids to which they belong."

Is it not plain from grammatical construction, that the reference to the word *temperature*, fully supplies the omission of the words "*that of*," without in any manner altering the "true meaning of the committee?" In other words, is it not plain, that the words "*that of*," or "*more than*," have a direct and exclusive reference to the word *temperature* in the passage of your letter, and can refer to no other word or words in it?

If the language made use of in your letter of the 20th ult. does not convey the "true meaning of the committee," it is my wish that it may be corrected; as it has ever been my desire, to give your letters the most fair and liberal construction which their language will admit.

You say, "The committee remain of their former opinion, that the opposite surfaces of your exterior room cannot be of the same temperature."

I am not aware that I have *ever contended* that the opposite surfaces of my exterior room *were* of the "same temperature;" but on the contrary, have always considered that they must of necessity be of *different* temperatures.

My experiments were based upon a constant *loss* of heat, which



could only take place in consequence of the existence of different temperatures, and this is clearly stated by me as I conceive, at page 17 of my paper, to which I beg leave to refer the committee.

I remain, sir, very respectfully, your obedient servant,  
**MARCUS BULL.**

To JACOB BIGELOW, M. D. &c. &c. &c.

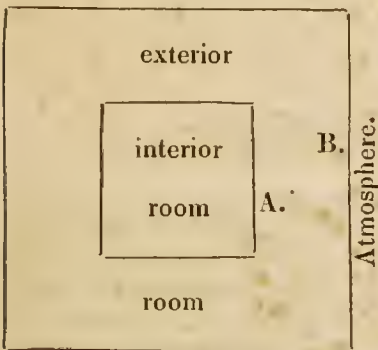
No. X.

*Dr. Bigelow to Mr. Bull.*

Boston, September 11, 1826.

DEAR SIR—It has been my aim to express the meaning of the committee intelligibly, but as it seems I have not been fortunate in this respect, I will endeavour to state it in a form which cannot be mistaken.

By the word *room* the committee mean *cavity*, and by *opposite surfaces of the room* they mean the *surfaces of the cavity* which are in *opposition to each other*. Or, to make the matter more plain, they mean the surfaces marked A and B, in the annexed diagram.



Now, the committee assert, that during your experiment the surface B is colder than the surface A, and that this difference of temperature will be in some measure proportionate to the difference between the atmosphere and interior room. And therefore, if, during your experiment, the atmosphere should fall in temperature; then, more heat will be radiated from A to B, and of course more heat will escape from the interior room to the atmosphere,

than if no reduction of the atmospheric temperature had taken place. And an experiment performed under such circumstances would give a different result, from one performed when the atmosphere was stationary from the beginning.

Very respectfully yours,

**JACOB BIGELOW.**

P. S. I am reluctant to add any thing more, which may lead you to digress from the main point; but since you say, "I am not aware that I have ever contended that the opposite surfaces of my exterior room were of the same temperature," let me cite some different passages from your letter of August 11th. You there say, "I should have said after the word *through*, a surface *uniformly heated* by a stratum of intervening air, warmer than the cold bodies." Again: "The

committee will probably agree with me, that the quantity of radiated heat lost by a heated body in a given time, will be uniform, provided the temperature of its *surface* and the *surface* of the recipient or colder body in *opposition*, remain the same," &c. Again: "Now supposing the *internal surface* of my *exterior room* to be maintained at a *uniform* temperature, I contend," &c. Again: "From the bad-conducting materials of which the room is composed, I am led to believe that no appreciable difference will be found to exist in the temperature of its *internal surface*," &c.

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No. XI.

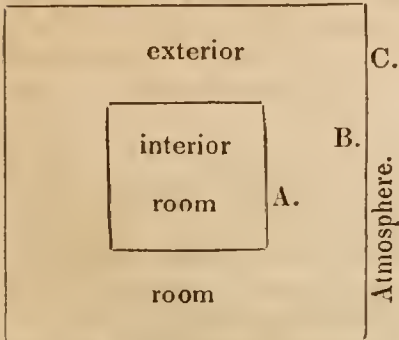
*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, September 16, 1826.

DEAR SIR—I received your favour of the 11th inst. yesterday, and will adopt with pleasure what you now state to be the meaning of the committee, however much it may be at variance with the obvious import of the language made use of in your letters of the 20th ult. and 2d inst.

I regret that I cannot perceive on the part of the committee, that spirit of candour towards me, which I have a right to expect from them in their present official capacity of *judges*; as its perception would prevent the necessity of commenting upon any part of your letter, except the explanation contained therein.

In your letter of the 2d inst. you say, "The committee remain of their former opinion, that the opposite surfaces of your exterior room cannot be of the same temperature." In your last you say, "By the word *room* the committee mean *cavity*, and by *opposite surfaces* of the *room* they mean the *surfaces* of the *cavity* which are in *opposition* to each other. Or, to make the matter more plain, they mean the surfaces marked A and B in the annexed diagram."



Without the aid of a diagram, your meaning would have been perfectly intelligible had you said, "The committee remain of their former opinion that the *exterior surface* of your *interior room*, and the *interior surface* of the *exterior room*, cannot be of the same temperature."

It is well known by the committee, that these rooms have always been considered, not only by me, but by themselves as shown in your diagram, as *entirely distinct*.

By the "opposite surfaces of the room," (for example, the exterior room,) is clearly to be understood, the two surfaces of *its own walls B and C*, and cannot by the aid of any diagram be confounded, or made to mean or include as *one* of the surfaces, the *exterior* surface of *another*, viz. the interior room. We should not say that the *surface* of a large *box*, standing in a room, is one of the "surfaces of the room;" and such a box may, for explanation, be considered to represent my interior room.

In reply to what I consider your very *unfortunate* P. S., I must repeat, that 'I am not aware that I have ever contended that the opposite surfaces of my exterior room were of the same temperature,' and I do not perceive that any proof can be drawn from the passages cited by you from my letter of August 11th to disprove it.

The words in the first passage cited, "a surface *uniformly* heated," cannot mean *two* or "opposite surfaces."

In the second passage I was speaking of the temperature of the surfaces of a *hot* and *cold* body, and the words "remain the same," can only refer to the same *difference* of temperature, it being impossible for me to conceive, how the surfaces of two bodies, the one *hot* and the other *cold*, should still be of the *same temperature*.

The two last passages refer to maintaining the "internal surface of my exterior room" at a *uniform temperature*, but do not include its *external surface* also, which would be necessary to sustain your charge.

Having noticed those parts of your letter which have compelled me "to digress from the main point," I will now notice the latter.

The objection of the committee may be stated intelligibly in very few words, viz. *That the surface B cannot be maintained at a uniform temperature, if, during an experiment, the temperature of the atmosphere should fall.*

The fact whether "the surface B is colder than the surface A," during an experiment, is *entirely immaterial*, provided they remain at the "same *difference* of temperature."

I beg leave to refer the committee to those parts of my former letters which are intended to prove that the surface B *may be maintained at a uniform temperature*, and I have particularly to request that they will give an *attentive perusal* to that of August 11th, in doing which I am persuaded they will withdraw their charge of its being "founded on a fresh mistake."

If the committee are still of opinion that the surfaces of my walls B and C, *do not* "partake the temperature of the contiguous atmosphere more than that of the solids to which they belong," the external surface, which I have marked C on the diagram, will not be affected by the *changes* they have supposed in the temperature of the atmosphere in contact with C; consequently the temperature of B cannot be affected thereby, which would be fatal to their objection.

If the committee take the opposite ground, and say, that the surfaces of my walls B and C, *do* "partake the temperature of the contiguous atmosphere more than that of the solids to which they belong," this position appears to be *equally fatal* to their objection; as



in admitting that the surface C partakes the temperature of the cold atmosphere in contact with C, they must also admit that the surface B must also partake the temperature of the warm air in contact with B, and as the warm air in contact with the latter is of a *uniform* temperature, consequently the temperature of B must remain *stationary*, so that taking the matter *either way*, the committee must perceive that their objection cannot be maintained.

I remain, sir, very respectfully, your obedient servant,

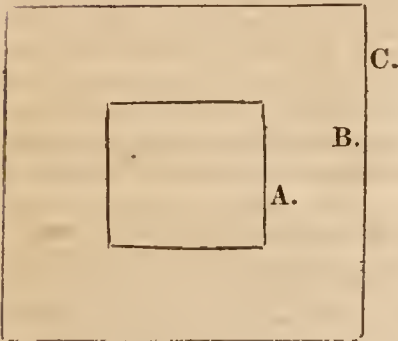
MARCUS BULL.

TO JACOB BIGELOW, M. D. &c. &c. &c.

## No. XII.

*Dr. Bigelow to Mr. Bull.*

Boston, September 22, 1826.



DEAR SIR—In your letter of the 16th, you say, “The fact whether the surface B is colder than the surface A during an experiment, is entirely immaterial, provided they remain at the same difference of temperature.”

As this statement bears more upon the question than any other in your letter, I beg leave to state, that during changes of the atmosphere, in the opinion of the committee, these surfaces will *not* remain even of the same *difference* of temperature. Suppose that, in an experiment, the difference of temperature between A and B is two degrees. Then let the atmosphere fall twenty degrees, and the difference between A and B will become *more than* two degrees, and will so continue until the interior room and the atmosphere shall arrive at their former relative temperature. But during all this time (as we formerly stated) more heat will be radiated from A to B, and of course more heat will escape from the interior room to the atmosphere, than if no reduction of the atmospheric temperature had taken place. Therefore, the result of the experiment will differ from what it would have been, had the atmosphere remained stationary.

The committee also “are still of opinion” that your surfaces “do *not* partake the temperature of the contiguous atmosphere more than that of the solids to which they belong.” For example, the surface C will never be reduced to the temperature of the atmosphere, so long

as the wall, to which it belongs, is warmer than the atmosphere. But it will nevertheless "be affected by changes in the temperature of the atmosphere," so as to expend or give off more heat when the atmosphere is cold, than when it is warm. And consequently more heat will escape from the interior room to the atmosphere, than if (in the case already supposed) no reduction of the atmospheric temperature had taken place.

Your obedient servant,

J. BIGELOW.

MARCUS BULL, Esq.

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No. XIII.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, September 27, 1826.

DEAR SIR—I received your favour of the 22d inst. yesterday. The objection of the committee appears now to be, that the surfaces A and B will not remain of the same difference of temperature, if, during an experiment, the temperature of the atmosphere should fall twenty degrees; this, however, I will remark, is about double the greatest depression ever experienced during any experiment, the result of which is given in my Table.

As no objection will probably be made to the practicability of producing a uniformity in the temperature of the surface A, it appears only necessary to show, that a uniformity of temperature may be maintained upon the surface B, during a change equal to the greatest which ever occurred during my experiments.

You say, "Suppose that, in an experiment, the difference of temperature between A and B is two degrees. Then let the atmosphere fall twenty degrees, and the difference between A and B will become more than two degrees."

The changes in the temperature of C are admitted, but these cannot influence A except by first lowering the temperature of B, which is the barrier against these changes, and the inference which you have drawn, can only be shown to be true, by proving the impossibility of maintaining the surface B at any required temperature, through the agency of heated air. If the temperature of B is 80°, I conceive that the same heat would be radiated from A to B in a given time, whether the temperature of C is 70° or 40°.

The committee do not I presume intend to suppose an *instantaneous* change of twenty degrees, but even admitting this for the sake of argument; do they suppose that this change operating upon the surface C would also produce a similar instantaneous change upon the surface B, or even such as to prevent the possibility of counteracting it by increasing the fire in the stove of the exterior room?

We do not find it impossible to maintain the temperature of our dwellings at  $60^{\circ}$  or  $70^{\circ}$ , when the temperature of the atmosphere is even at zero; and it is well known, that a considerable period of time is required, for a change of twenty degrees to produce any material effect upon the temperature of rooms in brick buildings, even where no fires are kept up to counteract it.

To give an instance of the practicability of maintaining two surfaces of a solid body at very different temperatures, I have only to state, that my Air Furnace (which is built in a cellar) has walls ten inches thick, and although the interior surface is sometimes near a *white heat* for a number of hours together, I do not recollect an instance in which I could not keep my hand without inconvenience upon the exterior surface of the wall, opposite to the furnace.

The temperature of the atmosphere has, with us, for a few days past, been unusually cold for the season, and was last evening at  $55^{\circ}$ . During the night it changed, and this morning, at 9 h. 30 m. it was  $76^{\circ}$ : a change so remarkable, induced me to make the following simple experiment, to ascertain whether the surface B would possess the same temperature as the air of the room, during the process of warming it from the atmosphere.

To perform this experiment, I made use of three mercurial thermometers, accurately corresponding with each other, one of which was suspended in its case from the interior wall of the exterior room, another was removed from its case, and the bulb placed against the surface B, the back side or half of this bulb being exposed to the influence of the wall, whilst the former was screened from its direct influence by the case, which did not touch the wall. These two thermometers were placed near each other, and at the same height on the wall. The third was suspended from the exterior wall, in the atmosphere, in the shade.

The following results were obtained.

Time.	Air.	Surface B.	Atmosphere.	Surface C.
9 h. 30 m.	$69^{\circ}$	$69^{\circ}$	$76^{\circ}$	
1 30	$71^{\circ}$	$71^{\circ}$	$78^{\circ}$	
3 30	$72^{\circ}$	$72^{\circ}$	$80^{\circ}$	
4 30	$72^{\circ}.5$	$72^{\circ}.5$	$78^{\circ}$	$78^{\circ}$

It did not occur to me to try the surface C until the last period of time noted, although the sun had been obscured nearly the whole time.

The walls on the two sides of the room exposed to the atmosphere are ten inches thick. At 4 h. 30 m. the centre, or mean temperature of the wall, may be supposed to have been  $75^{\circ}.25$ , as the surface B was  $72^{\circ}.5$ , and C  $78^{\circ}$ . It is worthy of remark, that it required six hours to elevate the temperature of the room  $3^{\circ}$ , the atmosphere having been not less than  $7^{\circ}$  warmer during the whole time.

You will not probably have an opportunity to repeat the experiment under the same circumstances, but you will undoubtedly very soon have the atmosphere so *cold* as to be able to make equally satisfactory



experiments, in which case, the committee will be able to decide, whether their opinion contained in the last paragraph of your letter is correct.

If the committee are of opinion that the surface B cannot be maintained at a uniform temperature, by the same aids made use of in performing my experiments, I have to request, that they will suggest what they would consider as satisfactory experiments to determine this point, which, if practicable, I will perform in the presence of any gentlemen they may name.

Dr. Hare suggested to me, that the American Philosophical Society would, at your request, appoint a committee for this purpose.

Yours, very respectfully,

MARCUS BULL.

TO JACOB BIGELOW, M. D. &c. &c. &c.

No. XIV.

*Dr. Bigelow to Mr. Bull.*

BOSTON, October 5, 1826.

DEAR SIR—You will please to observe, that the objections of the committee are not limited by the extent of an example. Examples are illustrations of *general principles*. What is said in my last of twenty degrees, is true of any supposable number of degrees.

The committee have too much respect for the discernment of yourself, and of your friend Professor Hare, to suppose, that after *maturely* considering what has been said in my late letters, you can really believe that the surfaces A and B can be kept, during atmospheric changes, “at the same temperature,” or at “a uniform difference of temperature.” The reasoning in your last letter is fallacious. Do you not perceive, that in order to sustain the surface B at, or near, a given temperature, while the atmosphere *falls*, you must *raise* the heat of the air in your exterior room, and that in so doing you will raise the heat of A, and thus produce the same relative difficulty, which you are seeking to avoid?

The Committee have no experiments to suggest, as they really know of none which would remove your difficulties.

Very respectfully yours,

J. BIGELOW.

MARCUS BULL, Esq.

## No. XV.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, October 10, 1826.

DEAR SIR—To exculpate Professor Hare from any suspicion of asserting opinions without mature consideration, or those which he does not “really believe,” you will permit me to remark, that he has not seen any of your “late letters,” or my replies thereto; having been seriously ill for some weeks. The suggestion of his, contained in my last, was made to me some time since.

My answer to your query, “Do you not perceive,” &c. is in the negative: as in the case supposed, no necessity exists that I should “raise the heat of the air in the exterior room:” all that is required is, that it should not be permitted to *fall below* the temperature at which the experiment was commenced. If, for example, an experiment was commenced with the interior room at 80° and the exterior at 70°, they were maintained at *these temperatures* throughout, or the experiment was considered a failure.

But even admitting for argument that your assertion is true, that “you must *raise* the heat of the air in your exterior room, and that in so doing you will raise the heat of A”—you must perceive, that as the *same air* acts also upon B, their “*relative*” difference of temperature would not be affected thereby.

There is evidently a misconception on the part of the committee relating to my experiments, which I fear I shall not be able to remove, except by a personal interview.

Yours, very respectfully,

MARCUS BULL.

TO JACOB BIGELOW, M. D. &amp;c. &amp;c. &amp;c.

## No. XVI.

*Dr. Bigelow to Mr. Bull.*

BOSTON, October 14, 1826.

DEAR SIR—In reply to the statements in your letter of the 10th, beginning “If for example,” &c. I will state a plain case, which may serve for a general answer.

Suppose the air of your exterior room at 70°, and the atmosphere 60°, and the surface B at a given temperature. It is required to preserve B at the same (i. e. a uniform) temperature. It is obvious, that B is held in equilibrium, between the *warming* influence of the heat generated within, and the *cooling* influence of the atmosphere without; for although the wall is a partial non-conductor, it can only retard, not prevent, the establishment of this equilibrium. Now, so long as the

warming influence may continue at  $70^{\circ}$ , and the cooling influence at  $60^{\circ}$ , B will continue at a uniform temperature. But let the cooling influence change from  $60^{\circ}$  to  $50^{\circ}$ , then the temperature of B will *certainly fall*, unless the warming influence is *raised* proportionally above  $70^{\circ}$ , so as to reproduce the former equilibrium.

Respectfully yours,

J. BIGELOW.

MARCUS BULL, Esq.

No. XVII.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, October 17, 1826.

DEAR SIR—Your letter of the 14th inst. is received. It gives me pleasure to find that you have at length stated the objection of the committee in an intelligible form, and in perfect agreement with what I had conjectured it to be, as stated in a number of my former letters, viz. That the surface B cannot be maintained at a uniform temperature, during the ordinary changes in the temperature of the atmosphere.

In addition to what I have already stated against the validity of this objection, I remark, that as the bulb of the thermometer suspended in the exterior room, was exposed to the direct influence of B, it would indicate the joint temperature of that surface, and the air of the room; so that A could never be materially affected by the changes of B, without a corresponding and *visible* effect being indicated by this thermometer. Now, as evidence that B did not experience the changes you have supposed, I state, that the bulb of the *differential* thermometer in the exterior room was *screened* from the direct influence of B, and was only affected by the air of the room, but I found it always to agree with the mercurial thermometer of that room, at fixed points on their scales, which would not have been the case, had not B been permanent in its temperature.

The objection of the committee is of a *practical* nature, and capable of being proved or disproved by experiment. To this *test* I have requested, in my letter of the 27th ult. that it may be submitted, in any practicable manner they may suggest, and which I would perform in the presence of any gentlemen they may name. This they have declined.—If the objection possesses any *practical* weight, it can be discovered; but if merely *theoretically* true, it will not affect the practical accuracy, or *utility* of my results.

I remain, very respectfully, your obedient servant,

MARCUS BULL.

TO JACOB BIGELOW, M. D., &c. &c. &c.



## No. XVIII.

*Dr. Bigelow to Mr. Bull.*

BOSTON, October 21, 1826.

DEAR SIR—The committee will thank you to state DEFINITELY whether, in your opinion, their objections are, or are not, "*theoretically true.*"

In other words, whether you believe that, theoretically speaking, the surfaces A and B can be maintained, during an experiment, at the same temperature, or at a uniform difference of temperature; if, in the mean time, atmospheric changes of temperature take place.

A DIRECT answer will oblige your obedient servant,

JACOB BIGELOW.

MARCUS BULL, Esq.

## No. XIX.

*Mr. Bull to Dr. Bigelow.*

PHILADELPHIA, October 30, 1826.

DEAR SIR—Your favour of the 21st inst. was duly received. The committee ask me "to state DEFINITELY whether, in my opinion, their objections are, or are not, '*theoretically true.*'"

I would reply, that let my answer be either in the *affirmative* or *negative*, I am unable to perceive, what possible bearing it could have, of a profitable or useful character, in relation to the subject matter of our investigation, this being *exclusively practical*; and I have in no case thought it either proper or necessary, to travel into any inquiries relating to the *abstract* or *theoretical* nature of the subject.

Suppose, for example, that I were to *admit* that the objections of the committee *were theoretically true*, but at the same time demonstrate to their satisfaction, or that of any competent judges, that this admission did not in any assignable way affect the *practical* results. The committee must in this case, as candid judges, wave their objections.

Suppose, on the other hand, I were to *deny* the *theoretical truth* of their objections: these they would endeavour to maintain, and we will suppose with success: but what would be the result? why truly, that there was some minute defect in the *theory* on which my experiments were founded, but which did not in any assignable or tangible degree, affect or invalidate the substantial practical results.

What then would be gained by the committee, but a *dilemma* in either case.

In relation to this subject, the committee will not perhaps disagree with me in opinion, that it is in the nature of *all theories* to fall short of a *perfect* application in *practice*.

No two thermometers perhaps have ever been made to agree exactly through all the degrees of their scales; nor was there probably a *LINE* ever drawn (in practice I mean) that was absolutely or *theoretically straight*.

What then would become of all the arts, if an absolute conformity to *theory* was required as indispensable to them.

I remain, sir, very respectfully, your obedient servant,

MARCUS BULL.

To JACOB BIGELOW, M. D., &c. &c. &c.

No. XX.

*Dr. Bigelow to Mr. Bull.*

BOSTON, November 2, 1826.

DEAR SIR—Your letter of October 30th, is this day received. After a delay of nearly six months, the committee, finding no reason to change the opinions expressed to you in June last, will present the report which was then contemplated, at the meeting of the Academy on Wednesday next.

Respectfully, your obedient servant,

JACOB BIGELOW,

MARCUS BULL, Esq.

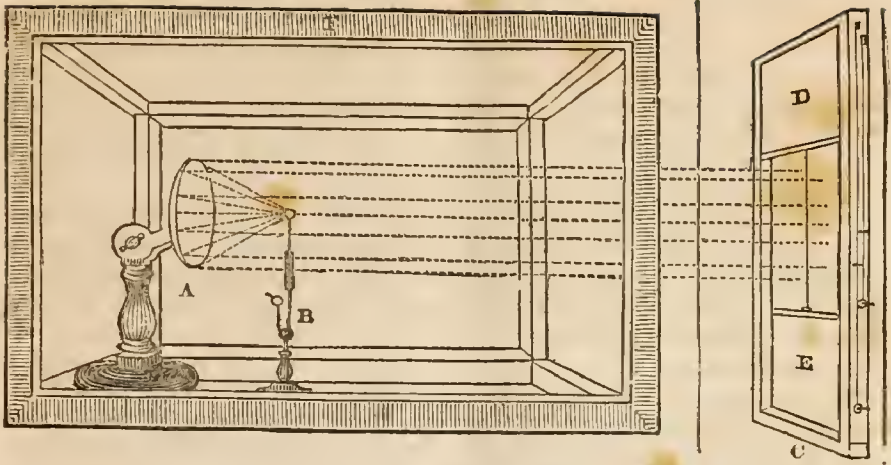
*For the Committee.*

In the commencement of the foregoing correspondence, (Letter No. II. page 6,) the committee state, "That the subject does not admit of the philosophic accuracy in its conclusions, which you appear to attach to it," and yet strange as it may appear, they condemn me in the sequel for not having effected what they themselves admit to be an impossibility.

The want of candour which the committee appeared to me to evince throughout the whole of the correspondence, was not well calculated to inspire me with much confidence in the soundness of the objections which they urged, as tending to invalidate the correctness of the deductions which I had drawn from my experiments, especially as these objections were in direct opposition to the opinions expressed by some of the best chemists of our country.

My experiments being entirely of a practical character, the validity of the only objection worthy of notice urged against their accuracy, was capable of proof by actual experiment; and had the committee been governed by the motives they professed, viz. a sincere desire to "arrive at a joint understanding of the truth,"\* they would not have rejected the proposition contained in my letter No. XIII. to submit their objection to the test of experiment. To this test, however, I determined to submit it, and accordingly during the succeeding winter (1827) arrangements were made, with the kind assistance of Professor Hare, of the University of Pennsylvania, for the performance of the following experiments; the utmost care being used to construct the apparatus in such a manner as to give to the objection of the committee its greatest weight.

\* Letter No. IV. page 11.



The following is a concise description of the apparatus made use of, together with the results of a course of experiments instituted to determine the question,—*Whether the interior surface of the walls of my exterior room, can be maintained at the same temperature as the air in contact with it, or at a uniform difference of temperature, during the ordinary changes in the temperature of the atmosphere?*

The wood cut represents a perspective view of the apparatus, which consists of a parabolic mirror of brass, A, 12.5 inches diameter, a differential thermometer, B, together with a frame, C, placed against the wall, in which frame two screens, D and E, slide in grooves at different distances from the wall, and are moved by cords attached to them passing over pullies in the top of the frame, and thence down the side of the latter. One of the screens, D, is of wood, the front surface of which is one inch from the wall; the other, E, consists of a plate of looking-glass, the silvered side being turned towards the wall, to prevent any immediate influence from the radiation therefrom, in consequence of its proximity, being placed within one-eighth of an inch of it.

To produce uniformity in the radiating power of the surfaces of the wall, and screens, the whole are covered with fine white drawing paper. White paper was selected, in consequence of its superior radiating power, being, as determined by Mr. Leslie, as 98 to 100, when compared with a surface of lamp-black.

The parabolic mirror is placed two feet from the wall, and parallel with it. In the focus of the mirror is placed one bulb of the differential thermometer, the other bulb being at a considerable distance below the influence of the mirror. To the stem of the former bulb the scale is attached.

The mirror and thermometer are covered by a glass ease, F, to prevent as much as possible any influence from currents of air, and from the face and hand of the operator in examining the scale of the thermometer, and moving the screens. The glass case is open at one end, opposite the portion of wall experimented upon.



The effect of the wall on the scale of the thermometer being observed, and one of the screens being subsequently interposed between the wall and the mirror, the difference of temperature between the surface of the *wall* and the *air* would be obtained, either at one inch from the wall, or at one-eighth of an inch, according to the screen interposed. The screens being surrounded by the air upon all sides, they must necessarily be supposed to acquire, and to radiate heat of the same temperature as the air with which they are in contact.

The wood cut is intended to represent the situation of the apparatus when obtaining the temperature of the surface of the wall, but in experimenting, it was my practice to obtain the temperature of the air or screen, previous to that of the wall. This plan was adopted to obviate a temporary source of error, which would otherwise have occurred, in consequence of the screens being constructed, for greater convenience, to move vertically rather than horizontally. The temperature of the strata of air varying at different heights within the room, the screens would acquire the temperature of the height at which they should be suspended, and if at the top of the frame they would be warmer, or at the bottom colder, than the stratum of air opposite to the portion of the wall experimented upon, so that if presented to the mirror from either of the former situations, they would not, immediately, indicate the proper temperature.

The experiments have been performed at various times during the past winter, when the temperature of the air in the exterior room had been elevated  $10^{\circ}$ ,  $20^{\circ}$ , and  $30^{\circ}$ , above that of the external atmosphere, and always with similar results at these different temperatures.

Using the glass screen, when the temperature of the air in the room was  $30^{\circ}$  above that of the atmosphere, (this being about double the difference experienced during any of my experiments on fuel,) the *greatest* difference found to be produced on the most delicate alcoholic *flat* bored differential thermometer which I could procure, the sensibility of which, when compared with Fahrenheit's, being as 120 to 1, was found to be  $6^{\circ}$ , from which deduct  $2^{\circ}$  for the oscillations of the instrument, (found by observation,) leaves an effect of  $4^{\circ}$ , or  $\frac{1}{30}$ th of a degree of Fahrenheit's scale, the air being, at this distance, that much warmer than the surface of the wall. The greatest effect produced by the wood screen (one inch from the wall) was  $16^{\circ}$ , under the same circumstances; so that, as the strata of air *increase* in temperature as we recede, with the screen, from the wall, if it were possible to interpose a screen possessing merely *surface*, within an infinitely small distance of the wall, no perceptible difference would probably be found to exist between the temperature of the surface of the wall and that of the air in contact with it. When it is recollected that this maximum effect (equal only to  $\frac{1}{30}$ th of a degree Fahrenheit) was produced by a surface of 122.71 inches, (the content of the mirror, 12.5 inches diameter,) concentrated upon a bulb of  $\frac{1}{2}$  inches in diameter, no difficulty will be found in believing what I state as a fact, that *no effect whatever* could be observed on the Fahrenheit's thermometers used in my course of experiments on Fuel, when placed in the focus of the mirror.

From these experiments it may be seen, that the objection of the committee cannot be sustained by any effect observable on the ordinary instruments made use of to measure degrees of heat, although the difference of changes in the temperature of the atmosphere should be *double* those ordinarily experienced, and the effect of any given extent of surface be magnified more than *a hundred fold*; and unless the committee can show, that the ordinary mercurial thermometers are not sufficiently accurate for the purposes to which they are daily applied, their objection must fail of being substantiated, and may be safely rejected as futile, and of no possible practical importance.

These experiments were performed in the presence of a number of scientific gentlemen, who will if necessary corroborate the statement of the results obtained.

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The preceding experiments were performed in the winter of 1826-7, and with a view to solicit from the Academy a reconsideration of my claim for the Rumford premium, for the purpose of giving me an opportunity to exculpate my former labours from the charge of inaccuracy, made by their committee; I visited Boston in May, 1827, prepared to exhibit a drawing of the apparatus made use of, together with a written description of these experiments, and their results.

At my interviews with the gentlemen composing this committee, it was stated to me that no objections would be made by them at the approaching meeting of the Academy, to granting the reconsideration about to be requested; but on the contrary that they would advocate such a request, and if granted, they had no doubt that a *fresh* committee would be appointed, who would have an opportunity of reviewing their labours; this course appearing also to them to be the only proper one to give me a *fair* hearing.

The following is a copy of my Memorial presented to the Academy.

BOSTON, MAY 28th, 1827.

*To the Hon. James Savage, Recording Secretary of the American Academy of Arts and Sciences.*

SIR—I have to request that you will do me the favour to lay before the Academy the following statement.

At the meeting of the Academy in May, 1826, an application was made on my behalf, for the Rumford premium, in consequence of a long course of experiments having been made by me, to determine the comparative quantities of heat evolved in the combustion of the principal varieties of wood and coal, used in the United States for fuel; and also to determine the comparative quantities of heat lost by the ordinary apparatus made use of for their combustion.

The apparatus employed in my experiments on fuel, was of a new construction, and has been considered by many scientific gentlemen both in this country and in Europe, as capable of insuring the most satisfactory results hitherto obtained on this intricate and highly important subject.

In consequence of certain objections made by the committee of the Academy to whom my claim was referred, tending to question the ac-

curacy of the apparatus employed by me, a correspondence took place between the committee and myself in relation to the validity of their objections. Failing to convince the committee by argument, that their principal objection was not well founded, I proposed to them to submit this question to the test of experiment, which they thought proper to decline; and in November last, an unfavourable report was made on my claim.

Since the report of the committee was made, their principal objection has been subjected to the most rigid and delicate test of experiment; and I have respectfully to request, that the Academy will be pleased to reconsider my claim for the Rumford premium, that an opportunity may be given me, to lay before them the results of these experiments, in any manner they shall be pleased to direct.

I have the honour to be, sir,

With great respect,

Your obedient servant,

MARCUS BULL.

After the meeting of the Academy, I received a note from Dr. Bigelow, of which the following is a copy.

Tuesday 29th May, 1827.

DEAR SIR—Your Memorial to the Academy is referred to the former committee, who will be ready to meet you at my house this evening at 8 o'clock.

Yours,

J. BIGELOW.

MARCUS BULL, Esq.

It is impossible for me to describe in words what were my feelings on learning that my reasonable expectations as to the appointment of a *fresh* committee had been disappointed; but they may be compared to those of a historical painter, whose keen sensibility should be roused by being told that he must submit his work, and reputation as an artist, to the decision of those who had previously, and in his opinion unjustly, decided against him.

During my conference with the committee at the house of Dr. Bigelow, I was informed by them that they did not consider the experiments instituted by me for the purpose of determining the validity of their objection, as in *any way bearing on the question!* and one of the gentlemen stated to me, that although *no* effect whatever could be observed on the Fahrenheit's thermometers when placed in the focus of the mirror, as stated by me; and supposing the difference of temperature between the surfaces A and B to vary  $1^{\circ}$ , yet he believed this source of error to be so great, that 25 per cent. more fuel would be consumed at one time than another, in performing any given experiment!

In proof of this opinion being well founded, he stated, that a mercurial thermometer when exposed to the radiation of heat from a sheet iron stove, as ordinarily heated, and within three or four feet of it, would be affected from five to ten degrees.



Now, suppose we fix the temperature of the stove at the low heat of  $300^{\circ}$ , and the effect of radiation as  $10^{\circ}$  on the scale of the thermometer. If we suppose the temperature of the stove to be reduced  $1^{\circ}$  or to  $299^{\circ}$ , the effect of radiation by arithmetical calculation would then be  $9.97^{\circ}$  or  $\frac{3}{100}$  of a degree less, a difference in effect which would not be observable on the scale of any mercurial thermometer, and the difference in the amount of fuel which would be required to maintain the stove at  $300^{\circ}$  or  $299^{\circ}$ , to supply the loss of heat by radiation only, and for a period of time equal to that occupied by any of my experiments, may be supposed to be equally trifling.

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The foregoing has been for some time prepared for publication, but this has been delayed for the purpose of obtaining from the Academy a copy of the Report of their committee, which they have at my request just furnished, and it is with great pleasure that I am enabled to give them an opportunity of speaking for themselves.

*To the American Academy of Arts and Sciences.*

THE committee to whom was referred the application of Mr. Marcus Bull for the Rumford premium, beg leave to report.

That Mr. Bull has laid claim to the premium on the ground of certain experiments performed with an apparatus suggested to him by Dr. Hare "to determine the comparative quantities of heat evolved in the combustion of the principal varieties of wood and coal used in the United States for fuel; and also to determine the comparative quantities of heat lost by the ordinary apparatus made use of for their combustion." The apparatus consists of two rooms, one constructed within the other; a stove being placed in the inner room to contain the combustible, of which the heating power is intended to be measured; also a stove in the outer room, to aid in sustaining an artificial temperature during the experiments.

Mr. Bull objects to the experiments of his predecessors, on subjects of this nature; on the ground of their "inaccuracy;" and quotes the observations of count Rumford to shew, "that in so intricate a subject, the utmost care is requisite, lest, after much labour, the inquirer should be forced to content himself with *approximations*, instead of accurate results, and valuations strictly determined." From this we are to conclude, that the object of Mr. Bull's experiments is to furnish us, in lieu of approximations, with accurate results, and valuations strictly determined.

In performing these experiments, equal quantities by weight of each kind of fuel, previously made absolutely dry; are burnt in the stove of the inner room; and the time is observed, during which, the combustion of each article will maintain the temperature of the inner room ten degrees higher than that of the outer room; which time is *supposed* to give the true relative heating power of the article. It is *endeavoured* to counteract the disturbing influence of different atmospheric temperatures, by regulating the heat of the outer room, so as to keep it always ten degrees below that of the inner room. This object Mr. Bull

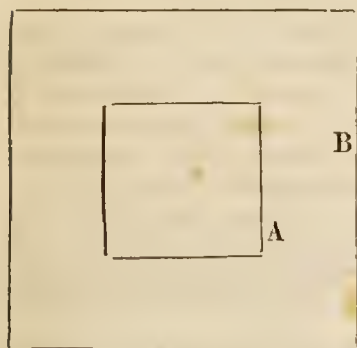
states that he effected, by opening a window in the outer room, when it was too warm; and kindling a fire in it, when it was too cold.

The committee have heretofore considered the experiments performed by Mr. Bull, with great attention; and were not able to perceive in them such "important discovery or useful improvement, on heat or on light" as should entitle Mr. Bull to the Rumford premium. The late communication of Mr. B. having relation to that decision of the committee; it may be proper to state some of the grounds on which it was formed, in detail.

Although the committee were of opinion that all due praise should be allowed to Mr. Bull, for the patient industry with which he has gone through a long and tedious course of experiments; yet at the same time they felt bound to state their conviction, that he had failed in the principal objects for which his experiments were intended. There are various grounds, on which these experiments are objectionable; of which the following will probably be deemed sufficient.—

1st. The principle, upon which these experiments are founded is radically defective; so that their results do not furnish any philosophical truths, on which reliance can be placed.

2d. They are of no practical value, and cannot be converted to any useful purpose, in the common concerns of life.—



In regard to the first of these objections, it will be seen, by referring to Mr. Bull's book and to the diagram which accompanies this report; that he has overlooked the effect of radiation which takes place from the inner to the outer wall, or from the surface A to the surface B in the diagram. This radiation will cause a continual escape of heat from the inner room to the atmosphere, whatever may be the intervening temperature of the air in the outer room; and this escape will

be more or less rapid *ceteris paribus*, in proportion as the weather is warmer or colder; so that any given experiment must afford a different result, according as the external atmospheric temperature should rise or fall during the time of its performance.

The above fact must be self evident to any person acquainted with the laws of the distribution of heat, and the committee believe Mr. Bull to be now convinced that it is philosophically true. He has however attempted to prove by subsequent experiments which were communicated to the Academy at their last meeting, and referred to this committee, that the surfaces A and B. are at nearly the same temperature with the air in contact with them, or at a uniform difference of temperature; during atmospheric changes. The committee are of opinion that if these new experiments prove any thing, they merely shew, that the different vertical strata of air in the outer room are of different temperatures; and that they increase in heat in proportion as they are nearer to A, and more distant from B. And if we admit this to be the fact, it does not alter the main and important truth, that A

and B will differ from each other, in proportion as the inner room differs from the atmosphere; whatever may be the temperatures existing between them. In regard to Mr. Bull's statement, that his experiments gave the same results during different temperatures of the atmosphere; this, if apparently true in some cases, would only show, that his outer wall is a slow conductor of heat. But the walls of houses, although slow, are nevertheless, sure conductors of heat; and it was expressly with a view to counteract their incompetency for philosophic purposes, that Mr. Bull's experiments were undertaken.—(See page 8th of Mr. B's book.)

The second objection of the committee belongs to the practical usefulness of Mr. Bull's experiments, supposing the principle on which they are founded to have been correct. Fuel as it is commonly burnt, contains more or less moisture; and is seldom or never used in the state employed by Mr. Bull, viz. that of absolute dryness. The quantity of water or aqueous matter retained in fuel by capillary attraction, amounts often to a third, and even to a half or more, of its weight; as we may learn from Mr. Bull's own book. Now this water in passing into vapour, renders latent a part of the heat produced by the combustion of the fuel. And since the more porous fuels, such as pine, afford much more capillary space, than the denser fuels, such as hickory; these articles will not have the same *relative* heating power in their common state, that they possess in their absolutely dry state. As the density of one fuel is found to be less, its amount of capillary space will be found to be greater; and in proportion to the amount of capillary space, will be the amount of water retained by capillary attraction; which water will go to render latent in practice, the heat produced by combustion. So that any man, who should govern his practice by Mr. Bull's tables; and should suppose that fuels as they are burnt, either in a green state, or in the driest state to which they are reduced by age and covering; have the same relative heating power, which they may possess in their totally dry state; would act under a perpetual error.—

The committee therefore do not recommend to the Academy, that any further measures be adopted in regard to this subject.

July 11th, 1827.

(Signed)

JACOB BIGELOW,  
DANIEL TREADWELL, } COMMITTEE.  
JOHN WARE,

AT a meeting of the American Academy of Arts and Sciences holden at Cambridge, July 11th, A. D. 1827, Dr. Bigelow, from the committee to whom was referred at the preceding meeting a communication from Marcus Bull, Esquire, of Philadelphia, made a report, which was accepted, and of which the above is a true copy.

Attest

F. C. GRAY, *Recording Secretary.*

It is with reluctance that I make a single remark on the foregoing Report of the committee; but, as they have made some errone-



ous statements, and presented fresh matter requiring comment, a review of it becomes necessary.

The committee state that they "have heretofore considered the experiments performed by Mr. Bull, with great attention." For this purpose it would be supposed necessary that my account of them should at least have been *read* with "attention:" but that this has not been done, is apparent from the first paragraph in their Report, in which they say, "That Mr. Bull has laid claim to the premium on the ground of certain experiments performed with an apparatus suggested to him by Dr. Hare," and they even refer subsequently, for another purpose, to the very page (8, quarto, 11, octavo edition,) and passages of my paper, from which I take the following extract, to show that they cannot have *read* my paper with common attention, and also for the purpose of correcting their misstatement.

"Having spent nearly four months of application in perfecting my apparatus, and removing difficulties which presented themselves at the threshold of every stage of the investigation, and feeling desirous to avail myself of any improvements which might be suggested to me, either in the apparatus, or the intended plan of conducting the experiments, I invited several gentlemen to examine it for that purpose, and among them, Dr. Hare, professor of chemistry in the University of Pennsylvania.

"The method which had been adopted, as described, to comply with the last requisition, [that the surrounding refrigerating medium be permanent at any required temperature,] did not appear to Dr. Hare to possess that degree of accuracy which was necessary, nor did it equal that which every other part of the apparatus, together with the intended plan of conducting the experiments, as described to him, appeared to possess. Dr. Hare stated to me, that "he had long been under the impression, that no accurate comparison could be made by means of the same single room heated at different times, with different fuel, on account of the varying temperature of the weather; nor by different rooms at the same time, from the difficulty of finding two rooms sufficiently alike, in form, aspect, size, and materials. It seemed to him indispensable, to have one room within another, so that, in the interval, a uniformity of temperature might be artificially sustained." As the method suggested by Dr. Hare, would remove this difficulty with which I had unsuccessfully contended, no time was lost in making a practical application of his suggestion, and a room of smaller dimensions was in consequence constructed within that originally intended for my experiments, in the best manner which my architect could devise; by which a free circulation of air is produced on all the exterior surfaces of the interior room, and this air may be sustained of a uniform temperature."

Dr. Hare was not apprised, of my experiments, until nearly four months after they were commenced, at which time he suggested the *addition of the interior room* to my "*apparatus*" as then constructed, and this is distinctly stated by me, as I had no wish to conceal it; but this alone constitutes only a *small part* of the *apparatus* employed by me; by which word, unless qualified, I have been led to suppose that an idea of the *whole of the instruments employed for any purpose*, is intended to be conveyed.

In the second paragraph the committee say, "Mr. Bull objects to the experiments of his predecessors, on subjects of this nature ; on the ground of their 'inaccuracy.'" This is very true, and I know of no other ground on which any man of sense would think it necessary to repeat laborious experiments upon *any* subject; but my experiments were almost exclusively made upon articles of fuel, not before experimented upon by any of my predecessors.

To the inference which the committee have drawn for me, from the observations of count Rumford, quoted by me, and by them, in the second paragraph, I must plead guilty ; as the avowal of any other "object" than that of furnishing "accurate results, and valuations strictly determined," for all practical purposes, must have impeached my judgment.

The assertion of the committee that, "The principle, upon which these experiments are founded is radically defective," has already been proved by experiment to be false in *practice*, and those only who are captiously disposed, and who wish to "split hairs with razors," will care whether in *theory* it be *true* or *false*.

The committee say that, "In regard to the first of these objections, it will be seen, by referring to Mr. Bull's book and to the diagram which accompanies this report ; that he has overlooked the effect of radiation which takes place from the inner to the outer wall, or from the surface A to the surface B in the diagram." If the committee mean to convey the idea that, at the time of constructing my apparatus, I was not aware of the fact, that heated bodies lose a portion of their heat by *radiation* ; I will excuse such an idea for the sake of its modesty ; but if they mean to be understood, that I have "overlooked the effect of" an *unequal* "radiation which takes place from the" "surface A to the surface B," in consequence of the latter surface presenting (as they aver) a refrigerating medium of *inconstant power* ; I must admit that I was not only ignorant at *that* time of any appreciable inequality of "effect" from such a cause, but that I am *now* ignorant of it, as

"—optics sharp it needs, I ween,

To see what is not to be seen."

*M'Fingal.*

The committee proceed in their Report by giving the following opinion, that "This radiation will cause a continual escape of heat from the inner room to the atmosphere, whatever may be the intervening temperature of the air in the outer room ; and this escape will be more or less rapid *ceteris paribus*, in proportion as the weather is warmer or colder ; so that any given experiment must afford a different result, according as the external atmospheric temperature should rise or fall during the time of its performance."

The committee advance the foregoing *opinion*, as a matter of fact ; as they say, "The above fact must be self evident to any person acquainted with the laws of the distribution of heat, and the committee believe Mr. Bull to be *now* convinced that it is philosophically true." If the committee really *believed* their opinion to be self-evidently

true, and so apparent, I can scarcely suppose that they would have formed such an estimate of my understanding, or my candour, as to have supposed that I could not perceive, or would not acknowledge, that it was *theoretically* or "philosophically true:" but to show that *their* fact does not amount to a "self evident" proposition, I have merely to state that among the many gentlemen of science who have expressed their opinions on this objection, urged by the committee, and who are perhaps quite as well "acquainted with the laws of the distribution of heat," no one of them has ever stated to me his belief in its *possible influence in practice*, when confined to the question of my walls, and to the difference and variations of temperature to which they are subjected, and but very few have expressed their belief in its probable truth in *theory*.

The question respecting the *philosophical* truth of the objection which the committee have urged, I have never considered of sufficient importance to admit or deny;\* but if I had done the former, I also admit, as probably do the members of the committee, that when a stone falls to the earth, the earth also approaches the stone, but must leave the practical amount to be estimated, by those who are fond of works of supererogation.

The committee state that I have "attempted to prove by subsequent experiments which were communicated to the Academy at their last meeting, and referred to this committee, that the surfaces A and B are at nearly the same temperature with the air in contact with them, or at a uniform difference of temperature; during atmospheric changes." I cannot refrain from expressing my surprise that such a statement should be made by gentlemen professing to have considered this subject "with great attention."

The experiments alluded to relate *entirely to the surface B*, as may be seen by the reader on referring to page 29, the practicability of maintaining this surface at a uniform temperature having been the exclusive object of these experiments, and the only important question of difference between the committee and myself. The wall of the surface A being constantly acted upon *from within* by an equal temperature, it was not exposed like the wall of the surface B *from without* to the variations in the temperature of the atmosphere.

I have never made any experiments to determine the difference of temperature between the surface A and the air in contact with it; but as the wall of this surface is of wood, and only one inch in thickness, and the wall of the surface B is of brick, and is ten inches thick, and both walls being usually exposed during an experiment to about the same difference of temperature on their reverse or opposite sides; I am inclined to believe, that a perceptible, but uniform difference would be found to exist between the temperature of the surface A, and the air in contact with it, and this in consequence of the different conducting power of the materials of which they are composed, and the great disparity in their thickness; this wall being but one inch, instead of ten inches, as is the case with the wall of the surface B, between which, and the air in contact with it, or rather at the distance of

\* See Letters, No. XVII, XVIII, XIX.



oneeighth of an inch, no practical difference was found. The committee will no doubt consider this, as a concession to their views; and if this straw will prevent their sinking they are at perfect liberty to catch at it, and their doing so would be quite in character, as is evident from many of their letters in the foregoing correspondence, particularly No. XVIII.

The committee do not appear to be satisfied with the results of these experiments, or even to admit that they bear upon the question at issue between us, and they still contend "that A and B will differ from each other, in proportion as the inner room differs from the atmosphere." Now, in confirmation of the accuracy of these experiments, and to show that they *bear directly* on the question at issue between the committee and myself, it will be proper to state in this place, that I have recently ascertained by placing against the surfaces A and B, the bulbs of two mercurial thermometers, accurately corresponding with each other, that when the temperature of the interior room is maintained  $10^{\circ}$  higher than that of the exterior, (as was done in my experiments on fuel,) the difference in the temperature of the surfaces A and B is  $3^{\circ}$ , and that no perceptible variation can be observed on the thermometers, during a change of  $10^{\circ}$  in the temperature of the atmosphere, which is as great as any which occurred during my experiments on fuel.

It will be observed by the reader, that the source of error urged by the committee, is entirely confined to that small portion of the heat given out by *radiation* from the surface A, to the surface B; but has no relation to that larger portion taken from A by the *air* of the exterior room, by the *conducting* process.

I have already stated that the difference in the temperature of the surfaces A and B, was found to be  $3^{\circ}$  during an experiment for that purpose, and I believe it to be very generally known by every "person acquainted with the laws of the distribution of heat," that the *whole amount* of heat radiated between bodies differing but  $3^{\circ}$  in temperature is *very small*, and as the committee appear to prefer opinions, to matters of fact; I give it as my opinion, that the *difference* in the amount of heat radiated between two bodies differing  $3^{\circ}$  in temperature at one time, and at another  $4^{\circ}$ , could not be discovered by any effect observable on the ordinary instruments used for measuring degrees of heat; and I presume, from the supposition already stated as having been made to me, by one of the committee, that they do not claim a greater *variation* than  $1^{\circ}$  in the temperature of the surfaces A and B; they also now admit that my "outer wall is a slow conductor of heat," this is all that I wish them to allow, and all that "any person acquainted with the laws of the distribution of heat," would require to know, to be convinced, that under such circumstances, no difficulty would be experienced in maintaining the surfaces A and B at a practically uniform difference of low temperature.

After the admission of the committee just quoted, they say, "But the walls of houses, although slow, are nevertheless, sure conductors of heat; and it was expressly with a view to counteract their incompetency for philosophic purposes, that Mr. Bull's experiments were undertaken. (See page 8th of Mr. B's book.)" If the walls of

houses were *not* "sure conductors of heat," and never lost it when once communicated to them, no urgent necessity would have existed that any experiments should have been made on this subject: but does the fact that they *are* "sure conductors of heat," render it impossible to make experiments sufficiently accurate for all practical purposes? What possible difference could there be as it respects utility, whether we ascertain the *relative*, or *positive*, amount of heat produced by the combustion of the different kinds of fuel, even if it were possible to ascertain the latter?

In noticing the latter part of the foregoing quotation, which appears to be rather obscure, I have merely to remark, that my experiments were not undertaken, as the committee say, "expressly with a view to *counteract* their [the walls of houses] *incompetency* for philosophic purposes," nor do I find in the matter of page 8, to which they refer, or indeed in any other part of my paper, any allusion to such a design. The intention of my experiments is clearly stated at page 21, (35 octavo) in the following words. "The object of my experiments being *practical utility*, rather than *scientific research*, I have estimated the comparative values of the different articles. These will be found in the last column of the table, and are equally applicable not only to every market, but for every change in the prices that can take place."

Whether this attempt to render the results of my experiments practically useful, has degraded them in the estimation of the committee I do not know; but it is very certain that a gentleman well acquainted with the manner in which scientific institutions are too frequently managed, observed, at the time I was computing the comparative values of specified quantities of the different articles of fuel, that this attempt to make them practically useful, would render them too *unphilosophical* in the eyes of the Academy.

"The second objection of the committee belongs to the practical usefulness of Mr. Bull's experiments," and in attempting to show that they are of no practical value, the committee state that, "Fuel as it is commonly burnt, contains more or less moisture; and is seldom or never used in the state employed by Mr. Bull, viz. that of absolute dryness." To the truth of this statement I most fully assent, (with the exception of the word "*never*,") but I am unable to perceive that it in any way disproves the "practical usefulness" or value of my experiments, and I have yet to learn, in what better state than "that of absolute dryness," it could have been employed by me.

As a preliminary and general answer to all their remarks about the quantity of moisture contained in the woods, and to those relating to the cause why green wood in some cases gives less heat than dry, and also to furnish my reason for having employed it in the latter state, I shall make some extracts from my paper, page 38, (62 octavo); for although the committee allude to some of these very passages, the whole of which are closely connected, yet it is evident, that they have not read them with much attention, as in that case, it is to be presumed that they would have devised some other objections more relevant to the point, or have omitted them entirely.

"The quantity of moisture absorbed by the woods individually, was

not found to diminish with their increase in density; whilst it was found that the green woods, in drying, uniformly lost less in weight in proportion to their greater density. Hickory wood taken green, and made absolutely dry, experienced a diminution in its weight of  $37\frac{1}{2}$  per cent., white oak, 41 per cent. and soft maple, 48 per cent.; a cord of the latter will therefore weigh nearly twice as much when green as when dry.

"If we assume the mean quantity of moisture in the woods, when green, as 42 per cent., the great disadvantage of attempting to burn wood in this state must be obvious, as in every 100 pounds of this compound of wood and water, 42 pounds of aqueous matter must be expelled from the wood, and as the capacity of water for absorbing heat is nearly as 4 to 1, when compared with air, and probably greater during its conversion into vapour, which must be effected before it can escape, the loss of heat must consequently be very great.

"The necessity of speaking thus theoretically on this point, is regretted; but, it will be apparent, that this question of loss cannot be solved by my apparatus, as the vapour would be condensed in the pipe of the stove, and the heat would thereby be imparted to the room, which, under ordinary circumstances, escapes into the chimney.

"The average weight of moisture in different woods which have been weather seasoned from eight to twelve months, will not be found to vary materially from 25 per cent. of their weight; every economist, therefore, will see the propriety of keeping his wood under cover in all cases where this is practicable."

Now, suppose that I had made experiments upon the woods at all the different degrees of humidity of which they are susceptible; for example, 49 different experiments upon soft maple, 1 of which in an absolutely dry state, and 48 others, differing from each other 1 per cent. in moisture. The first parcel experimented upon we will suppose to have been absolutely dry, and to have weighed 100 ounces, the second 101 ounces, or 100 of wood and 1 ounce of water, as we must suppose that each parcel contained 100 ounces of wood or ligneous matter, although the quantity of moisture should have varied from 1 to 48 ounces in the different parcels. Is it not apparent, that any heat rendered "latent" by converting the moisture into vapour in the body of my stove, would have again become *free* caloric, as the vapour condensed in the pipe of the stove, and would thereby have been imparted to the air of the room? and can there be any doubt that the whole quantity of moisture so converted into vapour, would have been condensed in passing through forty-two feet of extra thin *black* tin pipe, two inches in diameter, and principally formed into elbow joints; the pipe near the extremity for several feet being also *always at the temperature of the air of the room?*

This fact must make it appear self-evident, that the moisture resulting from the condensed vapour, must be reduced to the *precise temperature* that it possessed in the wood before the combustion of the latter; and there can be no doubt that this moisture would contain no other heat than its *specific heat* of that particular temperature; and that this specific heat would be neither more nor less than that of the moisture when combined with the wood, as the compound must ne-



cessarily be supposed to have acquired the temperature of the room, from being placed within it before, and remaining in it during the experiment.

That the results of these 49 different experiments would have been the *same*, no "person acquainted with the laws of the distribution of heat" will doubt: what then would have been gained, had the same process been gone through with all the woods experimented upon?

I ask the committee to state, what other, or more accurate standard can be taken in making comparative experiments upon *any* subject, than the *positive* weight or measure of the things subjected to experiment? If we wish to learn the quantity of heat given out in the combustion of the different woods, we must know the absolute quantity of *wood* or ligneous matter burned in each instance, and how are we to know this unless we deprive it of its extraneous moisture? and must not this standard be resorted to in any case, whether we burn it *green* or *dry*?

My experiments were intentionally made upon *wood* and *coal*; and not upon these articles compounded with water; and the question whether it be more economical to burn wood *green* or *dry*, was not the object of my inquiry, nor is it necessarily connected with the subject, as this must entirely depend upon the *manner* of burning it; for example, were it consumed in a stove, with a pipe similar to that used by me in my experiments, or in any other construction of apparatus for burning fuel, by which no portion of the heat generated is lost by escaping into the chimney, the same quantity and kind of wood or ligneous matter, would obviously be of the same value, in every possible state, from *green* to that of *absolute* dryness; but if consumed in a chimney fire-place of ordinary construction, by which 90 per cent. of heat is lost, the *dry* would undoubtedly be the most economical; this fact is stated in my paper, is urged by the committee at some length, and is also very generally known by every man who has paid any attention to, or cares about economy in his fuel; and it must be evident that I am in no way answerable for the losses sustained by those, who either ignorantly, or from choice, think proper to burn *green* wood, in place of *dry*, or who burn either, in an improper manner.

Experiments to determine the question of the comparative economy of burning wood absolutely dry, or with the various degrees of moisture of which it is susceptible; could only be performed by burning it in these several states, and *in every kind of apparatus made use of for its combustion*. This question, I leave to be solved *philosophically*, by those who may think it of sufficient importance, to justify the performance of such a course of experiments; but for all *practical* purposes, the results of my "*experiments to determine the loss of heat sustained by different constructions of apparatus ordinarily used for the combustion of fuel*," will probably be found sufficiently accurate;\* as, in proportion to the loss of heat sustained by the apparatus made use of, will be, in most cases, the comparative loss of burning wood in any

\* The committee have not thought proper to raise any objections, or even to notice these experiments, although obviously of the first importance in the question of economy in the use of fuel; and respecting which, they appear to be so very solicitous on points of comparative insignificance.

other state than that of absolute dryness ; and this loss will be proportional to the quantity of moisture positively contained in the woods ; but not to their *capacity* of containing it, as stated by the committee, in their report.

The committee attempt to prove from the fact, that the woods possess different *capacities* for containing water, or aqueous matter, according to their density, that "these articles will not have the same *relative* heating power in their common state, that they possess in their absolutely dry state. As the density of one fuel is found to be less, its amount of capillary space will be found to be greater ; and in proportion to the amount of capillary space, will be the amount of water retained by capillary attraction ; which water will go to render latent in practice the heat produced by combustion : " and from these premises, they draw the following grave conclusion ; " that any man who should govern his practice by Mr. Bull's tables," "*would act under a perpetual error.*"

The "common state" of wood as to moisture is an indefinite term, and may be supposed to comprehend all states ; as it is well known that bakers, generally use their wood absolutely dry, whilst other persons use it as they please, with all the degrees of moisture of which it is susceptible ; but whatever may be the state intended to be referred to by the committee, the question as to its "*relative* heating power," has already been sufficiently noticed.

That the "capillary space," or the *capacity* of these spaces in the different woods for containing water, should be stated by the committee as an invariable standard for what they do, or may *happen to contain*, is certainly a doctrine entirely *unique*. The question with the economist would be, *ceteris paribus*, not what they are *capable* of containing, but what do they *actually contain*, and I am not aware that the solution of such a problem could be given (were its solution necessary) by any "tables," or indeed by any thing short of actual desiccation.

My "tables" are constructed to give the relative value of specified quantities of the different *woods* and *coals*, regard being had to their relative heating power in equal quantities by weight, and also to the *average weight* of the *combustible matter* contained in the quantities specified ; taken, for the woods, as is stated in my paper, page 26, (43 octavo.) "From a pile of swamp white oak of medium size, which had been cut the preceding winter, and weather-seasoned during the interval, this being the state in which the *largest portion* of wood is sold ;" and with the *coals*, there was no difficulty, their volume being the same whether *wet* or *dry*.

It is believed that my "tables," so far from leading those governed by them into "perpetual error," give as accurate information as utility requires, or the nature of the subject will admit ; it being evidently impossible for any man to construct a set of tables adapted to every change in volume produced by the different degrees of humidity of which the woods are susceptible ; but even were this practicable, and had it been done by me, I ask any man possessing common sense, whether such a set of tables would enable him to determine by bare *inspection*, the *real quantity of moisture contained in any parcel of wood that might be shown him* ? Such tables could pretend to no other

possible use, and their entire incompetency to such a purpose must be admitted. If it be really necessary that this fact should be determined, it must be done by the only possible process, that of desiccation: the question of utility then returns; whence the use of such tables, after the fact is determined? If the committee cannot answer this, they are less clear-sighted than most other individuals.

I have now concluded my review of the Report of the committee, and take the liberty of remarking, that as my experiments were not made with a *pen upon paper*, I do not fear any candid examination to which they can be subjected. The results stated in my tables, were obtained by *actual and accurate experiment*; and the apparatus employed by me, is at the service of any *philosophical* inquirer, who has the patience and the talent necessary to repeat them.

Let the experiments be made upon *any* article of fuel which may be selected, taken in equal quantities of the *combustible matter* by weight, and let these be burnt both in the *wet* and *dry* states, and at different periods, presenting the *greatest* and the *least* diversity in the changes of temperature in the atmosphere; and if the sources of error which are stated by the committee to exist, be matters of fact, they will be found, and their *exact value* or importance learned, as they will be measured, precisely, by the difference in the *time*, which equal quantities of the combustible, shall, by the heat given out in its combustion, maintain the temperature of the interior room,  $10^{\circ}$  higher than that of the exterior.

I have ever been aware of the great responsibility incurred by those who publish as accurate, experiments which have not been conducted with that rigour which investigations of this kind always demand, and more especially when the results given are intended to regulate any of the common concerns of life. Under a strong conviction of this truth, and having had no theories of my own to establish, or any interest in either coal or wood lands, my researches were prosecuted, and have been presented to the world. If the results which I have obtained are correct, mankind would be benefited by acting upon them, and if any man, or body of men, whose stations give currency to their opinions, should impugn them upon grounds which are untenable, they undertake a responsibility equally great, and to their retarding of useful knowledge, add individual injustice. If I am not mistaken, I have in the foregoing remarks proved that the committee have placed themselves in the latter predicament, and that they have taken a course calculated to lead the public into "perpetual error."

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*Account of the Donation made by Count Rumford to the American Academy of Arts and Sciences, for the establishment of a biennial Premium, extracted from the Boston Journal of Philosophy and the Arts, for April 1824.*

In the year 1796, Count Rumford, then residing in London, presented to the American Academy of Arts and Sciences five thousand dollars, in three per cent. stock, for the purpose of establishing a biennial premium to be awarded to the author of the most important discovery or most useful improvement on heat or light which should be made in any part of America. The following letter addressed to the President of the Academy, accompanied the donation, and contains an account of the views of the liberal donor, and of the terms upon which the premium was to be awarded.

*To the Hon. John Adams, President of the American Academy of Arts and Sciences.*

SIR—Desirous of contributing efficaciously to the advancement of a branch of science, which has long employed my attention, and which appears to me to be of the highest importance to mankind; and wishing at the same time to leave a lasting testimony of my respect for the American Academy of Arts and Sciences, I take the liberty to request that the Academy would do me the honour to accept of five thousand dollars, three per cent. stock, in the funds of the United States of North America, which stock I have actually purchased, and which I beg leave to transfer to the Fellows of the Academy, to the end that the interest of the same may be by them, and by their successors, received from time to time, for ever, and the amount of the same applied and given, once every second year, as a premium to the author of the most important discovery, or useful improvement, which shall be made and published by printing, or in any way made known to the public, in any part of the continent of America, or in any of the American islands, during the preceding two years, on heat, or on light, the preference always being given to such discoveries as shall, in the opinion of the Academy, tend most to promote the good of mankind.

With regard to the formalities to be observed by the Academy in their decisions upon the comparative merits of those discoveries, which, in the opinion of the Academy, may entitle their authors to be considered as competitors for this biennial premium, the Academy will be pleased to adopt such regulations, as they in their wisdom may judge to be proper and necessary. But in regard to the form in which this premium is conferred, I take the liberty to request that it may always be given in two medals, struck in the same die, the one of gold, and the other of silver, and of such dimensions, that both of them together may be just equal in intrinsic value to the amount of the interest of the aforesaid five thousand dollars stock, during two years; that is to say, that they may together be of the value of three hundred dollars.

The Academy will be pleased to order such device or inscription to be engraved on the die they shall cause to be prepared for striking these medals, as they may judge proper.

If during any term of two years, reckoning from the last adjudication, or from the last period for the adjudication of this premium by the Academy, no new discovery or improvement should be made, in any part of America, relative to either of the subjects in question, (heat or light,) which, in the opinion of the Academy, shall be of sufficient importance to deserve this premium; in that case, it is my desire that the premium may not be given, but that the value of it may be reserved, and being laid out in the purchase of additional stock in the American funds, may be employed to augment the capital of this premium; and that the interest of the sums by which the capital may from time to time be so augmented, may regularly be given in money, with the two medals, and as an addition to the original premium, at each succeeding adjudication of it. And it is further my particular request, that those additions to the value of the premium arising from its occasional non-adjudications may be suffered to increase without limitation.

With the highest respect for the American Academy of Arts and Sciences, and the most earnest wishes for their success in their labours for the good of mankind,

I have the honour to be, with much esteem and regard,

Sir,

Your most obedient,  
humble servant,

RUMFORD.

*London, July 12th, 1796.*

Upon the receipt of the donation, the thanks of the Society were presented to Count Rumford in the following terms:

Voted, That the thanks of the Academy be presented to Count Rumford, for this his very generous donation, and that they experience the highest satisfaction in receiving this additional and very liberal aid for the encouragement and extension of those interesting branches of science, which he has specified as the objects of his gratuity, and which *he* has so successfully cultivated: That they entertain a high sense of the sentiments and views, so becoming a philosopher, which have prompted him to this distinguished act of liberality; and in the execution of the grateful office, which they have undertaken, of awarding and distributing the premium which Count Rumford has thus appropriated, they will sacredly comply with the conditions of the donation; indulging the hope, that he will meet his reward, in learning that many in his native country are thereby excited to emulate his labours, and to promote the accomplishment of his beneficent wishes for the advancement of science, and the augmentation of human happiness.

At a meeting held in May 1801, the Society voted, that they would, at their meeting in May of the next year and afterwards, biennially at their May meeting, decide upon the discovery or improvement which appeared to deserve the Rumford premium. The subject we believe has frequently been brought before the Society, and they have been ready at the appointed time to confer the premium upon any in-

dividual whose claims were sufficient to authorize it. No discovery, however, or improvement has yet been made which has been deemed worthy this honour, and the fund has of course been accumulating, according to the terms of the donation, ever since it has been in the hands of the Society.

At the present time the fund amounts to \$7361, 19, in 6 per cent., and \$7050 in 7 per cent. stocks. The premium awarded would therefore be the interest of these sums for two years; three hundred dollars in the form of a silver and a gold medal, and the residue in money. This we believe is one of the largest premiums offered by any society or institution for discoveries in science or improvements in the arts, and is well worthy the attention of the scientific and ingenious men of our country.

The next meeting of the Society will be held on Tuesday the 25th of May next, at their room in the Boston Atheneum. At this meeting the Society will be ready to decide upon any claims which may be offered for the premium in question; it being the regular biennial period at which, by their vote of 1801, this subject comes before them.

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As I do not now consider myself a candidate for the Rumford premium, it will not be deemed improper for me to review the conduct of the American Academy, both as it regards their general management of the trust committed to them, and their particular treatment of my late claim.

The donation of Count Rumford was made in July 1796, yet it appears that the Academy did not fix any period for the adjudication of the premium until May 1801, thereby suffering so much time to elapse as that the donation would appear to have become liable to forfeiture. They have been also remiss in their duty to the public, in neglecting to give sufficient publicity to the letter of Count Rumford accompanying the donation, by which it appears that the premium is intended to be a matter of fair competition to every inhabitant "in any part of the continent of America, or in any of the American islands." The ground for supposing the Academy to have been remiss in this particular, is inferred from a statement made to me by a member of that body, that with the exception of myself, the claimants for the premium have been residents of New England; and also from the knowledge of the fact, that very few of the present generation are aware of its existence.

The intention of the Count appears to be so obvious, that it is difficult to conceive of any construction which can be put upon the language made use of in his letter, to justify the Academy in never having awarded the premium, although it has been at their disposal for a period of nearly thirty years.

In the vote of thanks presented by the Academy to Count Rumford, they say, "that they experience the highest satisfaction in receiving this additional and very liberal aid for the encouragement and extension of those interesting branches of science, which he has specified as the objects of his gratuity, and which *he* has so successfully culti-



vated :” and further, that they “indulge the hope, that he will meet his reward in learning that many in his native country are thereby excited to emulate his labours.” Now it is very well known that these branches of science were cultivated by him with a view almost exclusively to their utility and application in the ordinary concerns of life, and he may without impropriety be emphatically styled *the practical experimenter*. His experiments were not prosecuted with a view to discover *improved substitutes* for, or *new sources* of heat, or light, but to render those, then in possession, together with the sources of their production, more subservient to the good of mankind.

The discovery of new sources of heat or light, should these ever occur, will probably be the result of *accident*, rather than of any laborious research, and we cannot suppose that the Count would think it expedient to establish a premium, as a reward for fortuitous discoveries ; nor can we suppose that he ever contemplated that successive discoveries of this nature would in any way be made every two years, and these sufficiently numerous to admit of “competitors for this biennial premium.” That the objects which Count Rumford intended to encourage were of a very different nature, more extensive and diversified, and less difficult to accomplish than has evidently been supposed by the Academy, is plainly to be inferred from the following passage in his letter : “And it is further my particular request, that those additions to the value of the premium arising from its *occasional* non-adjudications, may be suffered to increase without limitation.” And it is equally evident that he contemplated a state of *progressive improvement* in the several objects intended to be encouraged, and not *perfection*, in any instance, as, had the latter been his intention, whence the necessity of establishing a succession of premiums coextensive with the duration of time ?

The large amount to which the premium has accumulated, in consequence of never having been awarded, is another and increasing difficulty both to the Academy and to the candidates, and as it respects the latter, it is most unjust, inasmuch as the objects which would be considered entitled to the *original* premium, are not considered entitled to it in its present state, so that as the premium increases in amount, the requirements on the part of the Academy also increase, and this evil cannot now be removed, as even supposing it to be awarded for the future biennially, the amount of the premium will never be lessened, except in the case of a reduction of the interest of the stocks in which the fund is invested.

By the conditions of the trust, no necessity exists that applications should be made for the premium ; it is then clearly the duty of the Academy to seek for the proper objects of its bestowment, in case applications shall not be made for it : if so, why is it, that among the more prominent discoveries and improvements which have been made in this country on the subjects of heat and light since its foundation, that the premium has not been awarded to Professor Hare, for the discovery of the “Hydrostatic or Compound Blow-pipe,” and for his “Galvanic Deflagrator?” and to Professor Olmsted, for the discovery of the applicability of cotton seed to the making of “*Illuminating Gas*?” If

it be said in reply, that the two first were discoveries of a character too *strictly philosophical*, and therefore not calculated "most to promote the good of mankind," the same objection cannot surely be urged against the latter, as it evidently possesses a *practical* character; but should it be said in reply to this, that it was not *sufficiently practical*, inasmuch as it could not be used by every body, and consequently could not "be converted to any useful purpose, in the common concerns of life," in this case I beg leave to refer the Academy to their transactions, where, as I am informed, they will find *many* applications for the premium for discoveries or improvements of *precisely the latter character*, and no insuperable difficulty can be supposed to have existed in determining which was the "*most important*" that occurred, during any of the many biennial periods which have elapsed. Should the *discretionary power* with which they are invested be resorted to as a general justification of their conduct, this covert appears to be too unsound to afford the desired protection.

With respect to their treatment of my claim, I shall be permitted to remark, that my experiments on Fuel, &c. were not commenced with any intention to make them the ground of an application for the Rumford premium, having been entirely ignorant of its existence, until after my arrangements for their performance had been principally matured, and a few of the preliminary experiments actually performed.

My course of experiments was commenced in November 1823, (and finished in April 1826,) and prosecuted almost literally night and day, until May 1824, at which time I had occasion to visit Boston, and when on my way thither, in passing through New York, a gentleman who had seen the apparatus constructed for my experiments, put into my hands the "Boston Journal of Philosophy and the Arts," for April 1824, containing the foregoing account of the donation made to the American Academy by Count Rumford. My arrival in Boston was only a day or two previous to the regular biennial period fixed by the Academy for deciding upon any claims for the Rumford premium, and I was not then prepared to become a competitor; yet as I expected to complete my experiments before the next meeting of the Academy in August, I was advised to present a memorial to that body, explaining the object of my experiments, and requesting them to suspend their decision on the Rumford premium. A particular description of my apparatus, and the intended plan of conducting the experiments, was also given to Dr. James Jackson and to Dr. Jacob Bigelow, who were on the committee subsequently appointed by the Academy, (on the 25th of May, 1824,) to report on applications for the Rumford premium, and to which committee, I presume, my memorial was referred.

Intending to leave Boston on the 27th, and being desirous of learning the fate of my memorial to the Academy, I called at the residence of Dr. Jackson, but had not the pleasure of finding him at home. Very soon after, however, I received a friendly note from him, in which he appears most fully to have anticipated the object of my call at his house; and from his known candour, and his uniform kindness to me, both at that period and subsequently, I feel assured that he will not be displeased with my making the following extract from his note,



which, although it is not considered by me to have been written in his official capacity of chairman of the committee, is nevertheless viewed as expressing their sentiments. "I take it that our committee will do nothing until they receive your communication. If you are not able to make that as early as the 10th of August, it will be best for you to address a note to me stating how the matter stands, and when you probably can make the communication. It is a matter of doubt whether the premium of the last two years can be awarded to you, if your communication should be satisfactory—as your discovery was not made known during those two years—that matter will be considered: but at least you will be a fair candidate for the next two years."

I have been thus particular in my description, and have made the preceding extract, for the purpose of showing what was the opinion of these gentlemen, at that time, respecting my claim as a fair candidate for the premium; and to show that every inducement which I had a right to expect from members of the Academy, was held out to me, to prosecute my labours to the greatest degree of perfection within my power. The course of "experiments to determine the comparative loss of heat sustained by using apparatus of different constructions, for the combustion of fuel," was not originally contemplated by me, but was suggested by them, and considered as a very desirable appendage to render the subject complete. My experiments were not instituted for the purpose of solving *philosophical* but *practical* questions, and such as were considered of great importance to all classes of society, and I claim for them the discovery of the relative value of the different kinds of wood and coal used for fuel, and also the discovery of the comparative economy of the ordinary apparatus made use of for their combustion; and these discoveries were made at a sacrifice of health, convenience, money, and nearly one and a half years of time, which would not have been compensated by the whole amount of the premium, large as it may appear. But entirely independent of these considerations, I have yet to learn, wherein consisted the justice of the Academy, in rejecting my claim to the Rumford premium, upon the untenable ground set forth by their committee, the futility of which may be considered as the strongest, among the many encomiums which have been paid to the accuracy of my experiments, both in this country and in Europe.

The following considerations in relation to the trust, and the duties of the trustees, appear to me to be so obviously just, as to require little additional remark.

It was the plain and evident intention of Count Rumford, that the premium should be awarded to the author of the *most important* discovery, or useful improvement, on the subjects of *heat* or *light*; and there can be no doubt, that those subjects are susceptible of discoveries and improvements, of such a nature as was contemplated by the donor. Yet the Academy has never awarded the premium, and seems to act under the impression that no discovery or improvement of the kind can be made. What then is the plain duty of the Academy? If its opinion be, that the trust cannot be executed, that opinion ought to be announced in the most public manner; that the fund which is



in the hands of the trustees, may go, where of right it belongs, to the representatives of Count Rumford. If no such opinion as this exists in the Academy, or if the opinion does exist, and is unsound, the Academy ought to execute the trust. To them is imparted by the donor the power of awarding the premium to the most important discovery or useful improvement; and consequently the power of judging; but this power is to be guided by a sound discretion, and to be exercised under a due sense and just observance of the trust reposed in them. These then are questions, which the Academy is bound to answer, and to answer satisfactorily—how is it, and why is it, that this premium has never been awarded? how is it, and why is it, that this trust is not executed? how is it, and why is it, that scientific men are invited to direct their efforts, prosecute their researches, and exercise their faculties, on these subjects, under a delusive hope and promise of distinction and reward, which can never be attained?

*Aldebaran* \*

*Polaris*

*Arcturus*

*Mizar*

*Algenib*



*THE GREAT SCALES LIGHT*

*AT THE VERNAL EQUINOX LAT 41° 18'*

THE  
AMERICAN  
JOURNAL OF SCIENCE AND ARTS.  
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ART. XXXI.—*Observations on the Zodiacal Light ; with an inquiry into its Nature and Constitution, and its Relations to the Solar System ;* by DENISON OLMSTED, Professor of Natural Philosophy and Astronomy, in Yale College.

Read before the American Association for the Advancement of Science, at the Annual Meeting at Albany, August, 1851.

I SUBMIT to the Association a series of observations on the Zodiacal Light, made by me at Yale College from 1833 to 1839, upon the basis of which I propose to offer a new description of this mysterious phenomenon, and a brief inquiry into its nature and constitution, and its relations to the solar system. Particularly, I propose to inquire whether or not it is the origin of the meteoric showers of November and August.

Various circumstances conspire to interrupt the continuity of a series of observations on the zodiacal light ; among which are the following :—

1. The comparatively few nights in the year when, in our climate, the sky is cloudless, and the atmosphere sufficiently clear to afford good observations on a light so feeble and diffuse.

2. The low angle which the zodiacal light makes with the horizon for the greater part of the year while it is visible.

3. The presence of the moon, which entirely effaces it ; and, occasionally, for long periods, the presence of Venus or Jupiter, and sometimes of both planets. The light of Venus, especially, is often so bright, and the planet is so situated in the midst of the zodiacal light, as greatly to interfere with observations. Hence, a



number of years are necessary of diligent attention to the phenomena of this light, in order to become well acquainted with its habitudes and laws. Nor can I pretend to have made the best possible use of the opportunities afforded for viewing it, during the six years that my attention was directed to it. On the contrary, my observations were often interrupted by ill health, and other causes beyond my control. Still, they were sufficient to convince me that my previous knowledge of this body was exceedingly defective, and my notions of it very erroneous; and the same may justly be said of most or all of the descriptions and graphic representations of it given in works of science.

I will therefore, first, attempt an accurate *description* and *representation* of the zodiacal light.

Since the direction of this body is oblique to the circles of diurnal revolution, and since it appears only immediately before or immediately after the sun, and therefore more or less of it falls within the twilight, consequently, its appearances are very different in different latitudes, being seen best of all in the tropical regions, where its direction always makes a high angle with the horizon, and where the twilight is short; and being scarcely visible in such high latitudes as London and Edinburgh, except near the time of the equinoxes. Hence British writers who have attempted a description of it, have usually given one that is altogether vague and inaccurate. The lower latitude of our place of observation ( $41^{\circ} 18' 30''$ ) affords a much better view of it, and my description and representation of it will conform to its appearance at this latitude.

I learn from my friend, Prof. Dana, that while with the Exploring Expedition in the torrid zone, he seldom failed of seeing the zodiacal light morning or evening, when not prevented by some of the causes before enumerated; but during the summer months in our climate, we hardly see it at all. At the beginning of autumn we look for it in the morning sky, and at the end of autumn in the evening sky. The state of the atmosphere most favorable for seeing it at its minimum intensity, is that peculiarly transparent condition which either precedes or follows a copious rain. The presence of a black cloud, also, near the horizon, frequently enables us, by contrast, to see more distinctly the faint diffusive light of the upper portions. With these advantages we may unite that of fixing one eye on a darker portion of the heavens a few degrees to the right or left, and looking askance with the other eye over the region of the object sought. This last expedient will usually be found useful for fixing its exact boundaries, in its various stages of intensity.

Although, as was first remarked by Mr. E. C. Herrick, faint traces of the zodiacal light may be seen in the northeast early in August, yet it will hardly be obvious to common observation be-

fore the latter part of September. I quote from my record for September 25th, 1835:—

Observed the zodiacal light from 3 to 4½ o'clock, A. M. Very faint. Seen only by fixing the right eye on the region of Canis Major, and carrying the left eye along the ecliptic. Covers Regulus and the cluster in Cancer, and terminates a little south of Castor.

The earliest distinct view I have obtained of this body in the *evening* sky, was on the 21st of November, 1837, when I have the following record:—

Have constantly searched for the zodiacal light in the evening since the 13th inst. Imagined that that part of the milky way where this light would cross it was more luminous than common, but the light is ambiguous on account of the presence of Venus. But this evening examined in company with three of my astronomical pupils, all distinguished for acuteness of vision. At 7 o'clock, Venus being near the horizon and hid behind a cloud, we could severally define the boundaries of the zodiacal light. By fixing the right eye on the milky way near Altair, and the left eye near the head of Capricornus, we could discern a pyramid less bright than the milky way, but still sufficiently distinct to be sure of its presence. Its upper edge grazed  $\alpha$  and  $\mu$  Capricorni and  $\beta$  Aquarii, its vertex reaching to the right shoulder of Aquarius. Light very feeble and diffuse, but the triangular space between it and the milky way, embracing the Dolphin, perceptibly darker. Elongation from the sun  $90^\circ$ .

As a description of the zodiacal sufficient to guide the observer I will offer the following: From the middle of September until the latter part of November, he will confine his attention to the *morning* sky. An hour and a half before daybreak (which is at that season of the year, in our climate, about 4 o'clock,) he will first discern a feeble, diffuse, and scarcely visible light, of a pyramidal figure, extending from the horizon upward through the zodiac to Gemini, covering Regulus and Presepe, and terminating a little south of Castor. Near the horizon its material is usually mixed up with the vapors that prevail there, so as to prevent its forming a definite boundary at its base; but from an altitude of a few degrees above the horizon, the light gradually declines until it fades into nonentity. Along the central part of the pyramid the illumination is greater than at the borders. From the greater length and amplitude revealed to us by circumstances peculiarly favorable for observation, we have reason to think that, on ordinary occasions, we do not see the whole of the body, but that it really extends further than its visible boundaries, both in length and breadth. If the observer continues to watch this body from the middle of September onward through the month of October to the middle of November, he will perceive that the vertex or visible terminus moves along through the order of the signs, and nearly at the same rate with the sun, appearing, on the

25th of October, to occupy the space south of Denebola in the tail of the Lion, terminating a little above Regulus. From this time until the middle of November it appears nearly stationary, ascending from the horizon to the constellation Leo, in some part of which it terminates, the vertex varying somewhat in altitude with the condition of the sky. After the 13th of November, the light fades in the morning sky, contracts in dimensions, and soon becomes stationary and then retrograde with respect to the sun, proceeding eastward no further than  $\gamma$  Virginis, a point which it reaches by the 26th of November, having at this time an elongation of only  $60^\circ$ , whereas a fortnight before the elongation was  $90^\circ$ . As the sun advances in the ecliptic, while the light appears nearly stationary, the elongation on this side continues to diminish, as well as the dimensions and the illumination, until early in January, after which it is scarcely seen in the east until August.

The foregoing general statements are supported by observations taken at different times through the period of six years before mentioned, a few of which I extract from my records:—

*Nov. 26, 1837.*—This morning about daybreak, saw the zodiacal light—very bright and distinct, but elongation only  $60^\circ$ .

*Nov. 28.*—Commenced observations at 5 o'clock. Zodiacal light brighter than usual in preceding years at this season, but the vertex appears nearly stationary in Gamma Virginis.

*Dec. 5.*—Zodiacal light visible this morning as early as 3 o'clock. Not quite so bright as on the 28th of November, but increased in brightness from 3 o'clock till daybreak—vertex still in  $\gamma$  Virginis.

*Dec. 9.*—Examined the eastern sky from 4<sup>h</sup> 30<sup>m</sup> till daybreak. Very cold and clear. Zodiacal light much less bright than on the 5th. Width also less; when I first went out could scarcely see it. Became distinct by 5 o'clock, half an hour before daybreak. Yet much feebler than it was ten days ago. Contracted between Spica and Theta Virginis,  $4^\circ$  north of Spica, whereas a few days since the border grazed this star.

*Jan. 18, 1837.*—Zodiacal light very diffusive and ill-defined. Seen after this no more in the east.

We will now introduce the observer to the western sky. Here the zodiacal light first comes into view, so as to be distinctly defined, about the 21st of November, at which time it lies far in the southwest, crosses the milky way, the head of Capricornus, and has its vertex near the right shoulder of Aquarius, with an elongation from the sun of full  $90^\circ$ . From this time it climbs rapidly upwards, until by December 2d it reaches nearly to Algenib in the equinoctial colure, having an elongation of more than  $100^\circ$ . By about Christmas the vertex reaches almost to Alpha Arietis, having an elongation of towards  $120^\circ$ . It becomes nearly stationary through the month of January, but in



February and March it moves slowly onward through Taurus to Gemini, beyond which it scarcely advances. The accompanying diagram is intended to represent the general appearance of the zodiacal light, when seen under favorable circumstances near the time of the vernal equinox. It is seen of a pyramidal form with a broad base resting on the horizon. Its northern border grazes the bright star Algenib in Pegasus, passes south of Alpha Arietis seven or eight degrees, and about two degrees south of the Pleiades. Along its southern boundary we recognize the stars in the mouth and neck of the Whale, and still higher, Aldebaran, the Hyades, and the horns of the Bull. The successive positions attributed to the zodiacal light from the time of its earliest appearance in the western sky, the 21st of November, to the vernal equinox, are not absolutely uniform, but they still correspond to observations made during the six years before mentioned, as will appear from a few extracts from my record book. I have already recited the observation of November 21st, 1837, when the return of the body to the western sky was first recognized.

*Nov. 26, 1837.*—Light feeble, Venus being very bright; but seen after Venus was set, reaching nearly to the Fish south of Pegasus. Elongation  $100^{\circ}$ .

*Dec. 2.*—New moon begins to interfere with observations; but this evening the zodiacal light was visible after the moon was set. Covers the Pentagon in Pisces and reaches beyond it. Elongation  $110^{\circ}$ .

*Dec. 18.*—Early part of the day a violent rain and high wind. Cleared off towards night. Zodiacal light very bright, reaching at least to Alpha Arietis—nearly as bright as the milky way. Elongation  $120^{\circ}$ .

It ought to be remarked that the phenomena of this body were peculiarly striking in the autumn and winter of 1837, and the observations made this year show a greater intensity of light and a greater elongation from the sun than those of corresponding dates in 1835 and 1836.

*Dec. 21, 1835.*—This evening atmosphere very transparent. Zodiacal light very conspicuous, reaching nearly to Algenib, though quite faint towards the vertex. Elongation  $90^{\circ}$ .

*Dec. 28, 1837.*—Night favorable. Appeared to me not to reach quite so far eastward as it did a few nights since—certainly not beyond the equinoctial colure. Could not be certain much further than the pentagon of stars in Pisces. Elongation  $75^{\circ}$ .

*February 7.*—Zodiacal light very conspicuous since the last moon, but has advanced eastward very little since Christmas, still reaching only to Alpha Arietis. Elongation  $75^{\circ}$ .

*Feb. 24.*—First night since the moon has been away. Sky favorable for observation. Zodiacal light bright and well defined, its axis nearly in the ecliptic. Reaches to the space between Aldebaran and the Pleiades. Elongation  $85^{\circ}$ .

*March 26.*—Zodiacal light very bright, reaching above the Pleiades, which are a little north of the axis. Elongation  $60^{\circ}$ .

March 29.—Light more faint. Elongation  $60^\circ$ . Vertex near the ecliptic.

April 6.—Light fading rapidly. Very diffuse.

May 1.—Last night a very plentiful rain after a series of warm days. To-day air keen and sky very clear. This evening zodiacal light remarkably distinct, (for this season of the year,) being discernable much nearer the horizon than common, and reaching further eastward among the stars than I ever observed it before, namely, into the neighborhood of Castor and Pollux. Elongation  $60^\circ$ , but presumed to be much greater than it would be but for the extraordinary transparency of the atmosphere.

May 10, 1834.—Zodiacal light seen for ten minutes after twilight ceased—say till ten minutes after nine. Reached to Castor, but very diffuse. Elongation  $57^\circ$ .

Seen no more in the west till the latter part of November.

To present at one view the various elongations from the sun, observed from Nov. 21st to May 10th, the result is as follows:—

1. Nov. 21st,	Elongation,	$90^\circ$	7. Feb. 7th,	Elongation,	$75^\circ$
2. Nov. 26th,	"	$100^\circ$	8. March 29th,	"	$60^\circ$
3. Dec. 2d,	"	$110^\circ$	9. April 6th,	Light rapidly fading.	
4. Dec. 18th,	"	$120^\circ$	10. May 1st,	Elongation,	$60^\circ$
5. Dec. 21st,	"	$90^\circ$	11. May 10th,	"	$57^\circ$
6. Dec. 28th,	"	$75^\circ$			

From this tabular view it appears that when the body first came into view, on the 21st of November, it extended about  $90^\circ$  eastward of the sun; that its elongation increased rapidly from this period, being five days afterwards  $100^\circ$ , in six days more  $110^\circ$ , and in fourteen days after this  $120^\circ$ , which is the greatest elongation I have ever noticed; and being at the same time about  $60^\circ$  westward of the sun, its whole extent in longitude was  $180^\circ$ .

I have, in a few instances, remarked what was apparently a sudden and remarkable expansion of the zodiacal light, a circumstance more than once noted by Cassini. My record for November 21st, 1838, is as follows:—

At 5 A. M., about 20 minutes before twilight, the zodiacal light was very large, extending in breadth from Corvus to Arcturus. Never saw it so broad before. More inclined towards the south than usual, its vertex passing one or two degrees to the south of Regulus.

Whether this extraordinary enlargement in breadth, implying a space of more than  $40^\circ$  was owing to a change in the body itself, or to some unusual atmospheric refraction, or the accidental presence of an aurora borealis, it is impossible for me to decide.

It is well known that the great French astronomer, Dominique Cassini, was the first to direct the attention of astronomers towards the zodiacal light, and that he made numerous observations on it extending from 1683 to 1688 inclusive, which are pub-

lished in the eighth volume of the Memoires of the French Academy, together with observations on the same phenomenon made at Geneva by a friend of his, M. Fatio. An elaborate digest of these records are made by Mairan in his celebrated Treatise on the Aurora Borealis, including also a few observations of his own, and of several other philosophers. It is interesting to compare these ancient observations with such as we have been able to make at corresponding times of the year; and having made this comparison in numerous instances, I feel able to say that *the zodiacal light, in the main, is the same thing that it was in the days of Cassini and Mairan*, being subject to similar variations at different seasons of the year and in different states of the atmosphere. I shall avail myself of such aid as I can obtain from this and every other source in the remaining parts of this essay.

#### NATURE AND CONSTITUTION OF THE ZODIACAL LIGHT.

1. *Length.*—The extreme portions of this body sometimes extend beyond the earth's orbit. It is obvious that, at an elongation of  $90^\circ$ , it must reach a tangent drawn to the earth's orbit at the place of the spectator; and if it reaches beyond that tangent, as is sometimes the case, it must of course extend beyond the earth's path. According to one of our observations, on the 18th December, 1837, its elongation was  $120^\circ$ .

The variable apparent elongation to which this phenomenon is subject is more or less influenced by three causes: the state of the atmosphere, the inclination of its line of direction to the horizon, and the length of the twilight. In order to eliminate the effect due to atmospheric changes we require numerous series of observations, continued through successive years, and, if possible, instituted at long intervals of time. The mean of such an assemblage of observations would exhibit results nearly free from the effects of accidental variations in the transparency of the atmosphere. Since the axis of the zodiacal light does not deviate far from the ecliptic, we may imagine it to be represented by a portion of that circle on the artificial globe, and we shall easily see that since its inclination to the horizon varies between twenty-five and seventy-two degrees, being twenty-five at the vernal equinox, (twenty-five degrees with the eastern and seventy-two degrees with the western horizon,) this cause must greatly affect the degree of intensity of the zodiacal light. The same must obviously be the case with the variations in the length of twilight, being an hour and a half after sunset at the vernal equinox, and two hours and a quarter after sunset at the summer solstice. But were these causes, combined, the only or the chief reason why the apparent elongation of the zodiacal light from the sun is greater at one time than at another, then, since at the vernal equinox the elevation above the horizon is at its maximum and



the duration of twilight at its minimum, the apparent elongation ought to be greatest of all; whereas it is then only  $60^\circ$ , while from the 21st of November to the 18th of December, 1837, we found it increase from  $90^\circ$  to  $120^\circ$ , and this at a season of the year when the elevation above the southern horizon is near its minimum, and the duration of twilight is longer than before. Nor is this an anomalous fact; the elongation has uniformly appeared greater in the west, during the months of December and January, than during March and April. Again, at the winter solstice the elevation is much greater in the morning than in the evening; but the light is far more conspicuous in the west than in the east.

2. *Direction.*—The general direction of the zodiacal light is, as its name imports, from the sun along the zodiac. Cassini and Mairan thought that its axis lay nearly or quite in the plane of the solar equator, making an angle with the ecliptic of seven and a quarter degrees; and, accordingly, that its nodes must be in the part of the ecliptic which the earth traverses in June and November. But Cassini himself remarked, that the direction of the axis is not always the same. On several occasions the vertex appeared to him to veer to the northward of its previous direction, so that, while it would at one time just *graze* Alpha Arietis on its northern border, shortly afterwards that star would be wholly within it. Before I had met with these statements in Cassini, I had several times remarked the same changes in the direction of the axis, the vertex sometimes lying in the ecliptic itself. Nor, as I think, will the observations warrant the conclusion that the axis of this body cuts the sun and consequently lies across the ecliptic in the plane of a great circle. On the 19th of January, 1835, the northern border was  $8^\circ$  south of Castor, and the vertex directed to a point south of the Pleiades. Consequently, its axis could not have been far from the ecliptic. But, on the 20th of March, the vertex reached above the Pleiades, and the axis had perceptibly veered northward from the ecliptic. These observations taken in connection with those of Cassini, indicate that the supposed relation of this body to the solar equator is not constant. In the year 1843, M. Houzeau published an article in the *Astronomische Nachrichten* in which he investigated the plane of symmetry of the zodiacal light from data derived from a comparison and digest of all the observations he could collect. He makes the inclination less than half that of the solar equator, and the place of the nodes of course quite different from that assigned to them by Cassini.

If then, as is demonstrated by Houzeau, the normal place of the axis gives it an inclination of only about  $3\frac{1}{2}^\circ$ , the great occasional deviations from this direction confirm our remark, that the course of the zodiacal light along the zodiac is not always the same but is subject to vary with the seasons of the year.

3. *Motions.*—The zodiacal light sometimes moves forward in the order of the signs, it is sometimes stationary among the stars, and sometimes retrograde. Beginning with morning observations in August, and noting its positions from day to day, we see it first stretching across the middle of the constellation of the Twins.\* The vertex moves slowly along through the constellations Gemini, Cancer, and Leo, being, on the 13th of November, a little east of Gamma Leonis,† having, in three months shifted its place eastward nearly three signs, and consequently nearly kept pace with the sun in its annual revolution, maintaining an average elongation from that body of 90 degrees. After the middle of November its light fades away in the east, its vertex becomes nearly stationary and of course its elongation westward of the sun diminishes, until the early part of January, when it is hardly visible at all in the morning sky. In the mean time, this light has been rapidly rising in the evening sky, and to this we will next direct our attention.

We have seen that about the 25th of November its upper portions reach beyond Capricornus, its vertex extending to the right arm of Aquarius. From this time it moves onward, sometimes more rapidly than the sun, but with an average elongation of 90°, until about the 24th of February, when it reaches a point a little south of the Pleiades. From the latter part of February, its progress eastward has seemed to me slower than before, hardly gaining one sign for the next three months, scarcely ever being distinctly visible beyond Castor, although neither the want of elevation above the southern horizon, nor the length of the twilight, would prevent its being seen beyond this if in reality it existed there. Finally, early in April it rapidly fades away, and soon after the first of May disappears altogether.

These facts respecting the zodiacal light are derived chiefly from my own observations, made and recorded at different times during the six years following 1833; but on comparing them with the observations of Cassini made towards 170 years ago, a near correspondence will be found between them; and the same will be the case if the comparison be made with the tabular view of observations collected from various authorities, as given by Houzeau in 1843.

In some cases, the apparent progress of this body through the signs corresponds so nearly to that of the sun, as to suggest the idea that it is something attached to the sun, and has an apparent motion due to the same cause, namely, the motion of the earth in its orbit. In other cases, however, its movements are too sudden and too unlike those of the sun to permit such a conclusion.

\* Above this point the light is blended with that of the milky way.

† Cassini placed it in 1686 at  $\alpha$  Leonis.

At one time, as we have seen, its elongation from the sun increases rapidly from  $90^{\circ}$  to  $120^{\circ}$ ; at other times it becomes for considerable periods stationary among the stars, and even retrograde; facts which seem to imply motions of its own independent of the sun and the earth; and such motions in any body thus situated, though they might be greatly modified by perspective, can hardly be any other than motions of revolution. On this subject La Place has the following remarks, at the end of his chapter "on the figure of the atmosphere of the sun."

(1.) "This atmosphere can extend no further than to the orbit of a planet, whose periodical revolution is performed in the same time as the sun's rotary motion about its axis, or in twenty-five days and a half. Therefore, it does not extend so far as the orbits of Mercury and Venus, and we know that the zodiacal light extends much beyond them.

(2.) "The ratio of the polar to the equatorial diameter of the solar atmosphere, cannot be less than two-thirds, and the zodiacal light appears under the form of a very flat lens, the apex of which is in the plane of the solar equator. Therefore, the fluid which reflects to us the zodiacal light, is not the atmosphere of the sun, and since it surrounds that body, it must revolve about it according to the same laws as the planets: perhaps this is the reason why its resistance to their motions is insensible."

4. *Material.*—The matter of which the zodiacal light is composed, presents many analogies to that of comets. In its visible form, in its direction with respect to the sun, in its very shade and color, in its increasing density towards the sun, in its transparency which, as in comets, is such as to permit small stars to be seen through almost every part of it: in all these respects we recognize a great resemblance between the zodiacal light and the tails of comets. We are at least authorized to say that it is a "nebulous body."

From all the foregoing considerations on the nature and constitution of the zodiacal light, we infer, then, that it is a nebulous body, revolving around the sun in an orbit but slightly inclined to the ecliptic.

I proposed finally to inquire *whether or not the zodiacal light is the origin of the meteoric showers of November and August, and especially those of November.*

It may be known to some present that after the great meteoric shower of November 13th, 1833, I published in the American Journal of Science some observations on the phenomena and causes of that remarkable exhibition of shooting stars, in which I came to the conclusion that they proceeded from a nebulous body revolving about the sun, and, at its aphelion, approaching very near to that part of the earth's orbit through which the earth passes on the 13th of November. At the conclusion of the



essay, I suggested the possibility that the zodiacal light might be the body in question. I was reluctant, however, to insist on such a connection, because the existence of the nebulous body was inferred from evidence wholly independent of the zodiacal light, and even before the zodiacal light was thought of. In fact, at that time I had very vague ideas respecting this light, as something that appears in the west after twilight about the time of the vernal equinox, but I did not even know that it was ever visible at the period of the year when the November meteors occurred; for at that time I had never read either the observations of Cassini on this body, or the treatise of Mairan on the Aurora Borealis, where so much is ascribed to its agency in the production of this latter phenomenon. Nearly twenty years have since elapsed, and I have had sufficient opportunity to observe the zodiacal light, and to reflect on the question of its possible connection with the meteoric showers of November and August. The result is, an increasing conviction of such a connection. I may here remark that the first idea of such an origin of the November meteors, is now generally ascribed by European writers to M. Biot. It may be proper, however, to state that the paper in which M. Biot first mentions the subject, is an essay read before the French Academy soon after the meteoric shower of November, 1836, three years after my paper was published. M. Biot does, indeed, favor the idea that these showers of meteors have their origin in the zodiacal light; but in noticing the views which I had published, respecting the cause of the meteoric shower, which he did me the honor to review at some length and in a manner very encouraging to myself, he distinctly stated that I had in my paper suggested the idea that the zodiacal light might possibly be the very nebulous body in question.

I am aware that the opinions I have formed differ widely from those entertained by many members of this Association, whose eminent talents and great success in the investigation of truth entitle them to the highest deference; but, should I fail of convincing them of the correctness of my views, I still indulge the hope that I may secure increased attention to a natural phenomenon, which appears to me to have important relations to our solar system, as well as to several of the most sublime and mysterious phenomena of nature.

In the paper which I published in the *American Journal of Science* in the year 1834, on the cause of the great meteoric shower of November 13th, 1833, I inferred the existence, in the planetary spaces, of a nebulous body revolving around the sun, the extreme portions of which on the 13th of November lay over or across the earth's orbit, in such a manner that the earth passed through it, or at least near enough to it to attract portions of it into its atmosphere, where they took fire and exhibited the

phenomena of shooting stars. As the leading steps by which I arrived at this conclusion, after an extensive induction of facts, were very brief and simple, I may be permitted to repeat them here. I argued thus: If all the meteors which fell on this occasion (which were in vast numbers, and some of them proved to be bodies of comparatively large size,) had been restored to their original position in space, they would of themselves have composed a nebulous body of considerable extent. But, since the same shower had been several times repeated without any apparent exhaustion of the nebulous body, it was inferred that only small portions of that body came down to us, such as constituted its extreme parts which approximated nearest to the earth; and various reasons induced the belief that the nebulous body itself was one of very great extent. It was a striking fact that the earth had, during several preceding years, fallen in with this body at exactly the same part of its orbit. Now, since it is impossible to suppose that a body thus situated, and consequently subject to the sun's attraction, could have remained at rest in that part of the earth's orbit while the earth was making its revolution around the sun, the conclusion was that the nebulous body itself has a revolution around the sun, and a period of its own. Since the earth and the body met for several successive years at the same point of the ecliptic, that period must obviously be either a year or less than a year. It could not be more than a year, for, in that case, the body would not have completed its revolution so as to meet the earth at the same point for successive years. Its period might be a year, and it might be less than a year provided the time was some aliquot part of a year, so as to make it revolve just twice or three times, &c., while the earth revolves once. The time being given we easily find the major axis of the orbit by Kepler's third law. On trying so short a period as one third of a year, it gives a major axis too short to reach from the sun to the earth, and hence it was inferred that the body could not have so short a period as four months, since it would never in that case reach the earth's orbit, even at its aphelion. A period of six months was found to be sufficient, and this was accordingly assumed at first to be the time, although the possibility that the period might be a year was distinctly admitted. But, extensive as I even then believed the nebulous body to be, I had formed very inadequate notions of its real extent, for this may clearly be sufficient to reach from the sun to the earth, and thus to correspond in dimensions to the zodiacal light; and since the center of gravity of this body may be far within the earth's orbit, so its orbit may, even at its aphelion, be distant from the earth, and yet the extreme portions of the body may reach beyond the ecliptic. It would, therefore, be entirely consistent with my original views, to assign to a nebulous body of such an extent as

that of the zodiacal light, a period as short as one third of a year, or even less.

I do not assert positively that the zodiacal light is the veritable body which produces the meteoric showers of November and August. Before such an hypothesis can be proved to be true or false, with certainty, a greater number of precise observations continued through a series of years, would require to be made, and a careful comparison instituted between the hypothesis and the facts. Should the zodiacal light be found at last incompetent to explain the periodical meteors, the existence of a nebulous body, as inferred from a full survey of the facts in the case of the meteoric shower of November 13th, 1833, independently of all hypothesis, will still be true. But, with great deference, I submit to the Association, the following *presumptions* in favor of the opinion that the zodiacal light is the nebulous body which produces the meteoric showers of November.

1. The zodiacal light, as we have found in our inquiry into its nature and constitution, is a *nebulous body*.

2. It has a *revolution* around the sun.

3. It reaches beyond and *lies over the earth's orbit*, at the time of the November meteors, and makes but a small angle with the ecliptic.

4. Like the "nebulous body," its periodic time is *commensurable* with that of the earth, so as to perform a certain whole number of revolutions while the earth performs one, and thus to complete the cycle in one year, at the end of which the zodiacal light and the earth return to the same relative position in space. This necessarily follows from the fact that at the same season of the year it occupies the same position one year with another, and the same now as when Cassini made his observations nearly one hundred and seventy years ago.\*

5. In the meteoric showers of November, *the meteors are actually seen to come from the extreme portions of the zodiacal light*, or rather a little beyond the visible portions; and the same was true of the radiant point of the meteors, (when watched, as it was by Mr. Fitch,† from Oct. 16th to Nov. 13th, 1837,) namely, that the radiant always keeps the same relative position with respect to the vertex of the zodiacal light, being with that vertex in Gemini, in the month of October, and travelling along with it through the Constellation Cancer, and into Leo, where it was on the morning of the meteoric shower. Observations, so far as they have been made, indicate a similar relation between the meteors of August and the extreme portions of the zodiacal light.

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\* For the first suggestion of this analogy, I am indebted to one of my former pupils, Mr. Hubert Newton.

† Amer. Jour. Sci., xxxiii, 386.



These five propositions I offer as so many *facts* established by observation. Most of them appear in the original paper of Cassini on the zodiacal light; others may be seen in the tabular collection by Houzeau of all the known observations made at different periods; a few, not noted by others, have been added by myself. For the inferences here made respecting the connexion of this body with the periodical meteors, I alone am responsible.

AN ATTEMPT TO SHOW  
THAT  
LIGHT, HEAT, ELECTRICITY, AND MAGNETISM  
ARE  
EFFECTS  
OF THE  
LAW OF GRAVITATION.

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BY W. CLAY WALLACE, M. D.

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# LIGHT, HEAT, ELECTRICITY AND MAGNETISM.

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Light, Heat, Electricity, and Magnetism have been called Imponderable Bodies, although there are no facts to establish that they are material. There may be aqueous or other vesicles filled with, and floating in the air, without essentially altering its constitution; but the existence of *bodies* which cannot be detected either by their own weight or chemical union with others, is incompatible with known laws. If it can be shown that the effects are produced by disturbance of the equilibrium of bodies, by change of density and distance, the explanation of the phenomena will be simplified by being brought down to one cause.

In order to be understood, repetitions and assertions of what is mere hypothesis could not well be avoided; yet it is not more problematical to account for the law of interference of light by the supposition of a non-polarized cell of that which we can confine in a jar, and weigh, and analyze, than by the confluence of the waves of an imaginary ether; and it is not more problematical to explain by the same cell the elevation on both sides of a perforated card, than by the presumed existence of two imponderable fluids.

In the following attempt, we shall treat the subject in inverse order, from the common classification, by commencing with Magnetism.

*Magnetism.*—If the sun and planets be taken as an illustration of a system formed by immense masses of matter, the organized cell may be regarded as an example of those which are minute. In accordance with the law that matter is attracted towards matter with force proportionate to the square of the distance, bodies on the surface of the earth are most forcibly attracted to its own centre, yet they are also attracted by the matter of the sun, the moon, and the atmosphere. Owing to gravitation and the mobility of its walls, the organized cell, unable to maintain a spherical, assumes an ovoid form. A number of cells, if placed in a tube, would present their greater hemispheres towards the earth, and the lower cells become compressed

in proportion to the weight above them. It is presumed that the moment the tube is turned in a perpendicular direction, the cell nearest the earth is first and most powerfully attracted, and that the ovoids are formed in regular order, but with such inconceivable rapidity that the eye cannot detect the period of successive formation.

If, as the granules of a nerve are enveloped by neurilema, we could arrange the cells in a tube of nearly the same diameter with themselves, and so adhering to its walls that no change of linear position could be effected, all would become ovoid, and present the greater hemisphere towards the principal attracting mass.

A boy's sucker applied to a stone affords a familiar illustration of the principle that the gravitation of the stone can be overcome by cohesion, or attraction, at insensible distances. If, therefore, a cohesive force exerted at one extremity of the tube we have supposed, causes a succession of changes in the forms of the cells, we may account for cohesive attraction at the other extremity.

The egg of the domestic fowl is an enlarged example of the cell we have been considering. Before the addition of the shell, it has acquired the usual shape by gravitation, but after being covered it becomes permanently polarized. For reasons now to be given, we shall call the greater hemisphere the north, and the smaller the south pole. If the egg were placed longitudinally on a pivot, the greater hemisphere would preponderate, and if without change of position the contents could be drawn the other way, the effect would be reversed. On the inequality of the hemispheres of primitive cells so disturbing the equilibrium between the earth and the air, that their mutual attractions become greater in one place and less in another, our theory is founded.

From the granular structure of iron, it is inferred that the inorganic matter is arranged in cells as described, with the greater hemispheres directed towards the greater attracting power. A bar of this metal, when held perpendicularly, becomes a magnet; the lower end behaving as a north, and the other end as a south pole. The magnetism thus induced is temporary, but after hammering, the acquired polarity is not altered by change of position. In the latter case the successive molecular changes produced by attraction at one extremity, cannot be so easily communicated to the other; hence a slight change of molecular arrangement diminishes or prevents the power of transmission. In this way it is presumed the particles of water conduct impressions when fluid, but when changed into ice, the cells become permanently polarized, and do not conduct them; in this way, too,

amorphous carbon is a conductor, but when crystallized, it becomes a non-conductor.

In afterwards referring to conductors and the electric current, we do not mean to express the idea of a stream of an imponderable fluid passing through a cohering body, but successive molecular changes in the conductor, effected by cohesive attraction at one terminus. The change of condition in the conductor influences both air and earth, and when the termini touch the earth the metallic particles and the ground beneath them resume their natural positions: by retrograde motion both lines arrive at the original point of disturbance, and leave all behind them as before. Although not a fair example, transmission may be illustrated by placing a row of books on end, and some distance apart, in the way that boys amuse themselves at school. When the first book of the row is thrown down, all the rest fall in regular succession; and if we now suppose that gravitation could be counterbalanced by elastic springs representing the air, the books would resume their first positions. Excited conductors may be compared to soft iron magnets, in which the power is greater in proportion to the length of the rod, and when communication is effected by transmission from cell to cell, as in a galvanic trough wrought by innumerable air-pumps, or vacua produced by the oxydation of zinc, the effects become intense. As when one bar of iron is placed upon another, the lower end of the uppermost bar presents a north, and the upper end of that which is in communication with the earth a south pole, it is inferred that iron is always magnetic by position.

The air or other medium in which a magnet is placed, acquires polarity similar to that of the metal itself, and is arranged in curves, the direction of which may be seen by iron filings, or still better, by black lead sprinkled on a plate of glass laid over the magnet. The curves proceed from foci at the termini of the metal, and advancing from cell to cell, increase in diameter with the distance from the terminus. The air around a magnet may be compared to a number of cut elastic rings, arranged with their convex surfaces to each other; the middle third tied up in a straight bundle, and the loose termini forming curves in straining to be restored to their original condition. The air around a magnet may also be compared to a positively or negatively electrized bundle of hair, which diverges by the attraction of the atmosphere, and under favorable circumstances bends, to restore the equilibrium or complete the circuit.

The influence of the atmosphere on bodies polarized in a certain manner, may be illustrated by one of Prince Rupert's drops, which are prepared by pouring melted glass into water. The water being of



a lower temperature, the outer surface of the glass is immediately congealed. The north poles of the cells of the glass will, on cooling, be directed to the particles of the circumference, and the centre of gravity of the mass will present a point of no attraction. When the surface is broken, the particles of the glass being attracted by the air, fly off in all directions; an explosion ensues, and the mass becomes dust. In like manner, when the south poles of two magnets are brought near each other, the north poles of the air cells are presented to the iron, and as there is no central point of attraction at the meeting of the lines proceeding from each magnet, the metals drawn by the surrounding atmosphere recede from each other.



Both cases may be illustrated by Faraday's experiment of the electrized muslin bag, the fibres of which diverge equally, whether positively or negatively excited. No attractive force within the cone can be discovered by the electrometer; and when, by silk threads, the bag is turned inside out, the effects are precisely the same.

The earth, as has been stated, becomes ant-arctic to the bar of iron, yet the earth itself becomes arctic to the sun, and zinc becomes arctic to platinum, although it is ant-arctic to oxygen; it is therefore presumed, that when two north poles are brought near, the iron attracts the north poles of the aircells, a negative centre is formed as before, and similar results follow.



When opposite poles are placed near each other, the south pole of the one magnet influences the air cell in contact with the north pole of the other, and the north pole in its turn influences the air cell attached to the south pole of the other, with force proportionate to the distance, so that neutralized by opposing forces, one of the cells at or near the centre of the intervening line presents a new centre of gravity, to which the magnets are not only attracted themselves, but impelled by the surrounding atmosphere.



*North Pole.*—As by the coil theory, the magnetic poles would be placed at the axes of the atmosphere, we shall attempt another, and endeavor to give reasons why one pole of the mariner's compass should be directed to the northern rather than the southern hemisphere.

We have seen that polarity is affected by comparative density, and that the denser body is generally ant-arctic. As land is denser than water, we might expect that the centre of gravity of its surface would, as in other cases, present a south pole. In order to determine the position of this centre, it is necessary to take into account inequality of attraction by difference of elevation, and the effect of vegetation in increasing the extent of surface. By looking at the map, we perceive that most of the land exists in the northern hemisphere, and that south of the magnetic equator there is scarcely one-half of South America, one-third of Africa, and the continent of New Holland. On the eastern hemisphere there are many unproductive surfaces, such as the African and the Arabian deserts, the steppes of Tartary, &c. whereas on the western the foliage is luxuriant, and the land projects farther south. On the eastern hemisphere the woods have been cut down to allow for its millions of inhabitants, the cultivation of food, the growth of which causes an easterly deviation of the compass from May to the summer solstice, when the grain ripens and is cut down; whereas, on the western the forests far exceed the area of the land cultivated for the support of its comparatively sparse population. The location of the magnetic pole on the western side of the axis of the earth might thus be inferred from theory, if it were not pointed out by the compass. •

*Electricity.*—All the elementary forms of matter, with which we are acquainted, differ in specific gravity; yet the same element may, by becoming porous, crystallized, or chemically combined, be rendered specifically lighter or denser; thus the specific gravity of silix, when it constitutes the yellow covering of straw, is different from that of quartz. The enamel of the teeth, and the petrous portion of the temporal bone, are denser than other osseous structures. Even scratching the surface, or coating one metal with a thin film of another, essentially alters the behavior of bodies in producing electrical phenomena.

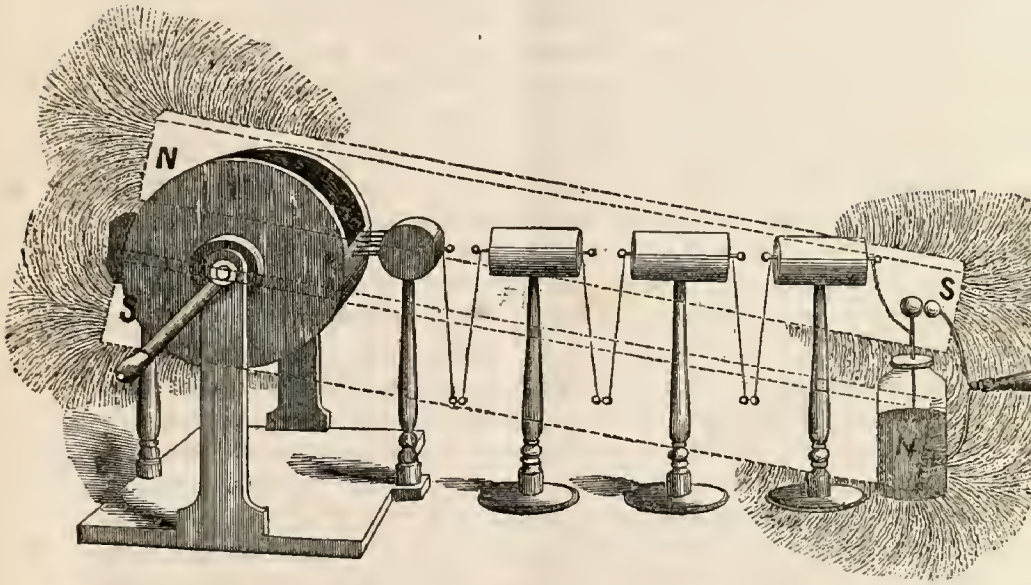
We have seen that the lower end of the rod of iron presents a

north pole when directed to the ground. If we were to place the rod on another bar of this metal, directed in the same way, and then separate them, the lower end of the former would still present a north and the upper end of the latter a south pole. Let us now compare two bodies of unequal density, such as zinc and platinum, with the iron and the earth. The outer cells of the zinc being drawn from the centre of gravity by the denser mass of the platinum, should, if metals are governed by similar laws, present the north pole, and the opposite cells of the platinum, being only partially drawn out by the weaker metal, should present the south pole, just as the cells of the ground or those of the upper portion of the iron rod are elevated by the weaker attraction of ██████ the atmosphere. If the platinum be roughened, pointed or made porous, so as to produce unequal distances between the metals, or, so to speak, to allow the attractive forces to proceed from foci, the effects are increased. Glass when rubbed with any body presenting unequal distances, such as silk, feathers, &c. or with mercurial amalgam, becomes on the same principle in an arctic condition, and attracts bodies repelled by sealing-wax; whereas sealing wax treated in a similar manner becomes ant-arctic, and attracts bodies repelled by glass. Both have become magnets, but owing to the polar arrangement of the one, and viscosity of the walls of the other, the cells do not possess that freedom by which metallic cells are permitted to alter and arrange their poles. When a magnetic needle is presented to excited glass or sealing-wax, the needle is attracted without discrimination of pole, because the air around the excited body, by becoming similarly affected, prevents that nice atmospheric balance on which magnetism depends. It may be shown by experiment that under fair circumstances the needle will be not only attracted to the excited body, but the excited body itself will, if permitted, point due north and south.

When the cylinder or the plate of the electrical machine is turned round, the glass becomes arctic and the rubber ant-arctic. The cells of bodies in the air and those on the ground may be compared to the conducting wires from a pair of galvanic plates, the termini of which are in opposite polar states; or they may be compared to those of two curved magnets placed above and below the machine, the south pole of the one representing the cushion, and the north pole of the other the electrified glass. As the pole of a magnet changes at a short distance from the commencement, south poles are presented at the teeth of the prime conductor, by which the north poles of the metallic cells are collected in foci. A series of insulated cylinders placed near the prime conductor become similarly affected, and resemble as



many magnets pointing in one direction, and attracting bodies which have acquired opposite polarity. When a Leyden jar is connected



with one of these cylinders, the lining tinfoil becomes in a like condition, and electrizes the inner surface of the glass. The inner and the outer coatings of the jar bear to each other a relation resembling the cylinders; for if the jar were placed horizontally on a glass pillar, and the outer covering turned inside out and also insulated, two additional conductors might be added to the series. When suspended in air, which is an imperfect conductor, the jar can be only moderately electrized; but when the outer surface is in communication with the ground, the inner surface can be highly charged. Although glass, by reason of the unchangeable polarity of its walls, prevents restoration of equilibrium, it presents no obstacle to the attraction of bodies beyond it, as may be illustrated by rubbing one side of a piece of window glass and placing light bodies on the other side, and also by the divergence of the electroscope in a vacuum. The coatings of the jar may be regarded as the termini of the magnets with which we commenced the comparison; the outer coating or terminus of the cells of the earth being now in an arctic, and the inner coating in an ant-arctic condition.

In an arctic condition water is to zinc as zinc is to platinum. In accordance with this principle, when a plate of each of these metals is

immersed in an acid solution, the oxygen or arctic portion of the water cell is attracted by the zinc, which in its turn is attracted by the platinum, and by the opposing forces the elements are made to embrace each other. The disunited hydrogen combines with the oxygen of the neighboring cell, and the change of associates continues until it reaches the platinum, where it is set at liberty, because the metal, being in an ant-arctic condition, is more attracted to its own mass than to a foreign body. The faces of the metals having by relative attraction become as two magnets placed north and south, their backs attracted proportionately by the air, acquire opposite polarity, and in this way the poles at the termini of the battery are reversed. The oxygen of the water in which the wires from the opposite plates are immersed, is set at liberty at the platinum, and the hydrogen at the zinc terminus. The Magnetic, the Leyden and the Galvanic cells bear a close resemblance to each other, and show that Faraday's method of constructing the latter is superior to other arrangements.

Mr. Sturgeon has shown that for the production of these phenomena different metals are not necessary, as all the effects can be exhibited by using a plate of cast zinc, and increasing the density of another plate of the same metal by hammering. Even a single piece of zinc exhibits some of the effects related, as during solution the lower portion becomes arctic and unites with oxygen, whereas the upper portion becomes ant-arctic, and gives off hydrogen.

It was discovered by Doberciner that when a stream of hydrogen was passed through porous platinum, the metal became red hot and the hydrogen inflamed. Water is formed by the separation of the oxygen of one of the air cells in the spongy mass, and its union with hydrogen, and the protoxide of azote is produced by the decomposition of another. When a mixture of oxygen and hydrogen gases is detonated, the condensation of the elements thus drawn together is so rapid, that a violent explosion is produced by the action and reaction of the surrounding air. In like manner by the chemical union of the constituents of the cells contained within the porous platinum, vacuum after vacuum is produced, and a current of air established to supply the place of the dissipated elements.

*Heat.*—As the disturbance of equilibrium necessary for the production of flame is great, combustion cannot take place when the temperature is diminished below the proper point, as may be illustrated by Davy's safety lamp. It would be unfair, therefore, to expect that all the phenomena of light and heat would be produced by low degrees of inequality of attraction.

When porous carbon is placed in a common fire-place, a certain

amount of disturbance of the equilibrium between the earth and the atmosphere is necessary for the union of the carbon with the oxygen of the air, just as we bring metals to a certain temperature before making an alloy. The oxygen is then attracted by the denser carbon, a current of air is established, and the dobereiner battery feeds itself. The wick in a candle and the coals in a fire burn more brightly after part of the carbon has been consumed and a porous crust been formed.

When the pores of the dobereiner are large, as in the lungs, the relative attractions are diminished, and being restrained by the continuous flow of the blood, the temperature does not extend beyond a certain degree. Here, also, disturbance of equilibrium is required to commence and continue the process.

When the pores of the dobereiner are of minute description, as in carbonate of lime, or semi-burned alumina, the combustion of a mixed stream of oxygen and hydrogen gases is complete, by being accomplished by little at a time, and in rapid succession. The combustion thus effected by continuous chemical action, polarizes the air to a great distance, and the light is so intense that it can scarcely be borne by the unprotected eye. When the poles of an excited galvanic battery are brought near each other in common air, chemical action ensues; some of the nitrogen is set free, and protoxide of nitrogen is formed with the sensations of heat and light, as related.

By disturbance of the equilibrium thus produced, the cells of the wire will, by attraction of the atmosphere, recede from each other and become expanded. This is the law of expansion.

By continuing the experiment, the metal falls down in drops. This is the law of fluidity.

Any vaporisable, solid, or fluid body placed at the poles, assumes the form of vapor. This is the law of vaporisation.

A new centre being formed by placing a red hot ball or a piece of ice in the focus of a parabolic mirror, the cells of the polished metal acquire new polarity, which may be communicated to another mirror placed at a distance, in the focus of which the air in a thermometer will be expanded or contracted. This is the law of radiation.

We have gone on the theory that every collection of matter represents the universe, consequently every globule of mereury resembles a world, the cells of which are not only attracted to their own centres but to the centre of the mass to which they belong. When the counter-attraction of the atmosphère is diminished, attraction to these double centres is increased and the volume becomes lessened. On this principle the thermometer falls when placed in recently rarified air, whereas it rises in a combination of sulphuric



acid and water, because the specific gravity of the latter mixture is greater than the mean. On the same principle also, the summits of lofty mountains are covered with snow, because there is less atmosphere above them to keep the particles so far asunder as to constitute fluidity; and when we descend from the surface of the ground the temperature increases, notwithstanding the unproved assertion that we are treading over molten lava; and on the same principle bodies which are solid at the poles, are fluid at the equator, because there the attractive force of the atmosphere is greater, on account of its greater diameter, the effect of which is counterbalanced by the increased distance between the surface and the centre of the earth.

It has been repeated that when the attraction of the air is diminished, central attraction is increased. Were water the fluid evaporated, the water cells would be compressed into a fluid when the equilibrium is restored. It is possible that at the commencement of fluidity the water cells are so arranged that they form hollow spheres, as it was observed by Saussure that the mist by which he was surrounded on Mount Blanc was formed of aqueous vesicles. If the vesicles were broken, the water cells would be drawn together and a new centre of attraction would be formed. It is also possible that when the atmospheric attraction is diminished to  $40^{\circ}$ , gravitation is counterbalanced by the breaking up of the cells into smaller spheres; and by the new attraction of each cellule to its own centre, the mass expands and acquires greater fluidity. On further diminution of temperature the cellules acquire a solid form, and, crystallizing into ice, float on the surface of the liquid. In the same way sulphur on cooling becomes thick at a certain temperature, it then acquires greater fluidity, and again expands on crystallizing; and iron and antimony, the most easily polarized of the metals, expand when becoming solid.

Having briefly noticed the effects produced by the action of unequal masses of matter on each other at unequal distances, we shall now make a few remarks on their arrangement.

- I. The Earth and the Sun . . . *Geoheliac Battery.*
- II. The Earth and the Moon . . . *Geoselenic Battery.*
- III. The Northern and the Southern Hemisphere . . . . . *Austro-Boreal Battery.*
- IV. The Earth and the Atmosphere . . . *Geopneumatic Battery.*
- V. Unequal Terrene Bodies . . . *Allobarytic Battery.*

The two last may be subdivided into those by which change of polarity is effected by alteration of—

1. Position; *Gilbert's Battery.* 2. Temperature and State; *a.*

*Seebeck's Battery* ; *b. Armstrong's Battery*. 3. Chemical Constitution ; *a. Dobereiner* ; *b. Galvanic* ; *c. Botanic* ; *d. Zoonic Batteries*.

Some of these have been already noticed, and others will be more appropriately mentioned when considering Light.

*Austro-Boreal Battery*.—Although the surfaces of land and water are only edge to edge, yet the columns of air arising from them being face to face may, when in opposite conditions, give rise to the storms which occur with most violence at what might be considered favorable localities, such as the inter-tropical islands and the capes.

When any irregularly formed body is balanced on a pivot on the prime conductor of a machine, it moves in a circle with velocity proportionate to the extent of electrization ; and by the rarefaction of the surrounding air, thermometer and barometer fall. As there is mutual attraction when currents move in one direction, and repulsion when in opposite directions, a cloud even moderately electrized, finding those around it in a proper condition, and moving the same way, may by adding to its bulk and force, become a storm, sweeping over the sea until it reaches the columns from the land, which revolving in a different direction, keep the tempest at a distance by a force as powerful as its own.

*Botanic Battery*.—Every tree may be considered a galvanic battery, with its leaves forming the divisions of the trough, the poles of which are the branches communicating with the air and the roots penetrating the ground. As the metal in electro-plating is set free from combination, and deposited at the south pole of a battery, the carbon contained in the atmosphere is deposited on and constitutes a part of the leaves, which electrized by the sun, fulfil the double office of lungs and heart.

Two microscopic plates of unequal power become a germ or battery, which converts the starch of the grain into sugar by digestion, and appropriates the prepared nutriment to its own growth. After this process, analogous to the incubation or gestation of animals, is accomplished, the plant, obliged to provide for itself, sends roots into the soil and extracts other materials, which it previously prepares, by acting on a stomach exterior to its body.

There are not yet sufficient facts to explain why it is that when a vessel of water is placed within fifteen inches of a gourd, a leaf of the plant will be found floating on the water on the ensuing day ? or how it is that a scarlet runner will approach a pole, placed at the distance of a foot or more, then follow the pole when moved in an opposite direction, and die if the change of position be too frequent ?

Many of the effects which are usually attributed to inorganic chemistry, are in reality produced by vital functions; thus vinous fermentation is effected by a minute fungus which, growing with great rapidity, evolves carbonic acid and converts a portion of the saccharine fluid into alcohol. By fungi of one description certain fluids may be converted into vinegar, and by those of another they may become putrid. Thus decay is not effected by any immediate alteration of chemical composition, but by myriads of minute batteries evolving gases at their tiny poles, and restoring the body to its original elements.

Diseases, both of plants and animals, are for the most part caused by vegetable or animal parasites; thus the ergot of rye is a vegetable, and the curculio of wheat an animal. The muscardine of the silkworm is produced by a cryptogamic plant which grows on stones, yet grows also in the intestinal canal, and by causing it to rot, the animal dies. The aphthæ of children are fungi, a portion of which taken from the mouth of one child may be propagated on the surface of milk in a vessel, and afterwards communicated to another child.

Most eruptive diseases run a certain course and reproduce their own species, according to the laws of organic life. The viscera of the body are not exempt from foreign ravages, as tubercles, cancers, &c. may infect the lungs, the brain, or the liver. As most fungi thrive best in the dark and die when exposed to the sun, so morbid parasites are often found in animals, which, created for exposure, are kept in the shade; thus stall-fed oxen are seldom free from diseases of the lungs, whereas those which roam in the fields are generally healthy. Among those who dwell on the shady side of the Alps, scrofula and mental imbecility occur in the worst forms, yet the individuals who dwell on the other side of the mountains are the healthiest of the human race. Although it is apparent that consumption is most frequent among those who are kept within doors, the thoughtlessness of those who submit their children to the modern system of education, by which they are confined for hours during the choicest part of the day, is remarkable. A potato or other plant growing in a room through which there may be a current of the purest air, is pale and easily broken; but let it be brought to the light and it becomes strong and of a natural color. If children were regarded as plants, and when old enough judiciously exposed to a moderately bright sky, they would acquire firmer constitutions.

*Zoonic Batteries.*—Many years ago we published an account of the resemblance of the brain to a Faraday's battery, and of some of the fibres proceeding from the inner plate to poles passing through



the coil of the pons Varolii. Besides the brain there are other galvanic arrangements for the functions of life, the principal of which are the lungs and the ganglionic system of nerves.

The apparatus for digestion is arranged for normal preparations, yet irregularities sometimes occur, such as the formation of sugar, which, entering the blood and passing off by the kidneys, produces diabetes; of hydrochloric acid producing dyspepsia; lactic acid, rheumatism; lithic acid, gout, &c.

The theory advanced explains the phenomena of thermo-electricity, electro-magnetism, magneto-electricity, and double refraction, the consideration of which is deferred, as in this short essay it is only intended to make some remarks on a suggestion of Sir Humphrey Davy.

*Light.*—We talk of sound, not as a material agent, but as the name of a sensation produced by vibrations of the air. In like manner light can only be regarded as the name of a sensation effected by disturbance of equilibrium of the media through which it is perceived. The retina is formed by a concave daguerreotype plate, composed of nervous matter arranged in a beautiful mosaic. Anterior to and terminating on the plate there is a series of transparent fibres, which after leaving the eyeball are collected into a bundle which proceeds to the brain. Impressions on the concave plate are conveyed by this bundle, or optic nerve, as a message is conveyed to a distant spot by the electro-magnetic telegraph; but how that which is immaterial perceives the material cannot be conjectured. If the changes produced on the retina by surrounding objects occasion corresponding changes at the tubercula quadrigemina, what we have seen may be permanently recorded as the marks on the paper at the termination of a telegraph, or the corpora lutea on the ovarium.

When the sun appears above the horizon, the cells of the atmosphere and those of the earth are polarised by more direct exposure to the attraction of the immense mass of matter. The polarity is communicated along the ground to bodies in the shade, and the air becomes electrized in all directions. When parallel lines of polarised air cells fall on the cornea, the transparent media are similarly affected, and a picture is formed on the retina where the first circuit is completed. At the completion of the circuit, as at the poles of a battery, chemical changes occur which increase the polarity of the nerve cells, until the latter reach the tubercula quadrigemina, where the telegraphic lines terminate. Thus vision is affected by a double galvanic circuit, the application of which might be useful in art, as

Dolland improved optical instruments by imitating the structure of the crystalline lens.

When a lighted candle is brought into a dark room, the do-bereiner battery of the wick polarises the air and the surfaces of the walls; the polarity passes to the ground and up the body to the eye, where the circuit is completed and vision effected as before.

When a beam of highly polarised air, which is aretic to the polarising body and ant-arctic to that on which it impinges, falls on a triangular prism, the glass becomes similarly affected, and in its turn polarises the air beyond it, but not in the same degree, for the lines of air cells are influenced by the unequal length of the glass, and by the size of the angles made by them and the outline of the prism. These unequally polarised lines of air cells or rays, which may be compared to what are called primary, secondary, tertiary, &c., currents of electricity, have different chemical effects. When ant-arctic, as at the blue portion of the spectrum and beyond it, there is analysis, as at the zinc pole of a battery moderately excited; when more aretic, as at the red, there is synthesis, as at the platinum pole; whereas, between the two, as at the yellow, the chemical effects are feeble, but there is more illuminating power, because the force of the surrounding air lines is greater, as iron placed within a coil makes the poles more energetic.

The cooling or heating effects of the poles of a moderately excited battery, may be illustrated by an experiment described in Davis's *Manual of Magnetism*. When thermometers are placed at the junctions of a bar of antimony with two bars of bismuth and electrized, the thermometer at the ant-arctic or zinc junction falls, and that at the aretic or platinum junction rises to a proportionably greater extent than the depression. It is presumed that the heating and cooling properties of the extreme ends of the spectrum, may be explained in the same way.

By intermittent attraction all the sensations are effected. The forms of solids are ascertained by touch, the qualities of fluids and vapor by taste and smell; by the undulations of the atmosphere we hear, and by its polarity we see; yet as far as matter is concerned, the falling of an apple explains them all. Irritation of the optic nerve produces only light, of a branch of the fifth pair only pain; each has its own function, and one will not supply the place of the other. Some nerves are chemists and architects to make proper selections from the raw material to construct and repair; some are watchmen

divided into those who give intelligence from without, and into those who give intelligence from within ; and some obey the orders of the will. All are servants, who work together in such harmony that in health we are unconscious of their existence. But how it is that all the varieties of color proceed from the polarity of the air effected by the angles of elevation of the cells of the visible object ; how it is that vibrations of the atmosphere, made in given time, produce all the varieties of tone which madden or soothe ; or how it is that by the varieties of odor or taste, we can distinguish the useful from the noxious, is, and will remain a mystery.

We have endeavored to show that in all nature individuals are attracted to families, families to circles, circles to the state, the state to the union, the union to the world, and the world to the universe. There is not a word that we utter or a gesture we make which does not influence the harmony of our locality ; and there is not a whisper of the wind or the falling of a leaf which does not influence the harmony of the surrounding elements. Without attraction the seed would not spring to cover the ground with green ; the flower would not blossom or be covered with dew ; the fruit would not ripen, and the tree would not wither. Without attraction the lungs might be filled with air, but it would afford no heat ; the digestive organs might be supplied with food, but it would yield no nourishment, and the current of life would cease to flow. As there would be no birth there would be no death ; there would be neither growth nor decay. If we have proved that light is attraction, then attraction, as is recorded, was the first-born of Heaven.





*from the author*

ESSAY

ON

CANONICAL FORMS.

BY

J. J. SYLVESTER, M.A. F.R.S.

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# SKETCH OF A MEMOIR ON ELIMINATION AND TRANSFORMATION.

By J. J. SYLVESTER, M.A., F.R.S.

[Extracted from the *Cambridge & Dublin Mathematical Journal*, May 1851.]

THERE exists a peculiar system of analytical logic, founded upon the properties of zero, whereby, from dependencies of equations, transition may be made to the relations of functional forms, and *vice versâ*: this I call the logic of characteristics.

The resultant of a given system of homogeneous equations of as many variables, is the function whose nullity implies and is implied by the possibility of their co-existence, *i.e.* is the characteristic of such possibility; but inasmuch as any numerical product of any power of a characteristic is itself an equivalent characteristic, in order to give definiteness to the notion of a resultant, it must further be restricted to signify the characteristic taken in the *lowest form* of which it *in general* admits.

The following very general and important proposition for the change of the independent variables in the process of elimination, is an immediate consequence of the doctrine of characteristics.

Let there be two sets of homogeneous forms of function;

the 1<sup>st</sup>,  $\phi_1, \phi_2 \dots \phi_n$ ,

the 2<sup>nd</sup>,  $\psi_1, \psi_2 \dots \psi_n$ .

Let the results of applying these forms to any sets of (*n*) variables be called

$(\phi_1), (\phi_2) \dots (\phi_n),$

$(\psi_1), (\psi_2) \dots (\psi_n);$

then will the resultant (in respect to those variables) of

$$\begin{aligned} \phi_1 \{(\psi_1), (\psi_2) \dots (\psi_n)\}, \\ \phi_2 \{(\psi_1), (\psi_2) \dots (\psi_n)\}, \\ \vdots \\ \phi_n \{(\psi_1), (\psi_2) \dots (\psi_n)\}, \end{aligned}$$

be the product of powers (assignable by the law of homogeneity) of the separate resultants of the two systems,

$$\begin{aligned} \{(\phi_1), (\phi_2) \dots (\phi_n)\}, \\ \{(\psi_1), (\psi_2) \dots (\psi_n)\}. \end{aligned}$$

By means of the doctrine of characteristics the following general problem may be resolved.

Given any number of functions of as many letters, and an inferior number of functions of the same inferior number of letters, obtained by combining, *inter se*, in a known manner, the given functions, to determine the factor by which, the resultant of the reduced system being divided, the resultant of the original system may be obtained.

If in the theorem for the change of the independent variables both sets of forms of functions be taken linear, we obtain the common rule for the multiplication of determinants: if we take one set linear and the other not, we deduce two rules, viz. That the resultant of a given set of functional forms of a given set of variables, enters as a factor into the resultant,

1<sup>st</sup>, of linear functions of the given functions of the given variables;

2<sup>nd</sup>, of the given functions of linear functions of the given variables:

the extraneous factor in each case being a power of what may be conveniently termed the *modulus of transformation*, i. e. the resultant of the imported linear forms of functions.

From the second of these rules we obtain the law first stated I believe for functions beyond the second degree by Mr. Boole, to wit, that the determinant of any homogeneous algebraical function (meaning thereby the resultant of its first partial differential coefficients) is unaltered by any linear transformations of the variables, except so far as regards the introduction of a power of the modulus of transformation. This is also abundantly apparent from the fact, that the nullity of

such determinant implies an immutable, *i.e.* a fixed and inherent, property of a certain corresponding geometrical locus.

There exist (as is now well known) other functions besides the determinant, called by their discoverer (Mr. Cayley) hyperdeterminants, gifted with a similar property of immutability. I have discovered a process for finding hyperdeterminants of functions of any degree of any number of letters, by means of a process of Compound Permutation. All Mr. Cayley's forms for functions of two letters may be obtained in this manner by the aid of one of the two processes (to wit, that one which will hereafter be called the derivational process,) for passing from immutable constants to immutable forms. Such constants and forms, derived from given forms, may be best termed adjunctive; a term slightly varied from that employed by M. Hermite in a more restricted sense.

The two processes alluded to may be termed respectively appositional and derivational. The appositional is founded upon the properties of the binary function  $x\xi + y\eta + z\zeta + \dots$ ; in which, whether we substitute linear functions of  $x, y, z$ , &c., or linear functions of  $\xi, \eta, \zeta$ , &c., in place of  $x, y, z$ , &c., or  $\xi, \eta, \zeta$ , &c., the result is the same.

Consequently, if we apply the form  $\phi$  to  $\xi, \eta, \dots \zeta$ , and take any constant (in respect to  $\xi, \eta, \dots \zeta$ ) adjunctive to

$$\phi(\xi, \eta, \dots \zeta) + (x\xi + y\eta + \dots + z\zeta + kt^{n-1})t,$$

calling this quantity  $\psi(x, y, \dots z, t)$ , the form  $\psi$  is evidently adjunctive to the form  $\phi$ : and if we expand so as to obtain

$$\psi(x, y, \dots z, t) = \psi_1(x, y, \dots z) t^\alpha + \psi_2(x, y, \dots z) t^\beta + \&c.,$$

it is evident  $\psi_1, \psi_2$ , &c. will be each separately adjunctive to  $\phi$ . These forms, when  $\psi$  is obtained by finding the determinant in respect to  $\xi, \eta, \dots \zeta$  of  $S$ , are, in fact, identical with Hermite's "formes adjointes".

The derivational mode of generating forms from constants depends upon the property of the operative symbol

$$\chi = \xi \frac{d}{dx} + \eta \frac{d}{dy} + \dots + z \frac{d}{dz},$$

applied to  $(\phi)$  a function of  $x, y, \dots z$ ; viz. that if in  $(\phi)$ , in place of these letters, we write linear functions thereof, to



wit  $x', y' \dots z'$ , we may write

$$\chi = \xi' \frac{d}{dx'} + \eta' \frac{d}{dy'} + \dots + \zeta' \frac{d}{dz'},$$

where  $\xi', \eta', \dots \zeta'$  will be the same functions of  $\xi, \eta, \dots \zeta$  that  $x', y', \dots z'$  are of  $x, y, \dots z$ .

Suppose now, in the first place, that in regard to  $\xi, \eta, \dots \zeta$ ,  $\psi(x, y, \dots z)$  is adjunctive to  $\chi' \cdot \phi(x, y, \dots z)$ ; then is the form  $\psi$  adjunctive to the form  $\phi$ , for on changing  $x, y, \dots z$  to  $x', y', \dots z'$ ,

$$\left( \xi \frac{d}{dx} + \eta \frac{d}{dy} + \dots + \zeta \frac{d}{dz} \right)' \phi(x', y', \dots z')$$

becomes  $\left( \xi' \frac{d}{dx'} + \eta' \frac{d}{dy'} + \dots + \zeta' \frac{d}{dz'} \right) \phi(x', y', \dots z')$ ;

and consequently  $\psi(x, y, \dots z)$  becomes  $\psi(x', y', \dots z')$ , multiplied by a power of the modulus of transformation, the modulus of that transformation, be it well observed, whereby  $x', y', \dots z'$  would be replaced by  $x, y, \dots z$ , and not as in the appositional mode of that converse transformation according to which  $x, y, \dots z$  would be replaced by  $x', y', \dots z'$ . It is on account of this converseness of the modes of transformation that the appositional and derivational modes of generating forms cannot except for a certain class of *restricted* linear transformations be combined in a single process. More generally, if instead of a single function  $\chi' \cdot \phi(x, y, \dots z)$ , we take as many such with different indices to  $\chi$  as there are variables, and form either the resultant in respect to  $\xi, \eta, \dots \zeta$ , or any other immutable constant in regard to those variables, (presuming in extension of the hyperdeterminant theory and as no doubt is the case, that such exist,) every such resultant or other constant will give a form of function of  $x, y, \dots z$  adjunctive to the given form ( $\phi$ ).

It may be shewn that every such resultant so formed will contain  $\phi$  as a factor.

Again, in the former more available determinant mode of generation, if we take the determinant in respect to  $\xi, \eta, \dots \zeta$ , it may be shewn that all the adjunctive functions so obtained will be algebraical derivees of the partial differential coefficients of  $\phi$  in respect to  $x, y, \dots z$ ; that is to say, if these be respectively zero, all such adjunctive functions so derived, as last aforesaid, will be zero, or in other

words, each such adjunctive is a syzygetic function of the partial differential coefficients of the primitive function.

To Mr. Boole is due the high praise of discovering and announcing, under a somewhat different and more qualified form and mode of statement, this marvel-working process of derivational generation of adjunctive forms. I was led back to it, in ignorance of what Mr. Boole had done, by the necessity which I felt to exist of combining Hesse's so-called functional determinant, under a common point of view with the common constant determinant of a function; under pressure of which sense of necessity, it was not long before I perceived that they formed the two ends of a chain of which Hesse's end exists for all homogeneous functions, but the other only when such functions are algebraical.

In fact, if we give to  $(r)$  every value from  $(2)$  upwards, the successive determinants in respect to  $\xi, \eta, \dots, \zeta$  of

$$\left( \xi \frac{d}{dx} + \eta \frac{d}{dy} + \dots \zeta \frac{d}{dz} \right)^r \cdot \phi(x, y, z),$$

will produce the chain in question, which, when  $\phi$  is algebraical and of  $n$  dimensions, comes to a natural termination when  $r = n - 1$ . The last member of and the number of terms in this chain are identical with the last member of and the number of terms in Sturm's auxiliary functions, when the variables are reduced to two. There is some reason to anticipate that this chain of functions may be made available in superseding Sturm's chain of auxiliaries; and if so, then the fatal hindrance to progress, arising from the unsymmetrical nature of the latter, is overcome, and we shall be able to pass from Sturm's theorem, which relates to the theory of Keno-themes, or Point-systems, to certain corresponding but much higher theories for lines, surfaces, and  $n$ -themes generally.

The restriction of space allowed to me in the present number of the *Journal* will permit me only to allude in the briefest terms to the theory of Relative Determinants, which, as it will be seen, plays an important part in the effectuation of the reductions of the higher algebraical functions to their simplest forms. Nor can the effect of the processes to be indicated be correctly appreciated without a knowledge of the circumstances under which the resultant of a *given* system of equations can sink in degree below the resultant of the *general* type of such system. Abstracting from the case when the equations separately, or in combination, subdivide into factors, this lowering of degree, as may be shewn by the doctrine of characteristics,

can only happen in one of two ways. Either the particular resultant obtained is a rational root of the general resultant, or the general resultant becomes zero for the case supposed, and the particular resultant is of a distinct character from the general resultant, being in fact the characteristic of the possibility not of the given system of equations being merely able to coexist (for that is already supposed), but of their being able to coexist for a certain system of values *other than* a given system or given systems. Such a resultant may be termed a Sub-resultant; the lowest resultant in the former case may be termed a Reduced-resultant. The theory of Sub-resultants is one altogether remaining to be constructed, and is well worthy equally of the attention of geometers and of analysts.

As to the theory of Relative Determinants, the object of this theory is to obtain the determinant resulting from eliminating as many variables as can be eliminated, chosen at pleasure from a set of variables greater in number than the equations containing them; and the mode of effectuating this object is through the method of the indeterminate multiplier. To avoid the discussion of the theory of sub-resultants and other particularities, I shall content myself with giving the rule applicable to the case (the only one of which as yet a practical application has offered itself to me in the course of my present inquiries) when all but one of the functions is linear.

If  $U, L_1, L_2, \dots, L_m$  be the first an  $n^c$  and the others linear functions of  $(n)$  variables, and it be desired to find the determinant of the resultant arising from the elimination of any  $(m)$  out of the  $(n)$  variables, the following is the rule:

Find the determinant, *i.e.* the resultant of the partial differential coefficients in respect to the given variables, and of

$$\lambda_1 \lambda_2 \dots \lambda_m \text{ of } U + L_1 \cdot \lambda_1 + L_2 \cdot \lambda_2 + \dots + L_m \cdot \lambda_m.$$

This resultant, in its lowest form, will be always a rational  $(n - 1)^{\text{th}}$  root of the resultant of the homogeneous system of equations to which the system above given can be referred as its type; and this reduced resultant divided by a power (determinable by the law of homogeneity) of the resultant of  $L_1, L_2, \dots, L_m$ , when all but the selected variables are made zero, will be the resultant determinant required.\* As regards

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\* The same method applies not only to the Final or Constant Determinant, but likewise to all the Functional Determinants in the chain above described, extending upwards from this to the Hessian, or as it ought to be termed, the first Boolean Determinant.



what has been said concerning the reducibility of the general typical resultant in the case before us, this is a consequence of, and may be brought into connection with, the following theorem, which is easily demonstrable by the theory of characteristics. If  $Q_1 Q_2 \dots Q_m$  be  $(m)$  homogeneous functions of  $(m)$  variables of the same degree,  $(r)$  of which enter in each equation only as simple powers uncombined with any of the other variables, then the degree of the reduced resultant is equal to the number of the equations multiplied by the  $(m - r - 1)^{\text{th}}$  power of the units of number on the degree of each, subject to the obvious exception that when  $r$  is  $m$ , (there being in fact but *one* step from  $r = m - 2$  to  $r = m$ ,) instead of  $r$ ,  $(r - 1)$  must be employed in the above formula. As an example of a sub-resultant as distinguished from a reduced-resultant, I instance the case of three quadratics  $U, V, W$ , functions of  $x, y, z$ , in each of which no squared power of  $z$  is supposed to enter: it may easily be shewn by my dialytic method that instead of six equations, between which to eliminate  $x^2, y^2, z^2, xy, xz, yz$ , we shall have only 5, the three original ones and two instead of three auxiliaries between which to eliminate  $x^2, y^2, xy, xz, yz$ , the *apparent* resultant is accordingly of the 9th instead of the 12th degree. But this is not the true characteristic of the possibility of the coexistence of the given systems, which in fact is zero, as is evidenced by the fact that they always *do* coexist, since they are always satisfiable by only *two* relations between the variables, to wit  $x = 0, y = 0$ . The apparent resultant is then something different, and what has been termed by the above a Sub-resultant.

I take this opportunity of entering my simple protest against the appropriation of my method of finding the resultant of any set of three equations of degrees equal or differing only by a unit, one from those of the other two, by Dr. Hesse, so far as regards quadratic functions, without acknowledgment, four years after the publication of my memoir in the *Philosophical Magazine*: the fundamental idea of Dr. Hesse's partial method is identical with that of my general one. Still more unjustifiable is the subsequent use of the *dialytic* principle, by the same author, equally without acknowledgment, and in cases where there is no peculiarity of form of procedure to give even a plausible ground for evading such acknowledgment. It is capable of moral proof that what I had written on the matter was sufficiently known in Berlin and at Königsberg, at each epoch of Dr. Hesse's use of the method.

I now proceed to the consideration of the more peculiar branch of my inquiry, which is as to the mode of reducing Algebraical Functions to their simplest and most symmetrical, or as my admirable friend M. Hermite well proposes to call them, their Canonical forms. Every quadratic function of any number of variables may always be linearly transformed into any other quadratic functions of the same, and that too in an infinite variety of ways; but in every other instance there will be only a limited number of ways, whereby, when possible, one form will admit of being transmuted into any other: and with the sole exception of a cubic function of two letters, such transmutation will never be possible, unless a certain condition, or certain conditions, be satisfied between the constants of the forms proposed for transmutation. The number of such conditions is the number of parameters entering into the canonical form, and is of course equal to the number of terms in the general form of the function diminished by the square of the number of letters. Thus there is one parameter in the canonical form for the biquadratic function of two and the cubic function of three letters, and no parameter in the cubic function of two letters. Hitherto no canonical forms have been studied beyond the cases above cited, but I have succeeded, as will presently be shewn, in obtaining methods for reducing to their canonical forms functions with *two* and *four* parameters respectively. Owing to what has been remarked above, the theory of quadratic functions is a theory apart. Simultaneous transformation gives definiteness to that theory, but has no existence for any useful purpose for functions of the higher degrees. Where the theory of simultaneous transformation ends, that of canonical forms properly begins; and in what follows, the case of quadratic forms is to be understood as entirely excluded. Such exclusion being understood, there is no difficulty in assigning the canonical, *i.e.* the simplest and most symmetrical general form to which every function of two letters admits of being reduced by linear transformations. If the degree be odd, say  $2m + 1$ , the canonical form will be  $u_1^{2m+1} + u_2^{2m+1} + \dots + u_{m+1}^{2m+1}$ ; if the degree be even, say  $2m$ , the canonical form will be

$$u_1^{2m} + u_2^{2m} + \dots + u_m^{2m} + K(u_1 u_2 \dots u_m)^2,$$

all the  $u$ 's being linear functions of the two given variables. It is easy to extend an analogous mode of representation to functions of any number of letters. From the above we see that for cubic, biquadratic, and quintic functions of two letters,

the canonical forms will be respectively

$$u^3 + v^3, \quad u^4 + v^4 + Ku^2v^2, \\ u^5 + v^5 + w^5,$$

with a linear relation in the last-named case between  $v, v, w$ .

First as to the reduction of any 4<sup>c</sup> function to Cayley's form

$$u^4 + v^4 + Ku^2v^2.$$

This may be effected in a great variety of ways, of which the following is not the simplest as regards the calculations required, but the most obvious. Let the modulus of transformation, whereby the given biquadratic function, say  $F(x, y)$ , becomes transmuted into its canonical form, be called  $M$ ; let the determinant of  $F$  be called  $D$ , and the determinant of the determinant in respect to

$$\xi \text{ and } \eta \text{ of } \left( \xi \frac{d}{dx} + \eta \frac{d}{dy} \right)^2 F(x, y),$$

which latter, for brevity's sake, may be termed the Hessian of  $F$ , (although in stricter justice the Boolean would be the more proper designation) be called  $D_2$ . Then, by examining the canonical form itself (which is as it were the very *palpitating heart* of the function laid bare to inspection), we shall obtain without difficulty the two equations

$$(1 - 9m^2)^2 = M^{12} D_1 \frac{1}{4^6}, \\ m^2 (1 - 9m^2)^2 (m^2 - 1)^2 = M^{24} D_2 \frac{1}{12^{12} 4^4}.$$

Eliminating the unknown quantity  $M$ , we obtain

$$\frac{m^2 (m^2 - 1)^2}{(1 - 9m^2)^2} = c, \quad \text{or} \quad \frac{m^3 - m}{1 - 9m^2} = c^{\frac{1}{2}},$$

where  $(c)$  is a known quantity.

This *cubic* equation for finding  $m$  is of a peculiar form; it being easy to shew *à priori*, by going back to the canonical form, that its three roots are  $m, \theta(m), \theta^2(m)$ , where

$$\theta(m) = \frac{m - 1}{3m + 1},$$

$\theta$  being a periodical form of function such that  $\theta^3(m) = m$ .

This it is which accounts for the simple expression for  $(m)$ , that may be obtained by solving the cubic above given. A



better practical mode is to take, instead of the determinant of the given function and its Hessian, the two hyperdeterminants and eliminate as before: a cubic equation having precisely the same properties, and in fact virtually identical with the former, will result. ( $m$ ) and consequently  $M$  being found, there is no difficulty whatever, calling the given function  $F$  and its Hessian  $H(F)$ , to form linear functions of the two, as

$$\left. \begin{aligned} \phi(m) \cdot F + \psi(m) \cdot H(F) \\ \phi_1(m) \cdot F + \psi_1(m) \cdot H(F) \end{aligned} \right\},$$

which shall be equal to, *i.e.* identical with,  $(u^2 + v^2)^2$  and  $u^2v^2$ , whence  $u$  and  $v$  are completely determined.

Another and interesting mode of solution is to take, besides the given function  $F$  and its Hessian, either the *second* Hessian or the post-Hessian of the given function, by the post-Hessian understanding the determinant in respect to

$$\xi \text{ and } \eta \text{ of } \left( \xi \frac{d}{dx} + \eta \cdot \frac{d}{dy} \right)^3 \cdot F:$$

any three of the four functions will be linearly related, and it may be shewn that, calling either the second Hessian (*i.e.* the Hessian of the Hessian) or the post-Hessian  $H'$ , we shall have

$$H' \cdot F + a \cdot H \cdot (F) + b \cdot (F) = 0,$$

where ( $a$ ) and ( $b$ ) will be *rational* and *integer* functions of the coefficients of  $(F)$ , and numerical multiples of two quantities  $R$  and  $S$ , such that the determinant of  $F$  will be equal  $R^3 + S^2$ ; and this, be it observed, without any previous knowledge of the existence of these hyperdeterminants  $R$  and  $S$ .

If now we go to Hesse's form for a cubic function of three letters, we shall find that precisely similar modes of investigation apply step for step. Calling the function  $F$  and its Hessian  $H(F)$ , and the post-Hessian or second Hessian at choice  $H' \cdot F$ , we shall find

$$H' \cdot F + m \cdot S \cdot H(F) + n \cdot R^2 \cdot (F) = 0,$$

where  $m$  and  $n$  are numerical quantities and  $R^3 + S^2$  equal the determinant of  $F$ . It is interesting to contrast this equation with the one previously mentioned as applicable to the 4<sup>c</sup> functions of two letters, viz.

$$H' \cdot (F) + m \cdot R H(F) + n \cdot S(F) = 0.$$

In both instances there is no difficulty in assigning the relations between the original  $R$  and  $S$ , and the  $R$  and  $S$  of any adjunctive form. All Arnohold's results may be thus obtained\* and further extended without the slightest difficulty.

As regards the equation for finding the parameter in Hesse's canonical form for the cubic of three letters, this will be of the 4<sup>th</sup> degree in respect to the cube of the parameter, and the roots will be functionally representable as

$$\begin{aligned} & x; \quad \theta(x); \quad \phi(x); \quad \psi(x), \\ \text{where} \quad & \theta^2(x) = \phi^2(x) = \psi^2(x) = x; \\ & \theta\phi(x) = \phi\theta(x) = \psi(x), \\ & \phi\psi(x) = \psi\phi(x) = \theta(x), \\ & \psi\theta(x) = \theta\psi(x) = \phi(x); \end{aligned}$$

owing to which property the equation is soluble under the peculiar form observed by Aronhold.

I pass on now to a brief account of the method, or rather of a method (for I doubt not of being able to discover others more practical), of reducing a function of the 5<sup>th</sup> degree of two letters (say of  $x$  and  $y$ ) to its canonical form  $u^5 + v^5 + w^5$ , subject to the linear relation  $au + bv + cw = 0$ , where the ratios  $a : b : c$ , and the linear relations between  $u, v, w$  and the two given variables are the objects of research. Here I have found great aid from the method of Relative Determinants; and I may notice that the successful application of more compendious methods to the question would be greatly facilitated were there in existence a theory of Relative Hyperdeterminants, which is still all to form, but which I little doubt, with the blessing of God, to be able to accomplish. It may some little facilitate the comprehension of what follows, if  $c$  be considered as representing unity.

Calling as before the given quintic function  $F$ , the modulus of transformation  $M$ , the Hessian and post-Hessian of  $F$ ,  $H$  and  $H'$ , and its ordinary or constant determinant  $D$ , we shall find

$$a^2.v^3.w^3 + b^2.w^3.u^3 + c^2.u^3.v^3 = M^2.H,$$

and  $P_1.P_2.P_3.P_4 = M^6.H'$ , where

$$P_1 = a^{\frac{3}{2}}vw + b^{\frac{3}{2}}wu + c^{\frac{3}{2}}uv,$$

$$P_2 = a^{\frac{3}{2}}vw - b^{\frac{3}{2}}wu - c^{\frac{3}{2}}uv,$$

$$P_3 = -a^{\frac{3}{2}}vw + b^{\frac{3}{2}}wu - c^{\frac{3}{2}}uv,$$

$$P_4 = -a^{\frac{3}{2}}vw - b^{\frac{3}{2}}wu + c^{\frac{3}{2}}uv;$$

also  $D = M^{20}$  multiplied by the product of the sixteen values of

$$a^{\frac{5}{4}} + b^{\frac{5}{4}}.(1)^{\frac{1}{4}} + c^{\frac{5}{4}}.(1)^{\frac{1}{4}}.$$

From the above equations it may be shewn that  $H'$ , (a

known function of the 8<sup>th</sup> degree of the given variables  $x, y$ ) must be capable of being thrown under the form

$$L\{(x - a_1.y)(x - a_2.y) \times (x - a_3.y)(x - a_4.y) \\ \times (x - a_5.y)(x - a_6.y) \times (x - a_7.y)(x - a_8.y)\},$$

$$\text{where } (a_1 - a_2)^2 \times (a_3 - a_4)^2 \times (a_5 - a_6)^2 \times (a_7 - a_8)^2$$

$$= \frac{D}{L^2} = K,$$

so that  $K$  is a known quantity.\* Accordingly the said equation of the 8<sup>th</sup> degree, considered as an algebraical equation in  $\frac{x}{y}$ , may by known methods be found by means of equations not exceeding the 4<sup>th</sup> or even the 3<sup>rd</sup> degree: in fact, to do this it is only necessary to form the equation to the squares of the differences of the roots of  $\frac{x}{y}$  in the equation  $H' \div y^8 = 0$ ,

which new equation will be of the 28<sup>th</sup> degree. If we then form two other equations of the 378<sup>th</sup> degree, one having its roots equal to  $\sqrt{K}$  multiplied by the binary products of the twenty-eight roots of the equation last named, the other to  $\sqrt{K}$  multiplied by the reciprocal of such binary products, the left-hand members of these two equations expressed under the usual form will have a factor in common, which may be found by the process of common measure and will be of the 6<sup>th</sup> degree, but whose roots consisting of three pairs of reciprocals may be found by the solution of cubics only.

In this way, by means of cubics and quadratics,

$$(a_1 - a_2)^2, (a_3 - a_4)^2, (a_5 - a_6)^2, (a_7 - a_8)^2,$$

can be found, which being known,

$$a_1a_2, a_3a_4, a_5a_6, a_7a_8,$$

can be determined in pairs by means of quadratics from the equation  $H' \div y^8 = 0$ . This being supposed to be done, we have

$$P_1 = f.L_1,$$

$$P_2 = g.L_2,$$

$$P_3 = l.L_3,$$

$$P_4 = k.L_4,$$

where  $L_1, L_2, L_3, L_4$ , are *known* quadratic functions of

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\* Or in other words, the post-Hessian determinant of a given function in two letters of the second degree, may be divided into four quadratic factors in such a way that the product of the determinants of these several factors shall be equal to the determinant of the given function.



$x$  and  $y$ . To determine the ratios of  $f, g, l, k$ , we have three equations\* obtained from the identity

$$fL_1 + gL_2 + hL_3 + kL_4 (= P_1 + P_2 + P_3 + P_4) = 0;$$

$f:g:h:k$ , being known  $fL_1:gL_2:hL_3:kL_4$  are known ratios.

But

$$P_1 + P_2 = 2a^{\frac{3}{2}}.vw,$$

$$P_1 + P_3 = 2b^{\frac{3}{2}}.wu,$$

$$P_1 + P_4 = 2c^{\frac{3}{2}}.uv.$$

Hence

$$a^{\frac{3}{2}}.vw = \lambda.P,$$

$$b^{\frac{3}{2}}.wu = \lambda.Q,$$

$$c^{\frac{3}{2}}.uv = \lambda.R,$$

where  $P, Q, R$  are known quadratic functions of  $x, y$ .

Hence  $a:b:c$  may be found by means of the identical equation

$$a^2.v^3w^3 + b^2.w^3v^3 + c^2.v^3u^3 = H(F),$$

whereby the ratios  $a^{-\frac{5}{2}}:b^{-\frac{5}{2}}:c^{-\frac{5}{2}}$  can be obtained without any further extraction of roots, shewing that there is but one single true system of ratios  $a^5:b^5:c^5$  applicable to the problem;  $a:b:c$  being thus found,  $\lambda$  is easily determined, and thus finally  $u, v, w$  are found in terms of  $x$  and  $y$ .†

I have little doubt that a more expeditious mode of solution than the foregoing‡ will be afforded by an examination of the properties and relations of the *quadratic and cubic forms*, adjunctive to the general quintic functions, and indeed to every  $(4n+1)^c$  function of two letters hereinbefore adverted to.

Sufficient space does not remain for detailing the steps whereby the general cubic function of *four* letters may, by aid of equations *not transcending the fifth degree*, be reduced to its canonical form  $u^5 + v^5 + w^5 + p^5 + q^5$ , wherein  $u, v, w, p, q$  are connected by a linear equation

$$au + bv + cw + dp + eq = 0;$$

the four ratios of whose coefficients  $a:b:c:d:e$  give the

\* For we must have the coefficients of  $x^2$  of  $xy$  and  $y^2$  in

$$fL_1 + gL_2 + hL_3 + kL_4,$$

all of them zero.

† The problem thus solved may be stated as consisting in reducing the general function  $ax^5 + bx^4y + cx^3y^2 + dx^2y^3 + exy^4 + fy^5$  to the form

$$(lx + my)^5 + (l'x + m'y)^5 + (l''x + m''y)^5.$$

‡ The coefficients in the reducing recurrent equation of the 6th degree in the process above detailed may rise to be of 541632 dimensions in respect to the original coefficients in  $F$ .

necessary number  $\frac{4.5.6}{1.2.3} - 4^2$  parameters furnished by the general rule. Suffice it for the present to say, that the analytical mode of solution depends upon a circumstance capable of the following geometrical statement: "Every surface of the 4<sup>th</sup> degree represented by a function which is the Hessian to any given cubic function whatever of four letters, has lying upon it ten straight lines meeting three and three in ten points, and these ten points are the only points which enjoy the following property in respect to the surface of the 3<sup>rd</sup> degree denoted by equating to zero the cubical function in question, to wit, that the cone drawn from any one of them as vertex to envelop the surface, will meet it not in a continuous double curve of the 6<sup>th</sup> degree, but in two curves each of the 3<sup>rd</sup> degree, lying in *planes* which intersect in the ten lines respectively above named; so that to each of the ten points corresponds one of the ten lines: these ten points and lines are the intersections taken respectively three with three, and two with two, of a *single and unique system* of five principal planes appurtenant to every surface of the 3<sup>rd</sup> degree, and these planes are no other than those denoted by

$$u = 0, \quad v = 0, \quad w = 0, \quad p = 0, \quad q = 0.$$

I have found also by the theory of Sub-resultants, that the analogy between lines and surfaces of the 3<sup>rd</sup> degree, in regard to the existence of double and conical points, is preserved in this wise: that in the same way as a double point on a curve of the 3<sup>rd</sup> degree commands the existence of a double point on its Hessian, so does a conical point in a surface of the 3<sup>rd</sup> degree command over and above the 10 necessary, and so to speak natural conical points, at least one extra, that is to say an 11<sup>th</sup> conical point on *its* Hessian. And here for the present I must quit my brief and imperfect notice of this subject, composed amidst the interruptions and distractions of an official and professional life.

*Observation.*—It may be somewhat interesting and instructive to my readers, to have a table of the successive scalar\* determinants of a quintic function of two letters presented to them at a single glance. Preserving the notation of page 197, we have the following expressions:

\* By which I mean the determinants in respect to

$$\xi, \eta \text{ of } \left( \xi \frac{d}{dx} + \eta \frac{d}{dy} \right)^r . F(xy).$$

The given function =  $u^5 + v^5 + w^5$ ,

its Hessian =  $M^2 (a^2 v^3 w^3 + b^2 w^3 u^3 + c^2 u^3 v^3)$ ,

its post-Hessian =  $M^6 \times$  the product of the *four* forms of

$$a^{\frac{3}{2}} v w + b^{\frac{3}{2}} (1)^{\frac{1}{2}} w u + c^{\frac{3}{2}} (1)^{\frac{1}{2}} u v;$$

its præter-post-Hessian =  $M^{12} \times$  the product of the *nine* forms of

$$a^{\frac{4}{3}} v^{\frac{1}{3}} w^{\frac{1}{3}} + b^{\frac{4}{3}} (1)^{\frac{1}{3}} w^{\frac{1}{3}} u^{\frac{1}{3}} + c^{\frac{4}{3}} (1)^{\frac{1}{3}} a^{\frac{1}{3}} v^{\frac{1}{3}},$$

and the final determinant =  $M^{20} \times$  the product of the *sixteen* forms of

$$a^{\frac{5}{4}} + (1)^{\frac{1}{4}} b^{\frac{5}{4}} + (1)^{\frac{1}{4}} c^{\frac{5}{4}}.$$

The success of the method applied depends (as above shewn) upon the fact of a certain function of the roots of the post-Hessian (which is an octavic function of the variables) being known, which fact *hinges* upon the circumstance that

$$(M^6)^2 \times (M^2)^4 = M^{20}.$$

P.S.—I have much pleasure in subjoining the cubical hyperdeterminant of the 12<sup>th</sup> degree function of two letters, worked out upon the principle of Compound Permutation hinted at in the foregoing pages, for which I am indebted to the kindness and skill of my friend Mr. Spottiswoode.

The function being called

$$a.x^{12} + 12bx^{11}.y + \frac{12.11}{2}.cx^{10}.y^2 + \&c... + ly^{12},$$

the following is its cubical hyperdeterminant :

$$\begin{aligned} & agm - bahl + 15aik + 10aj^2 - 6bfm, \\ & - 24bhk + 30bgl + 20bij - 24cfl + 114cgk, \\ & - 145ci^2 + 50chj + 15cem + 20cgi + 20ch^2, \\ & - 400dgj + 280dhi + 20del + 50dfe + 10d^2k, \\ & + 385egi - 135e^2k - 290ek^2 + 705fgh, \\ & - 330f^2i - 50g^3. \end{aligned}$$

Mr. Spottiswoode will I hope publish the work itself in the next number of the *Journal*, in which I shall also shew how the hyperdeterminants of the cubical function of three letters, Aronhold's *S* and *T*, may be similarly obtained.

April, 1851.





# SUPPLEMENT

TO A PAPER IN THE NUMBER FOR MAY (1851) OF  
 “THE CAMBRIDGE AND DUBLIN MATHEMATICAL JOURNAL,”

ENTITLED

“SKETCH OF A MEMOIR ON ELIMINATION, TRANSFORMATION,  
 AND CANONICAL FORMS.”

BY J. J. SYLVESTER, M.A., F.R.S.

SINCE the above paper was in print I have succeeded in obtaining a canonical representation of the quadratic and cubic functions adjunctive to the general quintic (5th degree) functions of two letters.

Let  $F$  the quintic function of  $x, y$ ,

$$= u^5 + v^5 + w^5,$$

and

$$au + bv + cw = 0.$$

$M$  being the modulus of the transformation, whereby transition is made from  $x, y$  to  $u, v$ . Then the quadratic adjunctive is

$$\frac{M^4}{c^4} \left\{ a^4 vw + b^4 wu + c^4 uv \right\} ;$$

and the cubic adjunctive is simply

$$\frac{1}{c^3} M^6 (abc)^2 uvw. *$$

\* The knowledge of the existence of these *lower* adjunctive forms is mainly a consequence of Mr. Cayley's splendid discovery of hyperdeterminant constants. In fact, they are respectively the quadratic and cubic hyperdeterminants in respect to  $\xi$  and  $\eta$  of  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} \left( \xi \frac{d}{dx} + \eta \frac{d}{dy} \right)^4 \cdot F$ ;  $x$  and  $y$  being treated as constants.

The fortunate proclaimer of a new outlying planet has been justly rewarded by the offer of a baronetcy and a national pension, which the writer of this wishes him long life and health to enjoy. In the meanwhile, what has been done in honour of the discoverer of a new and inexhaustible region of exquisite analysis?

Hence we can, in accordance with what I ventured to predict in the preceding sketch, find  $u, v, w$ , by means of a simple and practical co-process. To wit, call

$$F = lx^5 + 5mx^4y + 10nx^3y^2 + 10px^2y^3 + 5qxy^4 + ry^5.$$

Form the determinant,

$$\begin{array}{lll} lx + my & mx + ny & nx + py \\ mx + ny & nx + py & px + qy \\ nx + py & px + qy & qx + ry \end{array}$$

Let this cubic function, by solving it as a cubic equation, be made equal to

$$L(x + fy) \cdot (x + gy) \cdot (x + hy),$$

then

$$u = h(x + fy) \quad v = l(x + gy) \quad w = m(x + hy).$$

By means of the identity,  $F = u^5 + v^5 + w^5$ ;  $l^5, m^5, n^5$ , are known by the solution of linear equations, and thus  $u, v, w$ , are determined by solving a cubic equation instead of one of the eighth degree, as in the method first given, and the process of canonising a quintic function is rendered *practically* possible.

For brevity sake let  $c$  represent unity. The constant determinant of the cubic adjunctive will be found to be

$$3 M^{30} (abc)^{10}.$$

Calling, then, the cubic adjunctive of  $F$ ,  $C(F)$ , we have the remarkable equation

$$uvw = \frac{C(F)}{\sqrt[5]{\frac{1}{3} \square C(F)}}.$$

It may also be shown that if we call the Hessian of  $F$ ,  $H(F)$ , we shall have the following equally remarkable equation:

$$\square H(F) = \frac{1}{3} \square F \times \square C(F).$$

Again, calling the quadratic adjunctive of  $F$ ,  $Q(F)$ , we shall easily find

$$\square Q(F) = M^{10} \left\{ \begin{array}{l} (a^{\frac{5}{2}} + b^{\frac{5}{2}} + c^{\frac{5}{2}}) \\ (a^{\frac{5}{2}} + b^{\frac{5}{2}} - c^{\frac{5}{2}}) \\ (a^{\frac{5}{2}} - b^{\frac{5}{2}} + c^{\frac{5}{2}}) \\ (a^{\frac{5}{2}} - b^{\frac{5}{2}} - c^{\frac{5}{2}}) \end{array} \right\}$$



or, if we please,

$$= M^{10} \left\{ \begin{array}{l} a^{10} + b^{10} + c^{10} \\ - 2a^5 b^5 - 2a^5 c^5 - 2b^5 c^5 \end{array} \right\}.$$

$u, v, w$  being known,  $a, b, c$ , which are the resultants of  $v, w$ ;  $w, u$ ;  $u, v$  respectively are known. But their ratios, or, if we please to say so, the ratios of  $a^5 : b^5 : c^5$ , may be found independently and very elegantly as follows:—

Let  $M^{10} \times$  product of the 4 forms of  $a^{\frac{5}{2}} + 1^{\frac{1}{2}} b^{\frac{5}{2}} + 1^{\frac{1}{2}} c^{\frac{5}{2}} = A$ .

$M^{20} \times$  product of the 16 forms of  $a^{\frac{5}{4}} + 1^{\frac{1}{4}} b^{\frac{5}{4}} + 1^{\frac{1}{4}} c^{\frac{5}{4}} = B$

$M^{30} \times a^{10} \cdot b^{10} \cdot c^{10} = C$ .

$A, B, C$  are known quantities, being respectively what we have called  $\square Q(F)$ ,  $\square(F)^*$ ,  $\frac{1}{3} \square C(F)$ .

It may easily be shown that

$$B - A^2 = M^{20} \cdot 128 a^5 \cdot b^5 \cdot c^5 (a^5 + b^5 + c^5).$$

Hence  $M^5 \cdot a^5$ ,  $M^5 \cdot b^5$ ,  $M^5 \cdot c^5$  are the roots of  $\rho$  in the cubic equation

$$\rho^3 + \frac{B - A^2}{2^7 C^{\frac{1}{2}}} \rho^2 + \frac{1}{4} \left\{ \frac{(B - A^2)^2}{2^{14} C} - A \right\} \rho + C^{\frac{1}{2}} = 0.$$

$A, B, C$ , it will be observed, are independent and, as they may be termed, prime or radical adjunctive constants. Hitherto much mystery and uncertainty have attached to the theory of hyperdeterminants, from its having been tacitly assumed that they were always either of lower dimensions than the ordinary determinant, or else algebraical functions of such, and of the determinant. Whereas we now see that,

\* More strictly speaking (and this correction should be supplied throughout in the "Sketch"),  $B$  is the negative determinant of  $\frac{1}{2} F$ . After finding, by the method of characteristics, or any special artifices, the algebraic part of the value of a resultant or determinant, a process frequently of some complexity remains over in assigning its numerical multiplier; this part of the operation being analogous to that which occurs in the Integral Calculus, of determining the constant to be added after the general form of an integral has been determined. In the "Sketch," a correction for the numerical multiplier remains also to be applied to the expressions given for the successive Hessian determinants.

whilst the determinant of a function in two letters of the fifth degree is of eight dimensions, one of its radical or primitive hyperdeterminants is of four, but the other of twelve dimensions. This is a most valuable consequence, and would seem to indicate that the number of radical hyperdeterminants to a function, over and above the common determinant, is always equal to the number of parameters entering into its canonical form. The importance of this ascertainment of an unsuspected third *radical* constant, adjunctive to a quintic function of two letters, in making to march the theory of hyperdeterminants, can hardly be over-estimated.

From the equation last given we are enabled to assign the conditions in order that two functions of the fifth degree may be capable of being linearly transformed either into the other. For if we call  $F$  and  $F'$  two such linearly equivalent quintic functions, they must be capable each of being thrown under the same form  $u^5 + v^5 + (lu + mv)^5$ , where  $l$  and  $m$  shall be the same for each. Consequently we must have the roots of  $\rho$  in the same ratio for  $F$  and  $F'$ , which conditions may be expressed by means of the two equations

$$\frac{B - A^2}{C^{\frac{2}{3}}} = \frac{B' - A'^2}{C'^{\frac{1}{3}}}$$

$$\frac{(B - A^2)^2 - 2^{14} AC}{C^{\frac{4}{3}}} = \frac{(B' - A'^2)^2 - 2^{14} A' C'}{C'^{\frac{4}{3}}}$$

$A'$ ,  $B'$ ,  $C'$ , of course representing the same functions of the coefficients of  $F'$  as  $A$ ,  $B$ ,  $C$ , respectively of  $F$ .

The two conditions required in their simplest form are accordingly

$$\frac{A}{C^{\frac{1}{3}}} = \frac{A'}{C'^{\frac{1}{3}}}$$

$$\frac{B}{C^{\frac{2}{3}}} = \frac{B'}{C'^{\frac{2}{3}}}$$

$$\text{or } A^3 : B^2 : C :: A'^3 : B'^2 : C'$$

that is to say, *all quintic functions of two letters of which the determinant is to the subduplicate power of the radical hyperdeterminant of the twelfth order and to the sesquiduplicate power of the radical hyperdeterminant of the fourth order in given ratios, are mutually convertible.*

So for the quartic (*i.e.* biquadratic) function of two letters, calling  $R$  and  $S$  the radical adjunctive constants of the second and third order, the conditions of convertibility between different forms of the same is, that  $R^3 : S^2$  shall be a given ratio. And, in general, we may infer that the conditions of convertibility between different functions of any degree is, that the several radical adjunctive constants of each raised respectively to such powers as will make them of like dimensions, shall be to one another in given ratios. Of course all cubic functions of two letters, according to this rule, are mutually convertible without any condition, they having but one radical adjunctive constant ; and in fact all such functions, being representable as the sum of two cubes of new variables linearly related to those given, are necessarily convertible.

I have further succeeded in obtaining the canonical form of the *quadratic* adjunctive to *any odd degeed* function of two letters, which presents a wonderful analogy to the theory of relative determinants of *quadratic functions of any number* of letters, and constitutes an important step towards the construction of the theory of relative hyperdeterminants.

Let a function of two letters of the odd degree  $m (= 2n - 1)$  be thrown under its canonical form,

$$u_1^m + u_2^m + \dots + u_n^m.$$

and let there exist the  $n - 2$  equations,

$$a_1 \cdot u_1 + a_2 \cdot u_2 + \dots + a_n \cdot u_n = 0. \quad (1.)$$

$$b_1 \cdot u_1 + b_2 \cdot u_2 + \dots + b_n \cdot u_n = 0. \quad (2.)$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$l_1 \cdot u_1 + l_2 \cdot u_2 + \dots + l_n \cdot u_n = 0. \quad (m-2.)$$

Then, if  $M$  be the modulus of the transformation which converts  $u, v$ , into  $x, y$ , and if, on making  $\theta_1 \theta_2 \dots \theta_n$  disjunctively equal to  $1, 2 \dots n$  we use  $(\theta_{n-1}, \theta_n)$  to denote in general the determinant

$$\begin{array}{ccccccc} a_{\theta_1} & a_{\theta_2} & \dots & \dots & a_{\theta_{n-2}} & & \\ b_{\theta_1} & b_{\theta_2} & \dots & \dots & b_{\theta_{n-2}} & & \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ l_{\theta_1} & l_{\theta_2} & \dots & \dots & l_{\theta_{n-2}} & & \end{array}$$



the quadratic adjunctive of  $\frac{1}{m(m-1) \dots 2} \cdot F$  will be

$$\frac{M^{m-1}}{(1, 2)^{m-1}} \Sigma \left\{ (\theta_r, \theta_s)^{m-1} (u_r \cdot u_s) \right\} .^*$$

*N.B.* By means of this formula, and of the theorem for finding relative determinants of quadratic functions, we can obtain the general canonical form for one set of the biquadratic adjunctive constants (hyperdeterminants of the fourth order in Mr. Cayley's language) of any odd degreed function of two letters.†

Thus, for the fifth degree, preserving the relation of the "Sketch," we have the biquadratic adjunctive constant

$$= 0 \quad \left. \begin{array}{cccc} c^4 & b^4 & a & \\ c^4 & 0 & a^4 & b \\ b^4 & a^4 & 0 & c \\ a & b & c & 0 \end{array} \right\} \times \frac{M^{10}}{c^{10}}.$$

For the seventh degree, if we suppose the function to be equal to

$$u^7 + v^7 + w^7 + \theta^7,$$

and

$$au + bv + cw + d\theta = 0,$$

$$a'u + b'v + c'w + d'\theta = 0;$$

the biquadratic adjunctive constant will be  $\frac{M^{14}}{(cd' - c'd)^{14}}$  multiplied by the determinant

0	$(ab' - ba')^6$	$(ac' - a'c)^6$	$(ad' - a'd)^6$	$a$	$a'$
$(ba' - b'a)^6$	0	$(bc' - b'c)^6$	$(bd' - b'd)^6$	$b$	$b'$
$(ca' - c'a)^6$	$(cb' - c'b)^6$	0	$(cd' - c'd)^6$	$c$	$c'$
$(da' - d'a)^6$	$(db' - d'b)^6$	$(dc' - d'c)^6$	0	$d$	$d'$
$a$	$b$	$c$	$d$	0	0
$a'$	$b'$	$c'$	$d'$	0	0

\* The condition  $m=2n-1$  is only necessary in order that  $\Sigma'_n(u^m)$  may be a canonical, because a possible and determinate, form for any given function of the  $m$ th degree. But the theorem in the text, so far as it serves to obtain the quadratic adjunctive of  $\Sigma'_n(u^m)$ , is true for *all* odd values of  $m$ , whether greater or less than  $2n-1$ .

† See Note (A) of Appendix.

The determinants of the Hessian, the post-Hessian, and the præter-post-Hessian of  $F$  will be found (in the case of the quintic function) to be always multiples of powers of the determinant of the given function, and of its cubic adjunctive; and I believe that in general for a function of two letters of any degree the determinants of all the derived forms in the Hessian scale\*, will be necessarily algebraical functions of any two of them.

I hope very shortly to accomplish the reduction of functions, as high as the seventh degree of two letters, to their canonical form, and also to present a complete theory of the failing or singular cases of canonical forms.

26. *Lincoln's Inn Fields,*  
15th May, 1851.

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SINCE the above was in print I have discovered the following

GENERAL THEOREM

*for reducing a function of two letters of any odd degree to its canonical form.*

Let the degree of the function be  $(2n-1)$ ; then its canonical form is

$$u_1^{2n-1} + u_2^{2n-1} + \dots + u_n^{2n-1}$$

with  $(n-2)$  linear relations between  $u_1, u_2, \dots u_n$ .

To find  $u_1, u_2, \dots u_n$ , proceed as follows. Let the given function of the  $(2n-1)$ th degree be supposed to be

$$a_1 x^{2n-1} + (2n-1)a_2 \cdot x^{2n-2} \cdot y + (2n-1) \frac{2n-2}{2} \cdot a_3 \cdot x^{2n-3} \cdot y^2 + \dots \\ \dots + a_{2n} \cdot y^{2n-1}$$

---

\* I use the term Hessian (more properly speaking the Boolean) Scale, to denote the *determinants* in respect of  $\xi$  and  $\eta$  of  $\left(\xi \frac{d}{dx} + \eta \frac{d}{dy} + \&c.\right)^2 \cdot F$ .

Neither Hesse, however, nor any other writer up to the present time, had thought of constructing, and still less of turning to account, the functions (the first only excepted) which figure in this scale.

Form the determinant

$$\begin{array}{cccccccc} a_1x + a_2 \cdot y, & a_2x + a_3y, & a_3x + a_4y & \dots & a_n \cdot x + a_{n+1} \cdot y \\ a_2x + a_3 \cdot y, & a_3x + a_4 \cdot y, & \dots & \dots & a_{n+1} \cdot x + a_{n+2} \cdot y \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ a_2x + a_{n+1} \cdot y, & a_{n+1}x + a_{n+2} \cdot y & \dots & \dots & a_{2n-1} \cdot x + a_{2n} \cdot y. \end{array}$$

This determinant is a function of  $x$  and  $y$  of the  $n$ th degree, and by resolving an equation of the  $n$ th degree, may be decomposed into  $(n)$  factors, say

$$(l_1 \cdot x + m_1 \cdot y) (l_2 \cdot x + m_2 \cdot y) \dots (l_n \cdot x + m_n \cdot y);$$

we shall then have

$$\begin{array}{l} u_1 = p_1 \cdot (l_1x + m_1 \cdot y) \\ u_2 = p_2 \cdot (l_2x + m_2 \cdot y) \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ u_n = p_n \cdot (l_n \cdot x + m_n \cdot y) \end{array}$$

where the  $(l)$ s and  $(m)$ s are known, and the  $(2n-1)$ th powers of the  $(p)$ s may be found linearly, by means of the identical equation  $\Sigma u^{2n-1} = F(x, y)$ . [Thus *ex. gr.* a function of the seventh degree of two letters may be reduced to its canonical form

$$(lx + my)^7 + (l'x + m'y)^7 + (l''x + m''y)^7 + (l'''x + m'''y)^7$$

by the resolution of a biquadratic equation.] My demonstration of this extraordinary and unexpected consequence rests upon the following lemma\*, itself a very beautiful and striking theorem (no doubt capable of much generalisation) in the theory of determinants. Form the rectangular matrix consisting of  $n$  rows and  $(n+1)$  columns

$$\begin{array}{cccccccc} T_1 & T_2 & T_3 & \cdot & \cdot & \cdot & \cdot & T_{n+1} \\ T_2 & T_3 & T_4 & \cdot & \cdot & \cdot & \cdot & T_{n+2} \\ T_3 & T_4 & T_5 & \cdot & \cdot & \cdot & \cdot & T_{n+3} \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ T_n & T_{n+1} & T_{n+2} & \cdot & \cdot & \cdot & \cdot & T_{2n} \end{array}$$

where

$$T_i = a_1^{r-i} \cdot b_1^{s+i} + a_2^{r-i} \cdot b_2^{s+i} + \dots + a_{n-1}^{r-i} \cdot b_{n-1}^{s+i}$$

---

\* See Note (B) of Appendix.

Then all the  $n+1$  determinants that can be formed by rejecting any *one* column at pleasure out of this matrix are identically zero.

In order the better to realise the proof, suppose

$$n=4, \text{ so that } 2n-1=7.$$

Let

$$F(x, y) = a_1 x^7 + 7a_2 \cdot x^6 y + 21a_3 \cdot x^5 y^2 + 35a_4 \cdot x^4 y^3 + 35a_5 \cdot x^3 y^4 \\ + 21a_6 \cdot x^2 y^5 + 7a_7 \cdot xy^6 + a^8 \cdot y^7$$

Suppose

$$t^7 + u^7 + v^7 + w^7 = F(x, y) = G(u, v)$$

$$at + bu = v$$

$$a't + b'u = v'$$

Then, if  $M$  is the modulus of transition from  $x, y$  to  $u, v$  the hyperdeterminant, or, to adopt my new expression, the permutant  $P_4$  (meaning thereby)

$$\begin{array}{cccc} a_1 \cdot x + a_2 \cdot y & a_2 \cdot x + a_3 \cdot y & a_3 x + a_4 \cdot y & a_4 \cdot x + a_5 y \\ a_2 \cdot x + a_3 \cdot y & a_3 x + a_3 \cdot y & a_4 x + a_5 y & a_5 x + a_6 \cdot y \\ a_3 x + a_4 \cdot y & a_4 \cdot x + a_5 y & a_5 x + a_6 \cdot y & a_6 x + a_7 \cdot y \\ a_4 \cdot x + a_5 y & a_5 \cdot x + a_6 \cdot y & a_6 \cdot x + a_7 \cdot y & a_7 \cdot x + a_8 \cdot y \end{array}$$

which is a constant adjunctive in respect to  $\xi$  and  $\eta$  of  $\left(\xi \frac{d}{dx} + \eta \frac{d}{dy}\right)^6 F$ , will, according to the principles laid down in the preceding "Sketch," be the product of a power of  $M$  multiplied by the corresponding adjunctive constant of  $\left(\xi \frac{d}{du} + \eta \frac{d}{dv}\right)^6 \cdot H(u, v)$ , and is therefore a multiple of the determinant

$$\begin{array}{cccc} (1 + A_1)t + A_2 \cdot u, & A_2 t + A_3 u, & A_3 t + A_4 u, & A_4 t + A_5 u \\ A_2 t + A_3 u, & A_3 t + A_4 u, & A_4 t + A_5 u, & A_5 t + A_6 \cdot u \\ A_3 t + A_4 u, & A_4 t + A_5 u, & A_5 t + A_6 \cdot u, & A_6 t + A_7 \cdot u \\ A_4 t + A_5 u, & A_5 t + A_6 \cdot u, & A_6 t + A_7 \cdot u, & A_7 t + (1 + A_8)u \end{array}$$

where

$$A_1 = a^7 + a'^7, \quad A_2 = a^6 \cdot b + a'^6 \cdot b', \quad A_3 = a^5 b^2 + a'^5 b'^2 \dots \\ \dots \dots \dots A_8 = b^7 + b'^7$$

In this determinant the coefficient of  $u^4$  is



$$\left. \begin{array}{cccc} A_2, & A_3, & A_4, & A_5 \\ A_3, & A_4, & A_5, & A_6 \\ A_4, & A_5, & A_6, & A_7 \\ A_5, & A_6, & A_7, & 1 + A_8 \end{array} \right\} \text{ which is equal to }$$

$$\begin{aligned}
 & A_5 \times \left\{ \begin{array}{ccc} A_3, & A_4, & A_5 \\ A_4, & A_5, & A_6 \\ A_5, & A_6, & A_7 \end{array} \right\} - A_6 \left\{ \begin{array}{ccc} A_2, & A_4, & A_5 \\ A_3, & A_5, & A_6 \\ A_4, & A_6, & A_7 \end{array} \right\} \\
 & + A_7 \times \left\{ \begin{array}{ccc} A_2, & A_3, & A_5 \\ A_3, & A_4, & A_6 \\ A_4, & A_5, & A_7 \end{array} \right\} - (1 + A_8) \left\{ \begin{array}{ccc} A_2, & A_3, & A_4 \\ A_3, & A_4, & A_5 \\ A_4, & A_5, & A_6 \end{array} \right\}
 \end{aligned}$$

= 0, because the second factors of the products are all zero by the lemma. Hence the permutant  $P_4$  vanishes when  $t=0$ , and consequently it contains  $t$  as a factor, and in like manner it may be proved to contain  $u, v, w$ .

Hence  $t, u, v, w$  are the algebraical factors of  $P_4$ , and precisely the same proof applies to show in the case of a function in  $x$  and  $y$ , say  $F_{2n-1}$ , of any odd degree  $(2n-1)$  whatever, that the corresponding permutant  $P_n$  will contain the factors  $u_1, u_2 \dots u_n$  linear functions of  $x, y$ , such that

$$u_1^{2n-1} + u_2^{2n-1} + \dots + u_n^{2n-1} = F_{2n-1}$$

as was to be shown.

Whenever  $P_n$  has equal roots, this will denote either (which is the more general case) that the usual canonical form fails and gives place to a singular form, (owing to some of the coefficients of transformation becoming infinite,) or, which is the more special supposition, that the canonical form becomes catalectic by one or more of the linear roots\* disappearing. Thus in the cubic function, if  $P_2$  has equal roots, and consequently its determinant (which is coincident with that of the function itself) vanish, then the canonical form in general fails; so that, for example,  $ax^3y + bx^2y$  cannot in general be exhibited as the sum of two cubes: if, however, certain further relations obtain between the coefficients of  $F$ , the canonical form reappears catalectically, the function becoming in fact representable as a single cube. So, again, for the

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\*  $u_1 u_2 \dots u_n$  may be termed the linear roots of the form  $F_{2n-1}$ .



and call  $M$  the modulus of transformation in respect to  $u_1, u_2$ , and if we make

$$P_n = K u_1 \cdot u_2 \cdot \dots \cdot u_n;$$

then

$$\left\{ \begin{array}{cccccc} a_3 & a_4 & \dots & \dots & \dots & a_n \\ b_3 & b_4 & \dots & \dots & \dots & b_n \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ l_3 & l_4 & \dots & \dots & \dots & l_n \end{array} \right\}^{\frac{n \cdot n-1}{2}} \quad K = \text{the product of the}$$

$$n \cdot \frac{n-1}{2} \text{ factors of the form } \left\{ \begin{array}{cccccc} a_{\theta_1} & a_{\theta_2} & \dots & \dots & \dots & a_{\theta_{n-2}} \\ b_{\theta_1} & b_{\theta_2} & \dots & \dots & \dots & b_{\theta_{n-2}} \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ l_{\theta_1} & l_{\theta_2} & \dots & \dots & \dots & l_{\theta_{n-2}} \end{array} \right\}$$

$\theta_1, \theta_2, \dots, \theta_{n-2}$  being any  $(n-2)$  numbers out of the  $n$  numbers  $1, 2, 3, \dots, n$ .

It may hence be shown that

$$u_1 \cdot u_2 \cdot \dots \cdot u_n = \frac{P_n}{\left( \frac{1}{m} \square_{x,y} \cdot P_n \right)^{\frac{1}{2n-1}}}$$

$m$  being a number which is a function of  $n$ , and which may be shown to be equal to  $\square_{x,y} (x^{n-1} \cdot y + xy^{n-1}) \div$  product of the squared differences of the roots of  $l^{n-2} = 1$ ,

$$\text{i. e., } m = \left( \frac{(n-1)^2 - 1}{(n-2)} \right)^{n-2} = (-n)^{n-2}$$

and thus

$$u_1, u_2 \cdot \dots \cdot u_n = \frac{P_n}{\sqrt[2n-1]{(-n)^{-(n-2)} \cdot \square_{x,y} P_n}}$$

As an example of the mode of finding  $u_1, u_2 \cdot \dots \cdot u_n$ , let  $F = 3x^5 + 20x^3y^2 + 10xy^4$ ,

$$\text{then } P_3 = \left\{ \begin{array}{ccc} 3x, & 2y, & 2x \\ 2y, & 2x, & 2y \\ 2x, & 2y, & 2x \end{array} \right\} = 4x^3 - 4y^2x.$$

Hence  $u = fx$ ,  $v = g(x+y)$ ,  $w = h(x-y)$ .

To find  $f, g, h$ , we have  $u^5 + v^5 + w^5 = F$ ,

$$\therefore f^5 + g^5 + h^5 = 3; \quad g^5 + h^5 = 2; \quad g^5 - h^5 = 1;$$

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\*  $\square_{x,y}$  means the determinant in respect to  $x$  and  $y$ .

whence we have

$$F = x^5 + (x+y)^5 + (x-y)^5.$$

Again, we find

$$\square(4x^3 - 4y^2x) = -4^4 \times 12,$$

$$\left(\frac{1}{(-3)} \square P_3\right)^{\frac{1}{5}} = 4,$$

and accordingly

$$x(x+y)(x-y) = \frac{P_3}{((-3)^{-1} \cdot \square P_3)^{\frac{1}{5}}}$$

according to the general formula above given.

As a second example let

$$F = 3x^7 + 42x^5y^2 + 70x^3y^4 + 14xy^6 + y^7;$$

$$\text{then } P_4 = \left\{ \begin{array}{l} 3x, \ 2y, \ 2x, \ 2y \\ 2y, \ 2x, \ 2y, \ 2x \\ 2x, \ 2y, \ 2x, \ 2y \\ 2y, \ 2x, \ 2y, \ 2x+y \end{array} \right\} = 4(x^3y - xy^3) = 4 \cdot x \cdot y (x-y)(x+y)$$

and accordingly we shall find

$$x^7 + y^7 + (x-y)^7 + (x+y)^7 = F.$$

Moreover

$$\begin{aligned} \square(4x^3y - 4xy^3) &= 4^9 \\ \text{and } 4^{4-2} &= 4^2 \end{aligned}$$

$$\text{Thus } \frac{P_4}{tuvw} = \sqrt[7]{\frac{\square P_4}{4^{4-2}}}$$

agreeable to the general formula.

As a corollary to our general proposition, it may be remarked, that if  $F_{2n-1}$  be a symmetrical function of  $x, y$  of the  $(2n-1)$ th degree,  $P_n(F_{2n-1})$  will be also a symmetrical function of  $x$  and  $y$ , and may therefore be resolved into its factors by solving a *recurring* equation of the  $n$ th degree, which may, by well known methods, be made to depend on the solution of an equation of the  $\frac{n}{2}$ th or  $\left(\frac{n-1}{2}\right)$ th degree, according as  $n$  is even or odd.

Hence the reduction of a function of two letters of the degree  $4m \pm 1$  to its canonical form as the sum of powers may be made to depend on the solution of an equation of the  $m$ th degree; so that, for example, a symmetrical function of  $x, y$ , as high as the fifteenth or seventeenth degree, may be reduced by means of a biquadratic equation only.



In a short time I hope to present to the public a complete solution of the canonical forms of functions of two letters of even degrees, and possibly to exhibit some important applications of the principles of the method to the theory of numbers.

17th May, 1851.

## APPENDIX.

### NOTE (A).

THE permutants (meaning, in Mr. Cayley's language, the hyperdeterminants) of  $F_{2n+1}(x, y)$  of the fourth dimension in respect to the coefficients of  $F$ , may be all obtained by taking the quadratic permutant in respect to  $x$  and  $y$  of the quadratic permutant in respect to  $\xi$  and  $\eta$  of

$$\left( \xi \frac{d}{dx} + \eta \frac{d}{dy} \right)^{2l} \cdot F_{2n+1}(x, y)$$

$r$  having any integer value from 1 to  $n$ .

In extension of a theorem in the foregoing Supplement, which applies only to the case of  $l = n$ , I am able to state the following more general theorem, in which the same notation is preserved as at pages 5 and 6. The quadratic permutant in respect to  $\xi$  and  $\eta$  of

$$\frac{1}{(2n+1)2n \dots (2n-2l+2)} \cdot \left( \xi \frac{d}{dx} + \eta \frac{d}{dy} \right)^{2l} \cdot F_{2n+1}(x, y)$$

is equal to

$$\frac{M^l}{(1, 2)^l} \sum \left\{ (\theta_r, \theta_s)^{2l} (u_r \cdot u_s)^{2n+1-2l} \right\}.$$

If now we proceed to form the quadratic permutant of the above sum in respect to  $x$  and  $y$ , we know *a priori*, by reason of Mr. Cayley's invaluable researches, that we shall not get radically distinct results for all values, but only for certain periodically changing values of  $l$ .

I have not yet had leisure to seek for an explicit demonstration of this remarkable law, founded upon the above given canonical representation.

### NOTE (B).

The lemma, upon which the general method for reducing odd degree functions to their canonical form is founded, may be stated rather more simply and more generally as follows: —

The determinant

$$\begin{array}{ccccccc} T_{r_1} & T_{r_2} & . & . & . & . & T_{r_n} \\ T_{r_1+l_1} & T_{r_2+l_1} & . & . & . & . & T_{r_n+l_1} \\ T_{r_1+l_2} & T_{r_2+l_2} & . & . & . & . & T_{r_n+l_2} \\ . & . & . & . & . & . & . \\ T_{r_1+l_{n-1}} & T_{r_2+l_{n-1}} & . & . & . & . & T_{r_n+l_{n-1}} \end{array}$$

where  $T_\theta$  denotes  $A_1 a_1^\theta + A_2 a_2^\theta + \dots + A_m a_m^\theta$  provided that  $m$  is less than  $n$ , is identically zero. In the theorem, as thus stated, there is no substantial loss of generality arising from the omission of the (b)s.

Thus stated the theorem and its extensions evidently repose upon the same or the like basis as the theory of partial fractions.

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NOTE (C), referring to the original "Sketch."

The Boolo-Hessian scale of determinants furnishes a very pretty general theorem of geometrical reciprocity in connexion with the doctrine of successive polars. Let  $F(x, y, z)$ , a cubic homogeneous function of  $x, y, z$  equated to zero, express in general a curve of the third degree; then  $\left(a \frac{d}{dx} + b \frac{d}{dy} + c \frac{d}{dz}\right) F$  will express its first polar in respect to the point  $a, b, c$ , *i. e.* the conic which passes through the six points in which the tangents drawn from  $a, b, c$  to touch the given curve meet the same.

Again, if we take  $l, m, n$  the co-ordinates of any new point,

$$\left(l \frac{d}{dx} + m \frac{d}{dy} + n \frac{d}{dz}\right) \left(a \frac{d}{dx} + b \frac{d}{dy} + c \frac{d}{dz}\right) F$$

will express the polar, *i. e.* the chord of contact of the above conic, in respect to the last named point. If now we eliminate  $l, m, n$  between the three equations :

$$\left(l \frac{d}{dx} + m \frac{d}{dy} + n \frac{d}{dz}\right) \left(a \frac{d}{dx} + b \frac{d}{dy} + c \frac{d}{dz}\right) F = 0$$

$$\left(l \frac{d}{dx} + m \frac{d}{dy} + n \frac{d}{dz}\right) \left(a' \frac{d}{dx} + b' \frac{d}{dy} + c' \frac{d}{dz}\right) F = 0$$

$$\left(l \frac{d}{dx} + m \frac{d}{dy} + n \frac{d}{dz}\right) \left(a'' \frac{d}{dx} + b'' \frac{d}{dy} + c'' \frac{d}{dz}\right) F = 0$$

it is easily seen that the resultant of the elimination is the square of the determinant

$$\left. \begin{array}{ccc} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{array} \right\}$$

multiplied by the Hessian of the given function. And, moreover, that if we eliminate  $x, y, z$  we shall obtain precisely the same result with the letters  $l, m, n$  substituted for  $x, y, z$ . Hence it follows, that if we take the doubly infinite system of first polars to a given curve of the third degree, in respect to all the points lying in its plane, and then from any point in the *Hessian* to the given curve, draw pairs of tangents to each conic of the system so generated, then all the chords of contact will meet in one and the same point, which will itself be also a point situated upon the Hessian and conjugate to the former.

So, in general, for a function of any degree of any number of letters, viewed with relation to the doctrine of successive polars, the determinants of the Bools-Hessian scale take one another up in pairs; viz., the first takes up the last but one, the second the last but two, and so on; and consequently, if the degree of the function be odd, that function which (making abstraction of the constant determinant at the end) lies in the middle of the scale pairs with itself, and, in a sense analogous to that above exhibited for a function of the third degree, may be said to be always its own reciprocal.

May 24. 1851.

P.S.—I have just discovered the method of reducing functions of two letters of *even degrees* to their canonical form, which will shortly be published in a second Supplement.

At present I offer the annexed theorem (which strikingly contrasts with the law of uniqueness demonstrated of functions of an odd degree) as a foretaste of the enchanting developments with which I hope shortly to present my readers:—

If a given homogeneous function of  $x$  and  $y$  of the degree  $2n$  be supposed to be thrown under its canonical form,

$$u_1^{2n} + u_2^{2n} + \text{&c.} + u_n^{2n} + K(u_1 u_2 \dots u_n)^2,$$

then will  $K$  have  $n^2 - 1$  in general distinct values, to each of which will correspond a single distinct system of the linear functions of  $x$  and  $y$ ,

$$l^{\frac{1}{n}} u_1, l^{\frac{1}{n}} u_2, \dots \dots \dots l^{\frac{1}{n}} u_n.$$

ERRATUM IN THE SKETCH.—At page 198., fourth line from bottom, of the "Cambridge and Dublin Mathematical Journal," p. 13. of extract from same, for " $u^3 + v^3 + w^3 + p^3 + q^3$ ," lege " $u^3 + v^3 + w^3 + p^3 + q^3$ ."

THE END.

# REPLY TO PROFESSOR BOOLE'S OBSERVATIONS ON A THEOREM CONTAINED IN THE LAST NOV. NUMBER OF THE JOURNAL.

By J. J. SYLVESTER, M.A., F.R.S.

[Extracted from the *Cambridge and Dublin Mathematical Journal*, May, 1851.]

THE restricted space that can be spared for discussion in these pages, necessitates me to compress within the narrowest limit the remarks which I feel bound to make on Mr. Boole's extraordinary observations in the February number of this Journal, on my theorem contained in the antecedent number thereof, which statements I cannot, in the interests of truth and honesty, suffer to pass unchallenged. The object of that theorem was to shew how the determinant of the quadratic function resulting from the elimination of any set of the variables between a given quadratic function and a number of linear functions of the same variables, could be represented *without performing* the actual elimination by a fraction, of which the numerator would be constant whichever set of the variables might be selected for elimination, and the denominator the square of the determinant corresponding to the coefficients of the variables so eliminated. The numerator itself is a determinant, obtained by forming the square corresponding to the determinant of the given quadratic function, and bordering it horizontally and vertically with the lines and columns corresponding to the coefficients of all the variables in the given linear equations. An *immediate corollary* from this theorem leads to Mr. Boole's. Conversely upon the principle that "tout est dans tout" Mr. Boole devotes a page and a half of close print merely to indicate the steps of a method by which from his theorem mine is capable of being deduced, ending with the announcement, that the numerator in question is equal to the quantity

$$\phi_1 \phi_2 \dots \phi_r \theta(Q), \quad ;$$

(the symbols above employed being Mr. Boole's own,) and concludes with assuring his readers that "he has ascertained that Mr. Sylvester's result is reducible to the above form." Mr. Sylvester would be very sorry to put his result under any such form. Mr. Boole could scarcely have reflected



upon the effect of his words when he indulged in the remark which follows—"there cannot be a doubt that for the discovery of the actual relation in question, the above theorem is far more convenient than Mr. Sylvester's." Of the value to be attached to this assertion the annexed comparison of results is submitted as a specimen.

Let the quadratic function be

$$ax^2 + by^2 + cz^2 + dt^2 + 2exy + 2\epsilon zt + 2gxz + 2\gamma yt + 2hyz + 2\eta xt,$$

and the linear functions (taken two in number)

$$lx + my + nz + pt,$$

$$l'x + m'y + n'z + p't.$$

My numerator will be the determinant (hereinafter cited as the *extended* determinant),

$$\begin{array}{cccccc} a & e & g & \eta & l & l', \\ e & b & h & \gamma & m & m', \\ g & h & c & \epsilon & n & n', \\ \eta & \gamma & \epsilon & d & p & p', \\ l & m & n & p & o & o, \\ l' & m' & n' & p' & o & o. \end{array}$$

To find the numerator of Mr. Boole's fraction, we must form the symbolical operator

$$\begin{aligned} & \left\{ l^2 \frac{d}{da} + m^2 \frac{d}{db} + n^2 \frac{d}{dc} + p^2 \frac{d}{dd} \right. \\ & \left. + 2lm \frac{d}{de} + 2np \frac{d}{d\epsilon} + 2lm \frac{d}{dg} + 2mp \frac{d}{d\gamma} + 2lp \frac{d}{dh} + 2mn \frac{d}{d\eta} \right\} \\ & \times \left\{ l'^2 \frac{d}{da} + m'^2 \frac{d}{db} + n'^2 \frac{d}{dc} + p'^2 \frac{d}{dd} \right. \\ & \left. + 2l'm' \frac{d}{de} + 2n'p' \frac{d}{d\epsilon} + 2l'n' \frac{d}{dg} + 2m'p' \frac{d}{d\gamma} + 2l'p' \frac{d}{dh} + 2m'n' \frac{d}{d\eta} \right\} \end{aligned}$$

and after expanding the determinant here under written,

$$\begin{array}{cccc} a & e & g & \eta \\ e & b & h & \gamma \\ g & h & c & \epsilon \\ \eta & \gamma & \epsilon & d, \end{array}$$

perform the operations above indicated upon the result so obtained.

These are the operations and processes which, on Professor Boole's authority, we are to accept "*as without doubt far more convenient*" than the one simple process of forming, and when necessary, calculating the extended determinant above given. Here for the present I leave the case between Mr. Boole and myself to the judgment of the readers of this Journal.

In the April number of the *Philosophical Magazine*, I have shown that the extended determinant serves, not only to represent the full and complete determinant of the reduced quadratic function, but likewise all the minor determinants thereof; the last *set* of which will be evidently no other than the coefficients themselves. For instance, in the example above given, if we wish to find the coefficient of  $x^2$  after ( $z$ ) and ( $t$ ) have been eliminated, we have only to strike out the line and column  $e b h g m m'$  from the extended determinant; if we wish to find the coefficient of  $y^2$ , we must strike out the line and column  $a e g \eta l l'$ ; to find the coefficient of  $xy$ , we must strike out the line  $a e g \eta l l'$  and the column  $e b h g m m'$ , or *vice versa*.

In each of these cases the determinant so obtained is the numerator of the equivalent fraction; the denominator remaining always the same function of the coefficients of transformation as in the original theorem.

Again, if there be taken only one linear equation, and by aid of it  $x$  is supposed to be eliminated; and if the reduced quadratic function be called

$$Ly^2 + Mz^2 + Nt^2 + 2Pzt + 2Qyt + 2Rzy,$$

the same extended determinant as before given will serve, when stripped of its outer border, consisting of the line and column  $l' m' n' p'$ , to produce the various equivalent fractions: thus form the square

$$\begin{array}{ccc} L & R & Q \\ R & M & P \\ Q & P & N. \end{array}$$

The numerator of the fraction equivalent to  $\frac{L R}{R M}$ , i.e. to  $LM - R^2$ , may be found by striking out from the form of the extended determinant the line and column  $\eta \gamma \epsilon \delta p$ ; that corresponding to  $\frac{L Q}{R P}$ , i.e.  $LP - RQ$ , will be found by striking out the line  $g h c \epsilon n$  and the column  $\eta \gamma \epsilon d p$ , or *vice versa*; and so forth for all the first minor deter-

minants; and similarly the second minors, i. e.  $L, M, N, P, Q, R$ , may be obtained by striking out in each case a correspondent pair of lines and pair of columns. Thus, to find the numerator of  $L$  the same pair of lines and columns, viz.  $(g\ h\ c\ \epsilon\ n), (\eta\ \gamma\ \epsilon\ d\ p)$ , must be elided. To find the numerator of  $R$ , the pair of lines  $(g\ h\ c\ \epsilon\ n), (\eta\ \gamma\ \epsilon\ d\ p)$ , and the pair of columns  $(e\ b\ h\ \gamma\ m), (\eta\ \gamma\ \epsilon\ d\ p)$ , or *vice versâ*, will have to be elided; and so forth for the remaining second minors. I may conclude with observing, that the theorem contested by Mr. Boole is an immediate corollary from the general Theory of Relative Determinants alluded to in the "Sketch" inserted in the present number of the *Journal*.

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ON THE

DIURNAL VARIATIONS

IN THE

DECLINATION OF THE MAGNETIC NEEDLE,

AND IN THE INTENSITIES OF THE

HORIZONTAL AND VERTICAL MAGNETIC FORCES.

BY

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Professor of Mathematics and Natural Philosophy in Delaware College.

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EXTRACTED FROM THE AMERICAN JOURNAL OF SCIENCE AND ARTS, VOL. VIII,  
SECOND SERIES, 1849.

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IN a memoir published in vol. iv of this Journal,\* I gave an exposition of a new theory of Terrestrial Magnetism, of which the following are the fundamental principles: 1. Every particle of matter at the earth's surface, and to a certain depth below the surface, is the centre of a magnetic force exerted tangentially to the circumference of every vertical circle that may be conceived to be traced around it. 2. The direction of this force is different, according as it solicits the north or south end of the needle; and

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\* Pages 1 to 12 and 207 to 230.



it is always such, that to the north of the acting particle the tendency is to urge the north end of the needle downward and the south end upward, and that to the south of the same particle it is to urge the north end upward and the south end downward.

3. The intensity of the magnetic force of a particle of the earth, at a given distance, is approximately proportional to its temperature, or amount of sensible heat; and at increasing distances, diminishes according to some unknown law. I was conducted to these principles by the theory which I had been led to adopt concerning the physical nature of the Imponderables: which is, that all the phenomena of the imponderables are but different effects of different vibratory motions of the particles of matter, and of the ethereal undulations produced by these vibrations. I accordingly conceived each particle of the earth's mass to be the centre of a system of undulatory movements propagated through the surrounding ether, and of every variety of time and intensity of vibration within certain limits—waves of light, heat, and magnetism. The vibrations of the ethereal particles, in a wave of magnetism, I supposed to be in the surface of the wave, or transversal to the line of propagation of the wave, as is known to be the case with a wave of light, and I regard the magnetic forces as probably due to these transversal vibrations. I was thus led to consider the sun as the probable source, at the same time, of waves of heat, light, and magnetism, and that the molecular forces of vibration due to the different kinds of waves would probably vary according to the same law, or approximately so, in passing from one point to another on the earth's surface, and accordingly that the temperature of a particle might be taken as a measure of its magnetic force. Although I was thus conducted, by these physical speculations, to the fundamental principles of what may be characterized as the Thermal Theory of Terrestrial Magnetism, these principles may nevertheless have no real connection with the physical theory in which they originated. The tangential magnetic forces which I suppose, may be due to electric currents or may be fundamental properties of matter. The investigations of this and the previous memoir, conclusively establish the fact of the existence of these forces, and of their supposed connection with the thermal state of the earth, but are in no way essentially dependent upon any physical speculations concerning their origin. These form a debatable ground beyond the thermal theory which I have undertaken to develop and follow out into some of its consequences, about which I do not at present concern myself.

From the fundamental principles which I have stated, I deduced, in the memoir referred to, three simple formulæ; one, for the horizontal component of the directive force of the needle, or the horizontal magnetic intensity of the place; a second, for the

vertical intensity; and a third, making known the declination. These formulæ were afterwards tested by numerous comparisons with the results of observations made in every variety of locality in the northern hemisphere of the earth. The agreement was found to be very close—the differences amounting only to a few hundredths for the horizontal and vertical forces, and less than  $2^{\circ} 40'$ , and in most cases less than  $1^{\circ}$  for the declination. The positions of the magnetic poles, the pole of maximum intensity, and the magnetic equator were also theoretically deduced, and shown to correspond very closely with their observed positions. In view of the whole discussion the following great truths were supposed to have been established.

1. All the magnetic elements of any place on the earth may be deduced from the thermal elements of the same; and all the great features of the distribution of the earth's magnetism may be theoretically derived from certain prominent features in the distribution of its heat.

2. Of the magnetic elements, the horizontal intensity is nearly proportional to the mean temperature, as measured by a Fahrenheit's thermometer; the vertical intensity is nearly proportional to the difference between the mean temperatures at two points situated at equal distances north and south of the place, in a direction perpendicular to the isogeothermal line (that is, a line conceived to be traced through all points at which the mean temperature of the matter of the earth, near its surface, is the same as at the station of the needle): and, in general, the direction of the needle is nearly at right angles to the isogeothermal line, while the precise course of the inflected line to which it is perpendicular may be deduced from Brewster's formula for the temperature, by differentiating and putting the differential equal to zero.

3. As a consequence, the laws of the terrestrial distribution of the physical principles of magnetism and heat must be the same, or nearly the same; and these principles themselves must be physically connected in the most intimate manner.

4. The principle of Terrestrial Magnetism, in so far as the phenomena of the magnetic needle are concerned, must be confined to the earth's surface, or to a comparatively thin stratum of the mass of the earth.

5. The mechanical theory of terrestrial magnetism which has been under discussion, must be true in all its essential features.

6. We may derive the magnetic elements by very simple formulæ, and with an accuracy equal to that of Gauss's formulæ, from a very small number of magnetic data determined by observation, and the mean annual temperature of the place.

From the theoretical investigation of the normal state of the terrestrial magnetic elements, I propose, in the present article, to

proceed to the discussion in the light of the same theory, of their Diurnal Variations. This theory furnishes us the following general principles as a basis for this discussion. 1. The horizontal magnetic intensity of a place is proportional to its temperature. 2. The vertical intensity is proportional to the difference between the temperatures of two places situated at equal distances north and south of the isogeothermal line, in a direction perpendicular to it. 3. The direction of the needle is nearly perpendicular to the isogeothermal line. From these general principles we may draw the general conclusions, that the variations of the horizontal and vertical magnetic intensities must be linked to the variations of the temperature of the station of the needle and of the differences of temperature of places north and south of this, and that the variations of declination must be connected with the variations in the position of the ideal line passing through all places which have the same actual temperature as the given place; which line may be called the *true* isogeothermal line. If the latter conclusion be true, it may be added that the variations of declination must also be connected with the variations in the differences of temperature of places situated to the East and West of the station of the needle.

The data for the detailed discussion have been chiefly taken from the Report of the "Observations at the Magnetic and Meteorological Observatory at the Girard College, Philadelphia, made under the direction of A. D. Bache, LL.D., 1840 to 1845," and the Report of the "Magnetical and Meteorological Observations, made at Washington by Lieutenant J. M. Gilliss, U.S.N., dated August 13th, 1838." The first Report contains a complete series of Magnetic and Meteorological Observations, generally either bi-hourly or from hour to hour, extending from June, 1840, to June, 1845, besides Term Day Magnetic Observations, and Extraordinary Observations. Tables of the daily, monthly, quarterly, semi-annual, and annual means, and of the hourly or bi-hourly means for months, are given; and curves traced exhibiting these results to the eye, and showing the Extraordinary and Term Day Observations. The second Report comprises a Journal of Meteorological Observations made at four different hours during the day, (3 A.M., 9 A.M., 3 P.M., 9 P.M.,) kept from July, 1838, to June, 1842; a set of bi-hourly meteorological observations and observations of declination extending from June, 1840, to July, 1842; Term Day and Extraordinary Observations; and occasional observations of the dip of the needle. Tables of abstracts are also given, and curves showing the variations of declination and temperature on the term days.



*Diurnal Variations of the Horizontal Magnetic Intensity.*

The formula which the Thermal Theory of Magnetism has furnished for the horizontal magnetic intensity of a place is

$$H = C' T$$

in which  $C'$  is a constant, and  $T$  the mean annual temperature of the place. This formula is equivalent to the statement that the mean horizontal magnetic force is proportional to the mean temperature. We have therefore to compare the diurnal variations of the horizontal force with the diurnal variations of the temperature of the place. The theory strictly requires that the comparison should be with the daily variations in the absolute amount of sensible heat near the earth's surface, but we know, from the laws of the heating and cooling of bodies, that when the temperature is rising at its surface the earth is, in general, receiving more heat than it loses, and that when the temperature at the surface is falling, it is losing more heat than it receives—so that a rise or fall of surface temperature will in general indicate an increase or decrease of the total amount of heat. This consideration suffices for the enquiry which first arises, viz.: whether the horizontal force increases and decreases with the total amount of heat. A good set of observations of the daily variations of the temperature below the surface would be required for a thorough and minute discussion of the subject before us, but the facts already known and the established theory of the heating and cooling of bodies appear to supersede the necessity of such observations, except when the attempt is to be made to obtain precise quantitative determinations.

We will begin by comparing the curve showing the mean daily variation of the horizontal intensity at Philadelphia for the year 1844 (fig. 3), with the curve showing the mean daily variation of temperature for the same year (fig. 8).

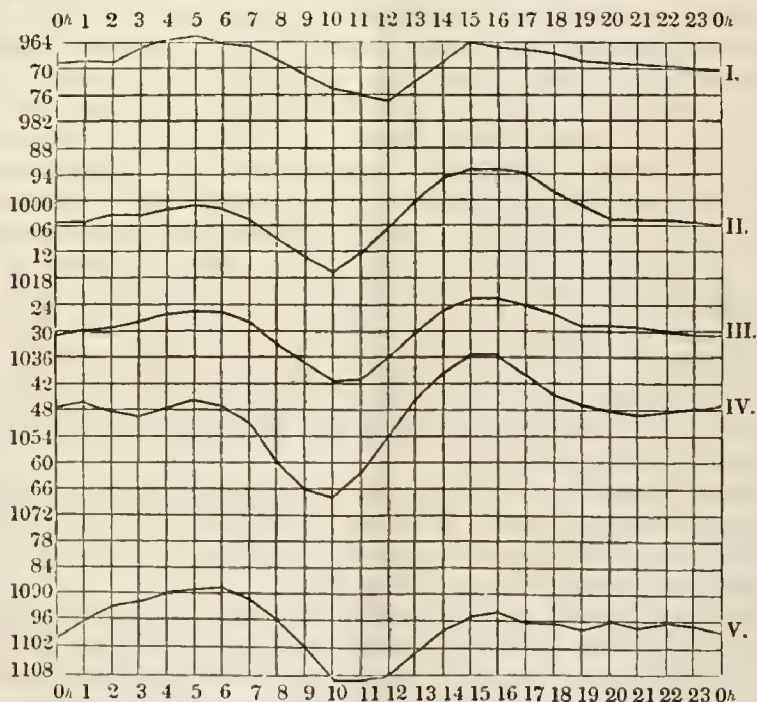
It will be observed that the horizontal intensity attains its maximum at from 15<sup>h</sup> to 16<sup>h</sup>, or from 3 to 4 P. M., and that the maximum temperature occurs at the same hour;—also that the horizontal intensity increases with the temperature in the forenoon (after 10<sup>h</sup>), and decreases with it in the afternoon and evening. The same correspondences are observable in the curves for the other years and for the quarters of years, with the single qualification, that the maximum of horizontal intensity sometimes occurs an hour or two later than the maximum of temperature. They are an indication that the daily variation of temperature is, in all probability, at least one cause of the variation of horizontal intensity. When we compare the curves still farther we notice the following points of difference between them. 1. The horizontal force increases during the latter half of the night until 5 to 6 A. M., and then decreases until 10 A. M., whereas the temperature



falls steadily until from 5 to 6 A. M., (the hour of the second maximum of horizontal intensity,) and after that begins to rise. Thus the one curve has two maxima and two minima, and the other one maximum and one minimum. 2. While the temperature falls in the afternoon and evening as rapidly as it rises in the forenoon, the horizontal force decreases less rapidly during the former period than it increases during the latter; and at the same time, as already intimated, the maximum of horizontal intensity frequently occurs an hour or two later than the maximum of temperature.

Figs. 1, 2, 3, 4, 5.

Curves of the Mean Diurnal Variations of the Horizontal Force for 1844 and the different quarters of the same year.



I. Jan., Feb. and March.—II. April, May and June.—III. 1844.—IV. July, Aug. and Sept.—V. Oct., Nov. and Dec.

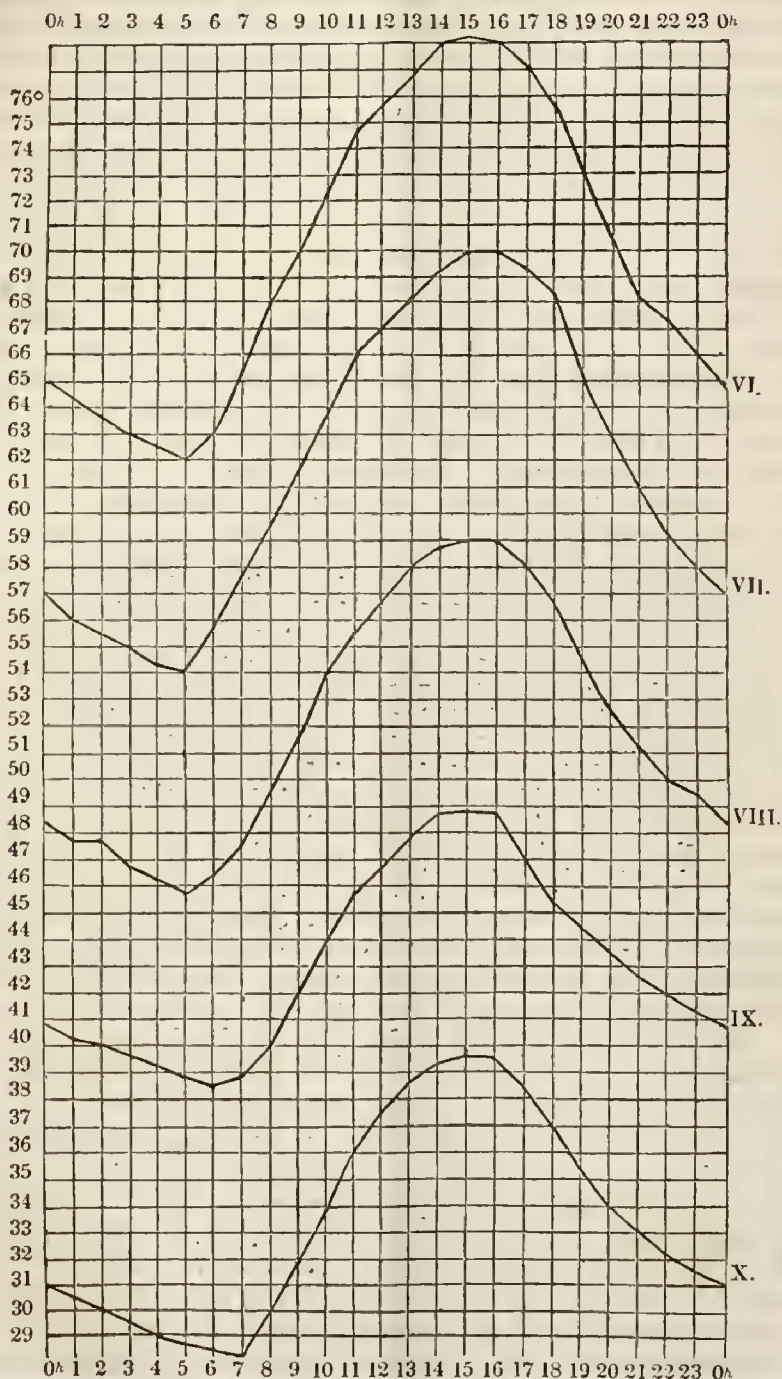
Increase of numbers corresponds to decrease of force. One division of Magnetometer scale = .000036 of horizontal force.

How are we to account for these discrepancies between theory and fact—these deviations of the actual daily variations of the horizontal force from the theoretical variations? To distinguish them from those which have just been considered, and which accord with our theory, let us call them the *secondary variations* of the horizontal force, without meaning thereby to intimate that they are of minor importance. The inevitable in-

ference from the first mentioned fact, is that if the daily variation of temperature is one cause of the daily variation of the horizontal force, there must also be some other cause in operation besides the mere variation of temperature. Is this cause one entirely unconnected with the variation of temperature, or is it some indirect effect of this variation? The Newtonian principle of not multiplying causes would prompt us to try the latter supposition. Besides a connection exists between the time of the secondary maximum of horizontal intensity and the time of maximum temperature, which serves to render it probable that this supposition is the true one. This connection may be inferred not merely from their coincidence in the mean curves for the year, but also from their approximate coincidence in the different curves for the quarters of the year (see figs. 1, 2, 3, &c., to 10);—in other words, from the fact that the time of the secondary maximum of horizontal intensity moves forward and backward, during the year, with the time of sunrise. It should be observed, however, that this fact is less distinctly shown by the curves for some years than for others. It has been recognized by Professor Loyd, in his observations at Dublin. He says, “The epoch of the morning maximum moves forward as the time approaches the winter solstice, appearing to depend upon the hour of sunrise which it precedes by a short interval.” The manifest inference from this connection is that the increase of the horizontal force during the latter part of the night, when the temperature is on the decrease, and the decrease of this force for several hours after sunrise, when the temperature is on the increase, are in all probability indirect effects of the change of temperature. While making a hasty comparison of the daily variations of the horizontal force with the theory that its intensity varies with the temperature, about a year since, it occurred to me that the secondary changes of this force, just noticed, were probably due to the deposition of condensed vapor from the atmosphere during the night, and the evaporation which immediately succeeds in the morning. These are well established effects of the daily fall and rise of temperature. The tendency of the deposition of vapor that goes on while the temperature is falling, must be to augment the horizontal force, and the tendency of the evaporation of the dew that falls at night, produced by the heat of the sun in the morning, must be to diminish this force. The deposition of vapor must tend to increase this force in two ways; viz., by the heat given out in the act of condensation, and by adding to the amount of matter at the earth’s surface which acts upon the needle. The evaporation must also tend to diminish the force in two ways; viz., by the loss of sensible heat accompanying the vaporization, and by the loss of a certain amount of matter, from the earth’s surface, whose horizontal magnetic action had formed a part of

Figs. 6, 7, 8, 9, 10.

Curves of the Mean Diurnal Variations of the Temperature, for 1844, and the different Quarters of the same year.



VI. July, Aug. and Sept.—VII. April, May and June.—VIII. 1844.—IX. Oct., Nov. and Dec.—X. Jan., Feb. and March.



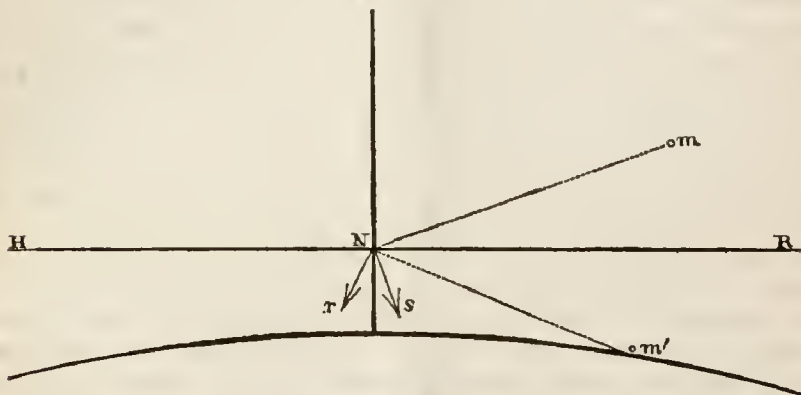
the horizontal force. It would seem that the increase of the horizontal force during the night and decrease in the morning, cannot be attributable solely to the heat given out by condensation and abstracted by vaporization, since, notwithstanding the deposition of vapor at night and the evaporation in the morning, the temperature continues to fall until morning, and after that rises steadily during the forenoon:—unless it should chance that a considerable portion of the heat evolved by the condensation penetrates below the surface, so as to augment the average temperature of the stratum which is subject to daily variations of temperature, and that, in like manner, the cooling due to evaporation lowers the average temperature of this stratum, at the same time that the surface temperature rises. I am disposed, therefore, to attribute the secondary variations of the horizontal intensity, under consideration, chiefly to the variations in the quantity of condensed vapor at the earth's surface, attending the fall and rise of temperature. But, whether the morning maximum and minimum are principally effects of variations in the quantity of magnetic matter in action, or of variations in the absolute amount of sensible heat due to the fall and rise of vapor, the effect in a given time, will on either supposition, be proportional to the amount of vapor deposited or of water evaporated. When we consider the entire secondary variations during the night, it is to be observed that, upon the view which I have adopted, the fall of vapor has two effects; it diminishes the rate of cooling of the earth, by the heat evolved, which makes the decrease of horizontal force less, and it augments the quantity of magnetic matter in action, which makes the diminution of this force still less, or converts the decrease into an increase, according to the amount of vapor deposited. In like manner the evaporation after sunrise has two effects. If any heat be evolved or abstracted, in addition to that connected with the variations of temperature at the surface produced by the rise and fall of vapor, it will conspire with the changes in the quantity of condensed vapor acting upon the needle, to produce an augmentation of the horizontal force during the forenoon.

Before presenting the considerations which I have to urge in support of the theory which I have now advanced, let me consider an objection which has probably occurred to the reader; viz., that the quantity of vapor that falls during the night is altogether too trifling to produce an effect so considerable—to more than neutralize the effect of the decrease of temperature; and, in like manner, that the loss of matter from the surface of the earth, by reason of the evaporation in the forenoon, is too small, in comparison with the whole amount of matter which has a variable action upon the needle, to have the effect attributed to it.



In answer to this objection, I have to offer the following considerations. 1. It is to be observed that the entire horizontal magnetic force of the vapor is added or abstracted with it, while the horizontal force is otherwise affected only by the variations of temperature. 2. The force of the vapor, which, from a situation above the needle falls below it, is changed from a diminishing to an increasing action. This will be readily seen on referring to fig. 11; in which N represents the position of the needle,

Fig. 11.



and H N R a horizontal line at the height of the needle and situated in the plane of the magnetic meridian. A particle  $m$ , in this plane will exert its force in the direction  $Ns$ , and therefore tend to diminish the horizontal force (R being supposed to be south of N), but when it falls to the position  $m'$  it acts in the direction  $Nr$ , and therefore now tends to increase the horizontal force. It is here taken for granted that a particle continues to act magnetically after it has left the earth's surface. It is only by a detailed discussion that we can determine whether this supposition be true or not. It is enough, for our present purpose, that it is not at variance with the theory. 3. The depth to which any considerable daily variations—or at least variations which indicate a change in the absolute amount of heat—extend, does not probably exceed nine inches. For, according to the observations of Quetelet, Director of the Observatory at Brussels, made from 1834 to 1839, the velocity of propagation of the diurnal variations of temperature is less than  $1\frac{1}{2}$  inches per hour, or less than 18 inches in twelve hours; and, the variations of temperature will be very much less below the depth of nine inches than above it. We may form some estimate of the difference from the following statement of the annual variations of temperature at various depths, given by Quetelet. The depths are in metres, and the degrees Centigrade.

Depth.—Metres.	Annual Variations.
0.19 . . . . .	13° 28
0.45 . . . . .	12 .44
0.75 . . . . .	11 .35
1.00 . . . . .	10 .58
1.95 . . . . .	7 .59
3.90 . . . . .	4 .49
7.80 . . . . .	1 .13

They become entirely imperceptible at the depth of about 24 m. If we suppose the daily variations of temperature to decrease from the surface downwards at the same proportionate rate, it will become less than 1° F. at the depth of 9 in., (taking the variation at the surface at 12° F., which is about the average for the year.)

4. Although the amount of vapor deposited during the night, may perhaps not exceed the  $\frac{2}{7 \cdot 5 \pi}$  of an inch, on the average, the loss of its entire amount of sensible heat may still be more than an equivalent for the daily variations of temperature of a depth of six or nine inches of soil.

In view of these statements it will not be deemed idle to enquire whether the alternate deposition and rise of vapor may not afford an adequate explanation of the secondary variations of the horizontal force. But before entering upon this enquiry, let us go back, and following the indications of the observations endeavor to ascertain whether there is any known phenomenon that satisfies the prominent conditions which they furnish. As preparatory to this it is important to state the laws according to which heat is radiated from the earth's surface into space, and propagated from particle to particle below the surface. These are as follows,

1. The loss of heat, from nocturnal radiation, in a calm clear night, is uniform at all temperatures. This law, we are told (see the No. of this Journal for November, 1848, p. 420), has recently been announced by Wilson, and has since been confirmed by the Observations of Melloni. It is, moreover, in approximate accordance with the general theory of radiation. The formula which this theory, in conjunction with experiment, has furnished, for the velocity or rate of cooling of a body by radiation is

$$V = ma^{t+\theta} - ma^{\theta} \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

in which  $\theta$  denotes the absolute temperature of the enclosure, or external medium, toward which the body radiates, and  $t$  the excess of the temperature of the body over that of the enclosure. If  $\theta$  and  $t$  are expressed in Centigrade degrees, then  $a = 1.0077m$ , for a vitreous surface, is 2.037; and it is about the same for the soil, which has about the same radiating power as glass. According to Pouillet the temperature of space is about  $-142^{\circ}$  C.

If we denote the temperature of the earth, in Centigrade degrees, by  $T$ , we shall have for the velocity of cooling of the earth, by radiation into space,

$$V = 2.037 \left( (1.0077)^T - \frac{1}{(1.0077)^{1.42}} \right) = 2.037 \left( (1.0077)^T - .339 \right)$$

According to this formula the velocity of cooling of the earth's surface at  $0^\circ \text{C.}$ , ( $32^\circ \text{F.}$ ) is to the velocity of cooling at  $15^\circ \text{C.}$ , ( $59^\circ \text{F.}$ ) as 67 to 78—that is, the rate of cooling diminishes, from  $59^\circ \text{F.}$  to  $32^\circ \text{F.}$ ,  $\frac{1}{7}$  of its amount at  $59^\circ$ .

Formula (1) was obtained by a comparison of theory with the experiments of Petit and Dulong. It may well happen that when we come to apply it to a case in which the temperature of the medium exterior to the cooling body is far below the range of the experiments, it will not give exact results.

In the application of this formula, I have supposed the radiation of the earth to be directly into free space. As a matter of fact, it is through the atmosphere, and therefore the rate of cooling of the earth must depend upon the mean temperature and also the absorptive action of the atmosphere. To apply formula (1) to the case of the earth,  $\theta$  should therefore be taken equal to the temperature of the sky, instead of the temperature of space: or, we may introduce into the formula another subtractive term, representing the emissive power or absolute radiation of the atmosphere. We must also allow for the absorption of heat by the atmosphere. Pouillet estimates this at a little less than  $\frac{1}{2}$ . Sup-

posing it to be  $\frac{1}{2}$ , and also that  $\frac{1}{n}$  of the heat radiated downward from the air reaches the surface of the earth, and denoting by  $t$  the mean temperature of the air, we have

$$V = ma^T - \frac{ma^\theta}{2} - \frac{ma^t}{n} \quad . \quad . \quad . \quad . \quad (3.)$$

It appears from the observations made by Pouillet with the actinometer, that the mean temperature of the atmosphere is about  $35^\circ \text{C.}$  below the temperature of the air, and falls, during the night, at about the same rate as the temperature at the earth's surface. Taking this result, and making the calculations for  $n=1$ , we find the velocities of cooling at  $32^\circ \text{F.}$  and  $59^\circ \text{F.}$  to be to each other as 77 to 86, or that the diminution is a little more than  $\frac{1}{10}$  of the velocity of cooling at  $59^\circ \text{F.}$  According to Pouillet, the absorptive power of the entire atmosphere for terrestrial heat is greater than 0.8, but as the heat radiated downwards from the atmosphere passes only through a portion of it, the value of  $\frac{1}{n}$  is doubtless less than 0.8. It is to be taken into account



also, that the difference between the mean temperature of the air and the temperature at the earth's surface appears to be, in general, from  $3^{\circ}$  to  $5^{\circ}$  C. lower at the end than at the beginning of a night. This being done, and  $\frac{1}{n}$  being taken equal to 0.5, the rates of cooling at the beginning and end of a night, upon which the thermometer falls from  $59^{\circ}$  to  $32^{\circ}$ , are found to be nearly as 47 to 46, or very nearly equal.

The experiments of Melloni and others have established, "that the portion of the sky concerned in the radiation is included within  $30^{\circ}$  to  $35^{\circ}$  of the zenith ;—that clouds beyond this have but little interfering effect."

The air cools, by radiation into space and the upper regions of the atmosphere, like the surface of the earth, by radiation to the earth's surface which cools more rapidly than the air from its superior radiating power, by contact with the earth and objects connected with the earth, and by condensation. The difference between the temperature of the earth's surface and of the air at the height of four or five feet may amount to several degrees. The most recent experiments upon nocturnal radiation, viz., those of Melloni, have established, "that while under certain circumstances some bodies can be cooled to  $8^{\circ}$  C. below the temperature of the air four or five feet above, in general, the effect of radiation is to reduce the temperature of vegetation, &c., not more than  $2^{\circ}$  below that of the surrounding air." Ordinarily the agitations of the air will be sufficient to establish very nearly an equilibrium of temperature between it and the earth's surface.

2. Heat is propagated from one particle to another of the earth's mass by ordinary radiation. Hence when two contiguous particles have the same temperature they exchange, by reciprocal radiation, equal quantities of heat, and when their temperature is different, the one will gain and the other lose, in a given time, an amount of heat proportionate to their difference of temperature. The rate at which this gain or loss takes place in any body, constitutes its conductivity. If we conceive the matter near the earth's surface to be divided into layers of particles, of indefinitely small thickness, in the cooling of the earth at night the heat lost by any one layer is gained by that next above it. The flow of heat from below upward will diminish the fall of temperature at the surface of the earth ; and the velocity of flow will be proportionate to the difference of temperature of the first and second layer. As the cooling goes on this difference will increase, and at the same time the difference between two consecutive differences will become less, and therefore the velocity of cooling will diminish. If the night were to continue for an indefinite time, this change would go on until the first differences attained to a maximum value, and the second differences became zero, when



the earth would have attained to a "movable equilibrium" of temperature. Pouillet has calculated that this would be the case at the temperature of  $-89^{\circ}\text{C}$ . Taking the fundamental principle that one layer gains what the next below it loses, we can derive a very simple formula, connecting the loss of heat at the surface of the earth in a given time with the losses of temperature of the different layers during the same interval of time. Let  $L$  = absolute loss of heat at the earth's surface, in a given time, by nocturnal radiation;  $l, l', l'', \&c.$ , denote the losses of temperature of the first, second, third, &c. layers in the same time;  $a, a', a'', \&c.$ , the quantity of heat received by the successive layers from the layer next below. Then  $L = l + a$ ,  $a = l' + a'$ ,  $a' = l'' + a''$ , &c., and hence

$$L = l + l' + l'' + l''' + \&c. \quad (4.)$$

The losses of temperature,  $l, l', l'', \&c.$ , of the different layers decrease with the depth, and for a night of twelve hours become zero at the depth of about eighteen inches. After sunrise, when the temperature at the surface is rising, the losses of temperature will still continue with the layers below the surface until the heat propagated downward reaches them in succession, and the cooling will gradually extend below 18 in., but these variations of temperature are not attended with any absolute loss of heat. It appears from observation that the law of decrease of the annual variations of temperature at different depths is that of a geometrical progression for depths which increase in an arithmetical progression. The same law probably holds good for the entire diurnal variations. The losses during the night simply probably decrease more rapidly, for, while the entire fall of temperature at and near the surface is about the same as for the night, lower down it is greater, and for a number of inches below 18 in. takes place during the day.

It is to be observed that formula (4) is equally applicable if we suppose  $L$  to represent the absolute loss of heat from the combined action of all the causes which affect the temperature of the surface. The same relation also obtains between the gain of heat during the day and the increments of temperature at different depths.

Since the loss of heat,  $L$ , by nocturnal radiation from the earth's surface is the same at all temperatures, for any given interval of time, it follows that whatever may be the variations of  $l, l', l'', \&c.$  during the night, the actual loss of heat from the whole stratum which undergoes a daily variation of temperature, occasioned by nocturnal radiation, is uniform at all temperatures. Accordingly any variations of  $l$  that may arise from the flow of heat towards the surface cannot be the cause of the observed variations of the horizontal magnetic intensity during the night.

Notwithstanding such variations of  $l$ , the horizontal force, as it depends only on the absolute quantity of heat, would decrease uniformly during the night.

Having laid down these general principles, let us enter upon the general enquiry to which the statement of these principles is preliminary. If we compare the nocturnal variations of the horizontal force with those of the temperature, as shown by the curves, (see figs. 1, 2, &c. to 10,) we find that the deviations from uniformity in the diminution of the horizontal force are attended with like deviations in the fall of the thermometer:—thus, while the thermometer falls less and less rapidly as the night advances, the diminution of the horizontal force becomes less and less; also, while during the fall and winter months the fall of temperature during the night is materially less than in the spring and summer months, the nocturnal diminutions of the horizontal force are less. It is true that the diminution of the horizontal force gradually passes into an increase, but this is only the result of a certain increase in the amount of the deviation from uniformity of diminution. The deviations therefore are of the same character—lie continually in the same direction—for the temperature and horizontal force. They are also cotemporaneous; they differ only in proportionate amount. We have therefore to seek for the cause of the secondary variations of the horizontal magnetic intensity of a place in the cause, whatever it may be, of the changes in the rate of diminution of the temperature during the night, and from one season to another. The former cause is probably identical or closely connected, physically, with the latter, but it is possible that the connection may be more or less remote, or even that the correspondence between their variations is accidental. Now the cause of the nocturnal inequality of temperature must be found in some phenomenon or fact connected either with the relations of the surface of the earth to the atmosphere, or with its relations to the matter below the surface. If it be a meteorological phenomenon, we may suppose it to consist in variations in the clearness of the sky, in the quantity of rain, in the quantity of dew, or, speaking more generally, of vapor deposited in other forms than that of rain, in the direction and force of the wind, and in the amount of heat absorbed by the atmosphere or exchanged with it. We may reject the phenomenon last mentioned at once; for, as we have seen, in a calm clear evening the nocturnal radiation is uniform at all temperatures, and this radiation is the actual radiation, one element of which is the exchange of heat with the atmosphere, and another the absorptive action of the atmosphere. If it be surmised that it is only the maximum radiation on clear evenings that is uniform at all temperatures, such is not the statement of the law, and if it were, then the uniformity must be independent of the density

of the air and of the quantity of vapor suspended in it; and if it be independent of the density of the air it must be independent of the density of the vapor, so long as this retains the aeriform state. It is also independent of any differences that may subsist between the temperatures and densities of the different strata of the atmosphere, for it is a well established fact that the temperature falls more rapidly, as we ascend in the atmosphere, in a summer than in a winter night. In other words, it holds good whatever may be the state of all the various particulars upon which the absorptive action of the atmosphere when transparent, can be supposed to depend.

As for the relative clearness of the sky, I find, on referring to the Report of the Meteorological Observations made at Philadelphia, that during a period of two years and three months, viz., from March, 1843, to July, 1845, the average clearness of the sky was somewhat greater after midnight than before it. The following numbers show the averages of the

*Mean Sky Covered by Clouds.*

	From 6 P. M. to midnight.	From midnight to 6 A. M.
1843 (from March), . . . . .	.63	.59
1844, . . . . .	.72	.62
1845 (to July), . . . . .	.64	.61

The numbers for the different quarters of years are as follows:

*Mean Sky Covered by Clouds.*

		From 6 P. M. to midnight.	From midnight to 6 A. M.
1843.	{ April, May, June, . . . . .	.53	.51
	{ July, Aug., Sept., . . . . .	.66	.66
	{ Oct., Nov., Dec., . . . . .	.72	.59
1844.	{ Jan., Feb., March, . . . . .	.71	.62
	{ April, May, June, . . . . .	.90	.83
	{ July, Aug., Sept., . . . . .	.65	.56
1845.	{ Oct., Nov., Dec., . . . . .	.61	.56
	{ Jan., Feb., March, . . . . .	.61	.60
	{ April, May, June, . . . . .	.67	.63

For the entire night, from 6 P. M. to 6 A. M., we have—

	1843.	1844.	1845.	Average.
Jan., Feb., March,	. . .	.66	.60	.63
April, May, June,	.52	.86	.65	.67
July, Aug., Sept.,	.66	.60	. . .	.63
Oct., Nov., Dec.,	.65	.58	. . .	.61

These numbers show that during these years the clearness of the sky was no greater from March to October than during the first and last quarters of the year, and therefore that the more rapid fall of the temperature at night toward the middle of the year, than toward the beginning and end of it, during the inter-



val of time embraced in the above table, cannot be ascribed to a greater average amount of nocturnal radiation resulting from a greater clearness of sky.

For the years 1841-2, the observations only furnish the averages for the entire day, of twenty-four hours. These, with the averages for 1843 and 1844, and from January to June, 1845, are as follows:

	1841.	1842.	1843.	1844.	1845.
Jan., Feb., March, .	·63	·55	·.	·80	·65
April, May, June, .	·56	·49	·59	·92	·72
July, Aug., Sept., .	·48	·52	·75	·70	·.
Oct., Nov., Dec., .	·59	·44	·79	·67	·.

These numbers serve only to confirm and extend the conclusion drawn from the previous table.

The following table, giving for each quarter of the year the mean number of days, during the years 1839, 1840-1, and parts of 1838 and 1842, on which the wind prevailed, at 9 P. M. and 3 A. M., from each direction, at Washington, will furnish the means of judging of the influence of particular directions of the wind.

		N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
January, &c. . . .	9 P. M.	6·1	10·0	7·5	5·0	12·3	11·1	8·5	15·5	15·5
	3 A. M.	6·1	10·7	6·0	3·0	11·3	11·5	6·0	16·8	17·8
April, &c. . . . .	9 P. M.	4·6	9·7	3·3	8·0	11·0	8·7	9·3	11·6	22·8
	3 A. M.	4·8	13·7	4·6	8·3	6·0	12·0	9·6	11·0	19·2
July, &c. . . . .	9 P. M.	5·6	5·0	4·0	7·3	9·7	9·0	10·5	11·1	21·6
	3 A. M.	6·2	10·9	5·7	3·6	5·4	14·3	9·3	11·0	20·0
October, &c. . . .	9 P. M.	6·8	12·3	7·8	2·7	6·8	11·0	8·8	19·0	16·3
	3 A. M.	9·5	12·0	6·9	4·2	3·8	9·7	7·7	22·4	17·4
Entire year, . . .	9 P. M.	23·1	37·0	22·6	23·0	39·8	39·8	37·1	57·2	76·2
	3 A. M.	26·6	47·3	23·2	19·1	26·5	47·5	32·6	61·2	74·4

From this table we derive the following, showing the relative frequency of the cold and warm winds.

		N., N.E., W., N.W.	E., S.E., S., S.W.
January, &c.	9 P. M.	40·1	35·9
	3 A. M.	39·6	31·8
October, &c.	9 P. M.	46·9	28·3
	3 A. M.	51·6	24·6
		N., N.W., N.E., E.	S., S.W., S.E., E.
April, &c.	9 P. M.	29·2	37·0
	3 A. M.	34·1	35·9
July, &c.	9 P. M.	25·7	36·5
	3 A. M.	33·8	32·6
		N., N.W., N.E.	S., S.W., S.E.
Entire year,	9 P. M.	117·3	102·6
	3 A. M.	135·1	93·1

On examining these tables, it will be seen that calms are about equally frequent before and after midnight, and that cold winds are rather more frequent and warm ones less so, throughout the



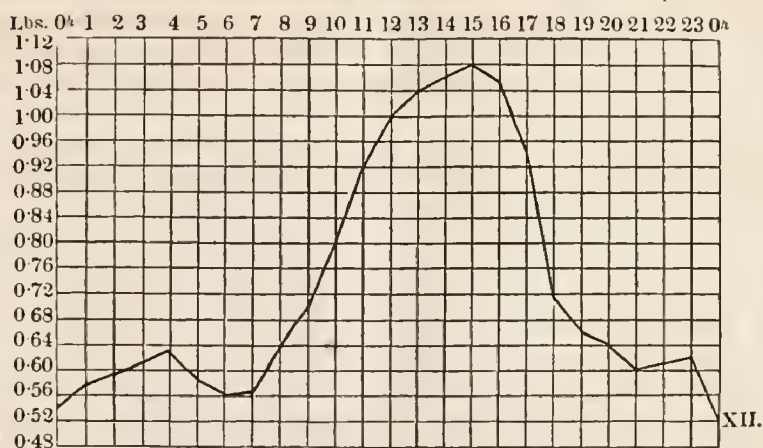
year, after than before midnight. It appears also that cold winds are more prevalent, and warm winds less so, in the first and last than in the middle quarters of the year. Both of these facts are opposed to the idea that difference of direction of wind may be the general cause of the diminution in the hourly decrement of temperature as the night advances, and in the nocturnal decrease of temperature from the warm to the cold months.

If we reject the *N.E.* and *W.* winds from the list of cold winds, that is, regard them as not affecting the mean temperature, in the colder months, cold winds will still be as prevalent after midnight as before: and if we reject the *W.* wind, in the warm months, from the list of warm winds, it will still be true that warm winds will prevail no more in these months after midnight than before. But upon these suppositions the cold winds will become more frequent in summer than in winter in the proportion of 31 to 22, and the warm winds less frequent in the proportion of 27 to 33: and both cold and warm will occur with about the same degree of frequency in summer as in autumn. It is to be observed, however, that the relative cooling effect of northerly winds in different seasons is not in exact proportion to their relative frequency, for the relative force of the wind is to be taken into account. Now it appears on examining the curves showing the force of the wind in the different quarters of the years 1843-4-5 at Philadelphia, that the force of the wind at night is from two to three times greater from September to April than from March to October. If we connect with this the facts, that the strong winds in winter are most frequently from the *N.W.*, and that, as shown by the first of the above tables, *N.W.* winds are much more frequent during the former than the latter of the above mentioned period, it will be seen that there is little room to doubt that the greater nocturnal decrease of temperature in the summer than in the winter, must be due to some other cause than the differences, generally subsisting, between the directions of the wind in these seasons. If there be any lingering doubt upon this point, it will be removed, if we reflect that the law of variation of the nocturnal decrease of temperature from one season to another, which we have been considering, is as true for one place as for another. It is found to hold good, with occasional partial exceptions, at all places, both in this country and Europe, where meteorological observations have been made. In fact, this law is essentially connected with the general fact that the loss of temperature, by nocturnal radiation, is equal to the daily rise of temperature, both in January and July. So universal a law cannot, it is believed, be dependent upon the direction of the wind, since the prevalent winds are often very different in different localities.

The question of the influence of the force of the wind follows next in order. Fig. 12 exhibits the mean variations of the force of the wind during the year 1844.

Fig. 12.

Curve of the Mean Diurnal Variations of Force of Wind for 1844, in Lbs.



On inspecting this curve it will be seen that the force of the wind is nearly the same before and after midnight, and that its principal variations occur during the day. The same law is shown by the curves for other years, and for quarters of years, so far as given. The nocturnal loss of temperature cannot therefore be materially modified by variations in the force of the wind, in the average of months and years. The curve showing the variations in the average force of the wind from month to month, which cannot conveniently be given here, indicates that the wind is highest in February or March, and is much higher during the first and last quarters of the year than toward the middle of the year. Since these strong winds are more apt to be from the N.W. than from any other quarter, their tendency will be to cause the temperature to fall more during the night, in the fall and winter, than in the spring and summer, instead of less as it does in fact.

Let us next consider whether it may chance that the variations in the nocturnal loss of temperature which I have specified, are due to the cooling or heating effect of rain. The observations in my possession do not furnish me with the means of making any but a very partial examination of the relative quantities of rain that fall during the first and last halves of the night. But the following facts will serve to show that the influence of rain, whatever it may be, can have no part in determining the law of the decrease of temperature for a single night. 1. Of twenty-one term days, observed at Washington, for which the curves of the daily variation of temperature are given, there are only five exceptions to the general fact that the temperature falls most slowly during the latter half of the night. 2. Of twenty-three days observed in Philadelphia, in October, 1843, there are but six exceptions to the same general fact. We may conclude from these facts, that

the slower rate of cooling after midnight is too common a phenomenon to depend upon rain, or prevailing direction or force of wind. Moreover, of the seventeen days observed at Washington, on which the law held good, as a matter of fact only two were rainy. The exceptional days were, with one exception, either rainy or foggy.

The following table shows the total quantity of rain that fell at Philadelphia during each quarter of the years 1842-3-4, and the first two quarters of 1845.

	1842.—Inch.	1843.—Inch.	1844.—Inch.	1845.—In.
Jan., Feb., March, . .	6·29	4·710	7·305	6·503
April, May, June, . .	10·59	10·330	5·244	6·604
July, Aug., Sept., . .	11·39	15·704	10·787	. .
Oct., Nov., Dec., . .	7·85	8·672	8·017	. .

Now, if we compare the numbers for 1844 with the mean nocturnal losses of temperature, in the different quarters of this year, which are  $11^{\circ}$ ,  $16^{\circ}$ ,  $16^{\circ}$ ,  $10^{\circ}$ , we see that while the fall of temperature is the same for the two middle quarters of the year, the quantity of rain fallen is about twice as great for one of them as for the other. The cloudiness of sky for these periods is  $\cdot 92$  for the first, and  $\cdot 70$  for the second. Again for the middle quarters of the year 1843 the quantities of rain are  $10^{\text{in}}\cdot 3$  and  $15^{\text{in}}\cdot 7$ , while the decrements of temperature are  $15^{\circ}$  and  $12^{\circ}$ —the reverse of what should be the fact, since if rain is the determining cause of the greater loss of temperature in a summer night, it can only have this effect by cooling the earth in summer and warming it in winter. These statements are sufficient to make it evident that the cause we are seeking does not consist in variations in the quantity of rain that falls at different seasons. The truth of this conclusion may be confirmed by the consideration already urged, in considering the influence of the wind, that the laws of the variation of the nocturnal loss of temperature are too general to depend upon a cause which must differ so much in its effects in different places.

It will have been observed that in considering the question of the influence of variations in the cloudiness of the sky, it was tacitly implied that the numbers representing the proportion of sky covered by clouds represented also the proportion of that part of the sky which is concerned in the nocturnal radiation, that was covered by clouds. This it will be recollected, is the part which lies within  $30^{\circ}$  or  $35^{\circ}$  of the zenith. Now it is not difficult to see that this portion of the sky will be, in the average of months, less cloudy than other parts, and especially than the parts near the horizon. For, the clouds in the horizon are much more distant than those of the zenith, and consequently are seen very obliquely, and as they are generally of considerable thick-



ness, and there are often several layers at different elevations, they will be frequently projected upon each other. Or the case may be stated thus; if we suppose isolated clouds to be scattered uniformly throughout the cloudy stratum, the line of sight which makes a small angle with the plane of the horizon will cross this stratum very obliquely, and be proportionally more likely to meet with a cloud than a line traversing this stratum perpendicularly at the zenith. Besides from the obliquity of this line a haziness which would scarcely be observed in the zenith might amount to a positive cloud at a distance from the zenith. This is well illustrated when a fog or mist is gradually being dissipated, and the sky is first seen in the zenith.

In this way it may very well happen that, although there may be considerable differences among the numbers representing the cloudiness of the sky in general, for the quarters of years, there may be no material variations in the nocturnal decrements of temperature, corresponding to these differences.

Besides the phenomena which have now been considered, there are two others, to which the laws of the nocturnal loss of temperature may be conjecturally attributed, viz. 1. The varying humidity of the soil at the earth's surface, attended with a change in its specific heat, and—2. An unequal exchange of heat between the atmosphere and the earth, by contact. That the first of these is to be rejected from the list of possible causes, will be seen at once upon considering that there is no evidence that the law of the continued decrease of the loss of temperature as the night advances, depends upon variations in the quantity of rain. There is no sufficiently general increase of humidity, except from dew, and the film of dew is too slight (as we shall see, not more than  $0^{\text{in}}\cdot 01$ , on the average, in July) to produce any material change in the specific heat of an inch, or half an inch, of thickness of the soil. Besides whatever influence may arise from this cause may be set down as an effect of dew.

As for the second of the above mentioned conjectures, the following objections lie against it. In the first place the quantity of heat received from the air by simple contact, when the air is perfectly tranquil, is very small, by reason of its low conducting power. In the second place, the observations show that, in the calmest nights the diminished fall of temperature after midnight is no less certain, than when the equilibrium of the air is disturbed and different masses are brought successively into contact with the surface of the earth. Again, since this law is observed to hold with respect to the temperature of the atmosphere at the earth's surface, the air, when agitated in contact with the earth, must lose from this cause less and less heat as the night progresses. But this is impossible, since the cooling of the earth's surface,



from radiation, goes on uniformly, and thus the difference between its temperature and the temperature of the air would increase, and therefore the cooling, for the same amount of agitation, should increase. In other words, the cooling of the air, attendant upon the same degree and extent of agitation, must be uniform. Unless, therefore, there is generally more wind, or the disturbance of the equilibrium of the air extends to a greater height during the latter than during the fore part of the night, the law above mentioned must have some other cause than that under consideration. Now, as a matter of fact, the observations do not disclose the existence of any general difference, of any amount, between the force of the wind before and after midnight. It appears from the curves showing the variations in the force of the wind at Philadelphia, that there are slight differences, but they sometimes lie in one direction and sometimes in the other.

In view of all that has now been stated, it may be confidently affirmed, that if the cause of the two anomalous facts connected with the nocturnal loss of temperature be any meteorological phenomenon, it must be the deposition of vapor from the atmosphere in other forms than that of rain, and chiefly therefore in the form of dew; and it may be stated farther, that either this must be the actual cause, or it must consist in variations in the amount of heat that is returned towards the surface during the night, by conduction from below, or, in other words, in the laws of the earth's cooling at night, irrespective of all atmospheric influences.

This result has been reached, it is true, by taking it for granted that, primarily, it is the temperature of the surface of the earth that varies according to the laws which I have stated, and that the temperature of the atmosphere, near the earth's surface, conforms, of necessity, in its variations, to the same laws, by reason of its relations to the earth's surface. This supposition is in accordance with the notion generally entertained upon this subject, and is not likely to be questioned, but the principles laid down in the general discussion preliminary to the present investigation serve conclusively to establish its truth. For it will be seen that the causes which make the nocturnal radiation from the earth's surface into space and the upper regions of the atmosphere uniform, will also tend to make the radiation of the air, in the same direction, uniform, and on a calm night the radiation from the air to the earth ought gradually to increase, inasmuch as the difference of temperature of the two ought slowly to increase, (radiation alone being considered,) and for small differences of temperature the radiation is proportional to the difference. From which it appears that the laws in question are not primarily true of the temperature of the atmosphere.

It was also taken for granted that there was no material variation in the radiating power of the earth's surface from one season to another, by reason of frost or more or less humidity, or changes in the state of vegetation or any other cause. That, in point of fact, the diminished loss of temperature at night in the winter cannot be owing to any such cause as this, will be seen on inspecting the following tabular statements.

*Average fall of thermometer from 9 P. M. to 4 A. M. at Philadelphia.*

	1842.	1843	1844.	Average.
July, .....	4°·40	5°·35	5°·18	4°·97
August, .....	....	....	5°·02	5°·02
September, .....	....	....	4°·84	4°·84
October, .....	6°·00	4°·04	3°·77	4°·60
November, .....	2°·57	1°·55	3°·40	2°·50
December, .....	2°·61	2°·43	2°·01	2°·35

*Mean Diurnal Variations of Temperature at Halle, Göttingen, Padua, and Forth Leith (near Edinburgh.)*

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Halle, .....	4°·25	7°·59	9°·11	14°·22	16°·79	16°·67	16°·57	16°·05	14°·45	12°·18	6°·21	3°·76
Göttingen, ...	7°·16	8°·06	12°·00	14°·94	17°·78	17°·46	18°·02	18°·16	17°·24	11°·55	6°·03	4°·68
Padua, .....	6°·21	7°·27	9°·00	9°·90	13°·68	12°·01	16°·00	16°·31	12°·45	8°·15	9°·31	7°·40
Forth Leith, ..	2°·66	3°·56	6°·15	10°·58	8°·58	8°·33	9°·68	7°·58	8°·04	4°·88	4°·08	2°·30

It will be observed that the nocturnal loss of temperature steadily decreases from August to December, and that there is no cause tending to diminish the radiating power of the earth's surface that operates in the same steady manner at all places, during this interval of time.

I conclude, therefore, that the cause of the nocturnal secondary variations of the horizontal force must either consist in variations in the amount of vapor deposited from the atmosphere, or be in some way connected with the upward flow of heat below the earth's surface. This upward flow of heat is not attended with any variations in the total amount of heat lost, and we therefore can connect the secondary variations with it, only by introducing some new hypothesis. Besides there exist good reasons for believing that it cannot be the cause of the observed variations in the nocturnal loss of temperature, and therefore cannot be the cause of the secondary variations of the horizontal intensity. In the first place, it appears from equation 4, p. 48, that the loss of temperature at the surface ( $t$ ), is equal to the entire loss of heat ( $L$ ) minus the sum of all the losses of temperature of the layers below the surface. It follows therefore, as  $L$  is always the same for the

same number of hours, that  $l$  cannot become less unless the sum of  $l', l'', l'''$ , &c. becomes greater; now it is a matter of observation that when  $l$  is less,  $l', l'', l'''$ , &c., are also less; the sum of  $l', l'',$  &c., cannot therefore be greater in winter than in summer, unless the number of layers which experience a change of temperature during the night increases to a sufficient extent to compensate for the diminished loss of temperature of those nearer the surface. This cannot be the case, for, in the first place the loss of temperature of the lower layers is always comparatively trifling, and in the next place the depth to which any perceptible cooling in consequence of surface radiation extends during the same period of ten or twelve hours, must depend almost entirely upon the length of the interval. Or, we may arrive at the same conclusion in another way. In the month of January the amount of heat gained during the day is lost during the night. The same is true in July. Now the quantities of heat gained during the day are quite different at these two epochs, the one of minimum and the other of maximum temperature, and therefore  $L$  must be variable, whereas it is constant. In the next place, although we are in want of the systematic observations of the diurnal variations of  $l, l', l'',$  &c., which would enable us to ascertain with certainty whether the sum of  $l', l'',$  &c. varied materially during the night or not, and therefore whether the variations of  $l$  are attributable to an unequal upward flow of heat; still, if we assume that the established law connecting the total variations of temperature of the different layers, viz., that they decrease according to a geometrical progression for equal increments of depth, is very nearly true for the variations that occur in any given time, and take  $\frac{3}{4}$  for the ratio of the progression, (derived from the data on page 45, answering to the depths of  $1^m, 2^m, 3^m,$  &c., taking variation at surface =  $15^\circ$ ,) recollecting that the cooling progresses at the rate of about  $1\frac{1}{3}$  inches per hour, we shall find that the sum of the losses of temperature of the layers below the surface up to the hour of 4 A. M. is materially less than twice the sum of the losses up to the hour of 10 P. M.—midway between the hours of maximum and minimum temperature. It can only be made equal to twice this sum by supposing that the fall of surface temperature is increased very nearly in a two-fold proportion—in other words, that it is proportional to the time, or uniform. This result does not depend upon the ratio  $\frac{3}{4}$  supposed to be used in the calculation, but upon the fact that the losses of temperature of the new layers below the depth of  $8^m$ , or  $9^m$ , after 10 P. M., are very small. If it be erroneous, it can only be because the law which I have assumed does not accord with fact. In view of all these considerations we may conclude, that the notion of a variable flow of heat toward the surface of the earth fails entirely



to explain the unequal losses of temperature at night in different seasons of the year, and that it is highly improbable that it has any considerable effect to diminish the losses of temperature from hour to hour during the night. Indeed all known facts connected with the variation of temperature, favor the idea that the laws of the nocturnal variation are the result of some cause at the earth's surface tending to diminish the amount of heat lost there, and producing the same effect as if the radiation varied between certain limits and according to certain laws. This cause can be nothing else than the deposition of condensed vapor from the air in contact with the earth.

Let us now take up the independent inquiry as to what is the testimony of observation and experiment in relation to the variations in the amount of dew deposited in different seasons and in different hours of the night, the actual amount deposited at any one season, and the quantity of heat given out in the condensation of the vapor into dew. I find the following statement with respect to the first of these points, in Lamé's *Cours de Physique*. "Dew is less abundant before midnight than during the hours which precede the rising of the sun. It is more frequent in spring, and especially in autumn than in summer." I conceive the meaning of the first of these statements to be, that on any individual night when the dew begins to fall early in the evening, it becomes more abundant towards morning. If this be true, it may arise from the fact that there will be sufficient displacement of the air, even on what would be called a calm night, to bring about more or less of an exchange of place between the air resting upon the surface and that which is posited above this; and as the cooling goes on, these various masses brought in succession to the surface will become more and more humid, and therefore more and more likely to deposit a portion of their vapor before they give place in their turn to other bodies of air. Such currents will be established by inequality of surface and of radiation, the irregular action of elevated objects, and perhaps other causes. When the air is perfectly tranquil, the deposition of vapor might increase towards morning by the temperature of the air above the surface becoming reduced below the dew point, in which case a portion of its vapor would be condensed and fall in a fine mist. This undoubtedly sometimes happens, and must be more frequent towards morning than before midnight. But there is another way in which such a phenomenon may be conceived to arise. The air in contact with the earth, and for a certain small distance above it, when dew is being deposited is saturated with vapor. Its temperature is probably somewhat higher than that of the ground, for the variations of temperature of the ground will not be communicated instantaneously to the air just above it. The amount of dew deposited in any small interval of time will then depend



not only upon the reduction of temperature, which takes place in this interval, but also upon the difference of temperature between the ground and the contiguous air, that obtains during this interval. Now, this difference may increase, as the night advances, at the same time that the loss of temperature from radiation continues the same; and the increase may be more than sufficient to compensate for the diminished temperature of the vapor, and thus the dew may be more abundant. It will be seen farther on, that even when a small quantity of dew falls during the night, a portion of vapor is abstracted from the air to the height of several hundred feet. We may learn from this fact that we cannot derive the variations in the quantity of dew from the observed variations in the falling of the dew point, noted only at one particular height. The observations should be made at various heights to furnish the data required for such a calculation.

Whether the fact really be in accordance with our understanding of the statement of Lamé or not, there can be little question that, in the average of weeks and months, there will be more dew deposited after midnight than before; for, besides that we have the authority of Lamé in support of the assertion, it will be observed that it is only when the elevation of the temperature of the air above the dew point at 4 P. M., is equal to, or less than, the amount that the temperature falls in the interval of time between this hour and sundown, that the dew will begin to fall at sundown, and it appears from observation, that at Philadelphia, the difference between the dew point and the temperature of the air, at 4 P. M., is, on the average, about equal to the entire average nocturnal loss of temperature, and is accordingly about  $8^{\circ}$ , more or less, greater than the fall of temperature down to the time of sundown. Dew must therefore be much more frequent after than before midnight. It is to be observed here, that it is not necessary that the humidity of the air should be above the average, in order that any dew may be deposited during the night, as may perhaps be inferred from the above statement; for, if it were not for the heat given out by the falling dew, the temperature would fall much lower; in other words, the actual loss of temperature is that due to radiation diminished by the heat given out by the condensed vapor.

As to the relative frequency of dews at different seasons, it must become greater, from the very nature of things, for the same loss of heat by radiation, in proportion as the humidity of the air becomes greater; and therefore must increase from July or August to December or January. It is true, that this increased frequency in the occurrence of dew, is partially compensated for by the diminution in the amount of dew deposited from saturated air in a given time, from the same reduction of temperature, as the mean daily temperature becomes lower; but the effect of

this cause is much less than appears at first sight, for, if less dew falls in any short space of time, at the lower temperature, the heat given out will be less, and therefore the cooling will be greater.

We have next to enquire into the absolute amount of dew deposited at any one season. The experiments of Professor Brocklesby, detailed in an article "upon the Influence of Color on Dew," published in the No. of this Journal for September, 1848, furnish data upon which we may base a calculation. The experiments I shall use are a suite of eleven, made at intervals from July 9th to August 4th. Strips of flannel, of the size of from 15 to 17.5 square inches, and of various colors, were placed upon closely shorn turf, or upon a smooth board elevated six or seven inches above the turf, and the amount of dew gained by them ascertained by weighing them. The average amount deposited upon the pieces of green flannel, in the eleven experiments, I find to have been 28.1 grains for a surface of twelve square inches. This is equivalent to a film of water of the thickness of  $\frac{1}{1000}$  of an inch. The average is very nearly the same for the other colors. It is to be observed with respect to these experiments that they were made at the season of the year when there is the least dew, and that the nights do not appear to have been selected with reference to the degree of humidity of the air, for the amount deposited on the different nights varies from about 20 grs. to 60 grs., and the first ten days are included within a space of twenty days. They were probably a fair average of the nights that occur at that season of the year. The radiating power of mold is 92, and of vegetation over 100, that of lampblack being 100. There is no reason to suppose that the radiating power of flannel is materially greater. I shall therefore consider myself entitled to assume that on clear nights in July the average amount of dew is not less than  $\frac{1}{1000}$  of an inch.

Connected with the question of the amount of dew is that of the height in the atmosphere to which the abstraction of vapor extends. We may obtain an estimate of the distance through which the vapor falls, by observing how much the dew point falls near the surface of the earth. At Philadelphia, where the hygrometric observations were made at the height of about four feet above the ground, this does not exceed  $2^{\circ}$ , on the average, in the months of July, August, and September. Obtaining the requisite data, and making the calculation, I find, that to furnish 0.01 of dew at the temperature of  $70^{\circ}$ , this reduction of the dew point must extend no less than 736 feet; and that this amount of dew is equivalent to all the vapor in the air, when the air is saturated, within forty-three feet of the earth's surface. In the average hygrometric state of the air (for both day and night) it is only half saturated with vapor, and all the vapor within eighty-six feet of the



surface of the earth would not condense into more than 0<sup>in</sup>·01 of dew, at the temperature of 70°. If we suppose the rise and fall of the dew point, occasioned by winds, to balance each other, then the average fall will represent the excess of the fall attendant upon dew and rain over the rise produced by nocturnal evaporation. The average fall attendant upon dew alone may then exceed 2°; and the withdrawal of vapor extend to a proportionally less height. We may confidently affirm, however, that on a clear night vapor must fall to the earth's surface from a height of several hundred feet. But we are not confined to mere inferential conclusions on this point, for, direct observations upon the varying hygrometric state of the air have been made upon the summits of the Righi and Faulhorn in Switzerland, which have established that even at such heights (4,530 feet and 7,240 feet), the quantity of vapor decreases during the night no less regularly than in the valleys below. But the fall of vapor doubtless occurs at a height much greater than that of the point from which it descends during the night as far as the earth's surface.

What is the amount of heat given out in the deposition of this quantity of dew? The experiments of Watt, and of Clement and Desormes have established that when a given weight of vapor is condensed into water of a given temperature, say 32° F., the quantity of latent heat disengaged is the same, whatever may be the temperature of the vapor, and is sufficient to heat the same weight of water, at the temperature of 32°, no less than 1157°. This then is the amount of latent heat evolved in the deposition of dew on a winter night. On a July night it will be 1119°. We may accordingly assume that all seasons of the year it is more than 1100°; and may therefore conclude that in the deposition of 0<sup>in</sup>·01 of dew, the heat evolved is sufficient to raise this 0<sup>in</sup>·01 of water 1100°, and to raise the temperature of one inch 11°. I do not find the specific heat of mold or soil in any table of specific heats in my possession, but in the very extended table given in Pouillet's *Elements de Physique Experimentale*, I find the specific heat of water set down as much greater than that of every solid in the list. It is therefore highly probable that the specific heat of the soil is much less than that of water. We may strengthen this conclusion, and at the same time obtain some approximation to the element sought, by attending to the specific heats of the principal ingredients of the soil. These are, of silica 0·19, of alumina 0·20, and of carbonate of lime 0·21, that of water being 1. The specific heat of dry soil is therefore probably about 0·20. That of soil in its ordinary moist state must be somewhat greater. A somewhat rough experiment gave me 0·41 for the specific heat of a mass of earth of medium moisture, composed of sand with some clay, the density of which I found to be 1·5.

To be able to determine how much the temperature of a given depth of soil will be raised by a given amount of heat, we must know the specific gravity as well as the specific heat of the soil. Now the specific gravity of sand is stated to be 1·5, that of clay to be 2·2, and that of common earth 2·0. It appears, therefore, that the increased rise of temperature of one inch of soil, above that of water, by reason of the less specific heat of the soil, should be generally pretty nearly counterbalanced by the diminution consequent upon its greater specific gravity. But, as the specific heat is doubtless less than 0·5, while the specific gravity does not exceed 2, the augmentation will be somewhat greater than the diminution, and therefore the heat given out in the deposition of 0<sup>in</sup>·01 of dew will raise the temperature of one inch of soil more than 11°. If we take the specific heat equal to 0·4, according to the before mentioned experimental determination, and the specific gravity equal to 2, the effect of this amount of heat we find to be 13°·7. Upon a perfectly dry and sandy soil it would not, probably, be less than 36° (since the density would be 1·5, and the specific heat about 0·2). On the other hand, upon a clay soil saturated with moisture, it might be as low as 10°. But I will suppose for the present, that the heating effect of 0<sup>in</sup>·01 of dew is no more than 11° to one inch in depth of soil, and proceed to enquire what quantities of dew would suffice upon this supposition, to reduce the loss of temperature due to nocturnal radiation down to the actual losses observed in different seasons. On referring to the table of annual variations of temperature at various depths below the earth's surface, given on page 45, we find that the variations at the depths, 1<sup>m</sup>, 2<sup>m</sup>, 3<sup>m</sup>, &c., form pretty nearly a geometrical progression, of which the ratio is about  $\frac{2}{3}$ ,\* (the variations continuing below 8<sup>m</sup>, and becoming nearly imperceptible at the depth of 18<sup>m</sup>). Assuming this law and ratio for the nocturnal variations, at the depths of 1<sup>in</sup>, 2<sup>in</sup>, 3<sup>in</sup>, &c., recollecting that in a night of twelve hours the cooling extends to the depth of about 18<sup>in</sup>, and taking 12° as the fall of temperature at the surface, which is about the average for the year, and forming the progression, I find the sum of the different terms to be 36°. This then is the actual loss of heat. Now the average fall of temperature at sundown is 2° per hour. If we suppose this to be due entirely to radiation, then, but for the heat given out by the dew, the entire decrease of surface temperature in the course of the night would be 24°; and therefore the entire loss of heat would be  $2 \times 36^\circ$  or 72°. To reduce this to 36° the heat given out by the dew must be 36°; and therefore the amount of dew must be about  $\frac{2}{10}$  of an inch. We have then this result; upon

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\* The true ratio is between  $\frac{2}{3}$  and  $\frac{3}{4}$  and nearest to  $\frac{2}{3}$ . I take  $\frac{2}{3}$  in preference to  $\frac{3}{4}$ , as, for reasons that will appear soon, it will here furnish results nearer to the truth.



the suppositions made the average quantity of dew, for the year, that must be deposited in the interval between 5 P. M. and 5 A. M. to satisfy the requirements of the theory, is about  $\frac{3}{10}$  of an inch, and therefore about three times greater than the actual average for July, as ascertained from observation;—or rather, strictly speaking, about three times greater than the average of the results of Professor Brocklesby's experiments, which, it is probable, is somewhat higher than the actual average for this month. On glancing at the curves of diurnal variation of temperature for 1844, (see p. 42,) it will be seen that the average fall of temperature about the time of sundown, varies, from one quarter of the year to another, from  $1\frac{1}{2}^{\circ}$  to  $3^{\circ}$ . This difference is probably the result of the joint action of three different causes, viz., the unequal evaporation that obtains, at this hour, on some days, the unequal deposition of dew, at the same hour, on other days, and ascensional currents varying in velocity. The first and last, so far as they act, will tend to make the fall of temperature at sundown greater in summer than it would be from radiation alone; and the second tends to make it less than this, perhaps, during the greater part of the colder half of the year. It is probable that the first tendency prevails over the second, and therefore that the average loss of heat, from radiation alone, is not greater than  $2^{\circ}$  per hour, the average loss of heat at sundown for the entire year. It is certain that it must be less than  $3^{\circ}$ . On the supposition that it is  $3^{\circ}$ , the entire loss, from radiation, would be  $108^{\circ}$ , instead of  $72^{\circ}$ ; and the amount of dew required  $0^{\text{in}}.06$ . On the other hand, there can be no question that the estimate I have made of the actual amount of heat lost is too high, for it is the sum of the entire variations of temperature of the different layers, to the depth of  $18^{\text{in}}$ , in the course of a day, whereas the actual loss of heat during the night is equal to the sum of the actual losses of the same layers in the course of the night, and the loss of each layer in this interval of time will be less than its entire loss, with the single exception of the layer at the surface. From these considerations it appears that the average quantity of dew necessary to reduce the average loss of temperature due to nocturnal radiation down to the actual average loss, for the year, is certainly materially less than  $0^{\text{in}}.06$ , and is probably less than  $0^{\text{in}}.03$ . As to the quantities required for the other seasons, taking the radiation at  $72^{\circ}$ ,  $15^{\circ}$  for the average nocturnal loss of temperature in the interval between the vernal and autumnal equinox, and  $9^{\circ}$  for the same in the interval between the autumnal and vernal equinox, I find that the heating effects of the dew during the night, in these two periods must be, respectively,  $27^{\circ}$  and  $45^{\circ}$ , and the quantities of dew  $0^{\text{in}}.04$ , and  $0^{\text{in}}.024$ . The average for July, to correspond to an average of  $0^{\text{in}}.024$  for the six warm months, must be less than  $0^{\text{in}}.02$ , and probably, judging from the

considerable variations in the amount of dew from March to July, and especially from July to October, noticed by the casual observer, should be less than  $0^{\text{in}}\cdot 01$ , and therefore approximate to the actual average for July, which as we have seen is probably something less than  $0^{\text{in}}\cdot 01$ . The agreement between theory and observation is therefore as close as it could reasonably be expected to be, in a case in which there is an uncertainty, within certain limits, in reference to some of the numerical results arrived at inferentially, and in which the observations have not all that completeness and definiteness which is essential to a thorough testing of the theory. This is seldom realized except where the observations are made for the express object of subjecting a theory after it has gained a foothold in the region of science, to the most rigid scrutiny.

I conclude, therefore, that the heat evolved from the dew, or condensed vapor, that falls at night, is nearly, if not quite sufficient to reduce the theoretical decrease of temperature due to radiation, down to the amount which actually obtains; and that the variations in the quantity of dew that falls at night, from one season to another, are attended with sufficient variations in the amount of heat imparted to the earth, to effect the changes observed in the nocturnal decrease of temperature during the year.

As to the general explanation of the effect of dew, I conceive that, in the average of months, the amount of dew deposited from hour to hour during any one night, and from night to night, is in proportion to the humidity of the air, and therefore must increase steadily from sunset to sunrise, and from summer to winter. I consider that it has been conclusively established, that it is only from the varying amounts of heat imparted to the earth, consequent upon these varying quantities of dew, that the observed changes in the nocturnal losses of temperature from hour to hour, and from night to night, can possibly result.

We have seen in a former part of the present paper, that the secondary nocturnal variations of the horizontal magnetic intensity of a place, correspond, in respect to time and direction, with the deviations in the nocturnal loss of temperature from uniformity, and that the cause of these deviations is therefore, in all probability, either identical with or closely related to the cause of the variations in question. It follows therefore from the conclusions which have just been obtained, that the probable cause of the secondary variations of the horizontal force is to be found in the varying quantities of dew deposited at night; from one hour to another, and from one season to another. It is to be observed, however, that it is not essential that the unequal losses of temperature at night should be attributable entirely to the thermal influ-

ence of dew, to enable us to draw this inference. We have seen that the results of observation and experiment conduct us to the conclusion that the tendency of this influence is to produce the inequalities which have been under consideration. This furnishes sufficient ground for the inference; but if the entire amount of the inequalities of the decrease of temperature be not due to this influence, there is room to doubt whether the secondary variations of the horizontal force might not be attributable to the action of the other unknown cause or causes tending to produce unequal variations of temperature at night. In this event also, in estimating the effect of the dew upon the horizontal force, we could not be positively certain that the entire variations of temperature at night were attended by corresponding variations in the amount of heat in the stratum of variable temperature below the earth's surface: unless, at least, it should be admitted that the unknown cause must consist in some relation or phenomenon external to this stratum.

Having thus arrived, inductively, at the probable cause of the secondary nocturnal variations of the horizontal force, let us see how it applies itself to the detailed explanation of these variations. It is abundantly evident, from what has gone before, that the continual accumulation of dew, or condensed vapor, at the earth's surface from evening till morning, must tend to diminish the rate of decrease of the horizontal force; and that the increase in the quantity of dew that falls, from hour to hour, must tend to make this diminution greater and greater, and that it is not improbable that towards morning the effect of the increased quantities of dew may prevail over the effect of the loss of heat, and thus that the horizontal force may begin to increase. (See the July No. of this Journal, p. 40, figs. 1 to 5.) The actual variations of the horizontal force, it will be observed, are the result of two antagonistic causes: the tendency of the uniform loss of heat, from radiation, is to make the horizontal force decrease uniformly during the night; the tendency of the dew is to augment this force, but it acts unequally, producing the greatest effect towards morning, when it ordinarily prevails over the other cause. The dew tends to augment the horizontal force in two ways; by furnishing a certain amount of heat to the earth, and thus diminishing the loss of heat, and by adding to the amount of magnetic matter at the earth's surface. The joint effect of these two opposing causes should be different in different seasons. From the vernal to the autumnal equinox, the dew is less in amount and the nocturnal fall of temperature is greater, and therefore the height of the morning maximum of horizontal force ought to be less, than from the autumnal to the vernal equinox—a conclusion which accords with fact. (See as above.)



To obtain a theoretical estimate of the relative height of this maximum in these two periods, we have only to seek for the amount of the diminution of the horizontal force during the night, that would result from radiation alone, and then allow for the proportionate effect of the dew, both thermal and directly magnetic. Now it appears, on an examination of the curves of the daily variation of horizontal force, that the diminution of the horizontal force at sundown, which must be chiefly due to radiation, is about one-half of one of the division-spaces in the diagram: according to this, the tendency of the radiation is to diminish the horizontal force, during a night of twelve hours, (from 5 P. M. to 5 A. M.,) the amount of six of these spaces. We have obtained for the proportionate amounts of dew on a single night during the two periods above mentioned  $0^{\text{in}}\cdot024$ , and  $0^{\text{in}}\cdot04$ . These numbers bear to each other the ratio of 6 to 10, or 3 to 5. Now, the curves of the daily variations of the horizontal force for the middle quarters of the year show that during these periods the morning maximum is about one division and a half below the evening maximum. According to this the tendency of the entire effect of the dew must then be to increase the horizontal force  $4\frac{1}{2}$  divisions. Increasing this number in the proportion of 10 to 6, we have for the effect of dew during the first and last quarters of the civil year, (or from the autumnal to the vernal equinox,)  $7\frac{1}{2}$  divisions: which makes the morning  $1\frac{1}{2}$  divisions higher than the evening maximum. This is a close approximation to the actual state of the case, as shown by the curves. The effect of the dew is partly attributable to the heat evolved, diminishing the loss of heat and of surface temperature, and partly to the direct magnetic action of the dew. If the continual diminution in the loss of temperature at night, from hour to hour, could be partially attributed to any other external cause, the effect of the dew in augmenting the horizontal force would only have to be diminished in the same proportion.

It remains for us now to consider the secondary variations of the horizontal force which occur during the forenoon. We have already seen that the horizontal force decreases from 4 or 5 A. M. to 10 A. M., although the temperature increases; and have attributed this fact to the evaporation of the dew and rain that fall during the night. This explanation involves the supposition that, in the average of months, the amount of the evaporation is greatest early in the day. That this is really the case may be inferred from the following considerations. In the first place the dew deposited at night is evaporated during the morning hours. In the next place the greater part of the evaporation of the rain that falls during the night and the latter part of the day, will have place during the forenoon of the following day; except when the



rain is a heavy one, or the ground was previously quite wet, in which case, in the same state of the sky, the evaporation will be most abundant during the warmest part of the day. The average amount of rain that falls during a single night is considerably greater than the average amount of dew. The average quantities of rain that fell during the different quarters of the year, at Philadelphia, according to the observations for the years 1842-3-4, vary from five inches to sixteen inches, which is equivalent to from 0<sup>n</sup>.03 to 0<sup>n</sup>.09 (nearly) in a single night. It is to be expected, therefore, that, if the morning decrease of the horizontal force be really attributable to evaporation, there will be variations in the amount of the decrease connected with the variations in the quantity of rain. That such a connection really exists will be manifest on consulting the following table, showing the quantities of rain, and the average decrease of the horizontal force for the different quarters of the years 1842-3-4.

	1842.		1843.		1844.	
	Rain.	Force.	Rain.	Force.	Rain.	Force.
1st Quarter,	6.8	3			7.3	2
2d "	10.6	4	10.3	3.25	5.2	2.5
3d "	11.4	5.75	15.7	4.75	10.8	3.5
4th "	7.8	3.75	8.7	2	8	3

The following table of averages will make the connection in question still more evident.

	Rain.	Ratios.	Force.	Ratios.
1st Quarter, .....	6.8		2.5	
2d " .....	8.7	1.3	3.25	1.28
3d " .....	12.6	1.45	4.66	1.43
4th " .....	8.2	.65	2.92	.63

The correspondence between the ratios is remarkably close, and would seem to indicate that, in the average of years, the diminution of the horizontal force in the morning is mainly due to the evaporation of the water that has fallen in rain and is but slightly effected by the variations in the rise of temperature, and in the amount of dew.

### *Diurnal Variations of the Vertical Magnetic Intensity.*

According to our general theory the vertical intensity is proportional to the difference of temperature of two places situated at equal distances to the north and south of the station of the needle, and on a line perpendicular to the isogothermal line. We have then to enquire whether the diurnal variations of the vertical intensity are proportional to the diurnal variations of the difference of temperature of two places thus situated. The theory strictly

requires that the difference between the average temperatures, at any time, of the ground near the surface should be taken, but there is little reason to doubt that the laws of the variations of this difference, at any one season, will be very nearly the same as of the variations of the difference of surface temperatures. Whatever errors may result from taking the latter difference instead of the former will probably be simply errors of quantity.

Although I am not in possession of the precise data demanded for a minute prosecution of the present inquiry, still the meteorological observations made at Philadelphia and Washington will furnish differences of temperature, which will doubtless, in the average for weeks and months, differ little from the differences demanded. Let us then compare the curves showing the mean diurnal variations of the vertical intensity for the different quarters of any one year, 1841 for example, with the curves showing the mean diurnal variations of the difference of temperature of Washington and Philadelphia for the same periods of time. (See figs. 13 to 20.) On examining these curves it will be seen that the maximum of vertical intensity, at all seasons of the year, is not far from the hour of maximum daily temperature (between noon and 4 P. M.) and the minimum toward midnight, and that the same is true of the differences of temperature. The curves of vertical intensity, for the other years, conform to the same general law, and the calculations of difference of temperature, so far as

Curves showing the Mean Diurnal Variations of the Vertical Force, for quarters of years.

Fig. 13.

Jan., Feb. and March, 1842.

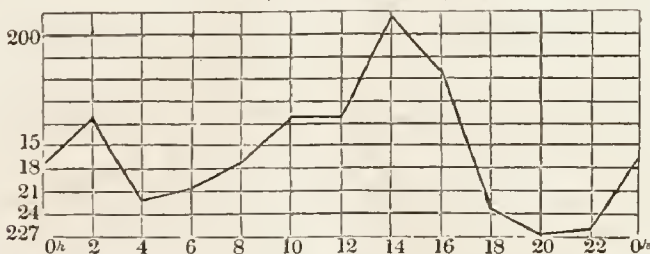
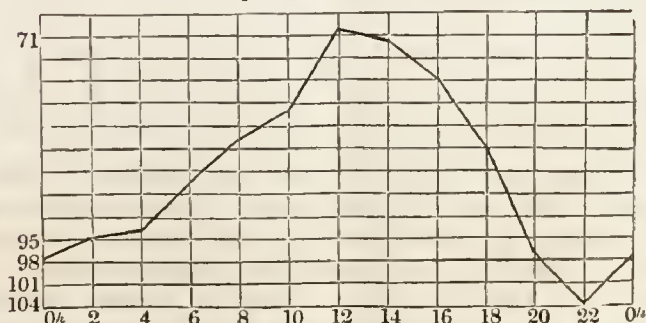


Fig. 14.

April, May and June, 1841.



Curves showing the Mean Diurnal Variations of the Vertical Force, for quarters of years.

Fig. 15. July, Aug. and Sept., 1841.

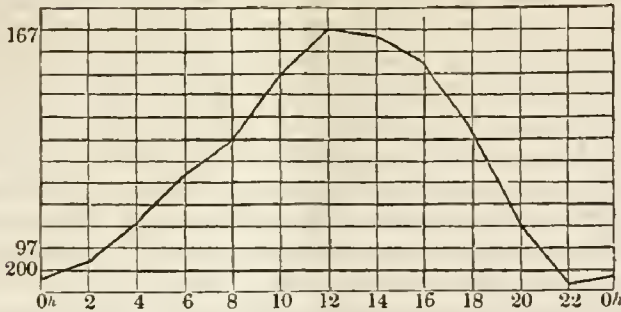
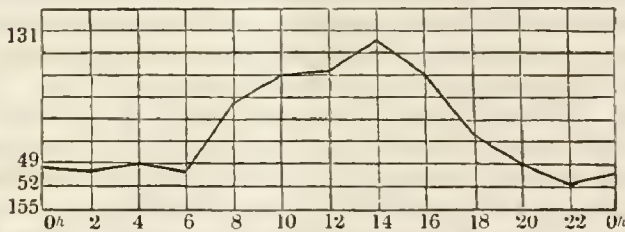


Fig. 16. Oct., Nov. and Dec., 1841.



One division of magnetometer scale = '000033 vertical force.—Increase of numbers corresponds to decrease of force.

Curves showing the Mean Diurnal Variations of the Difference between the Temperatures of Philadelphia and Washington, for quarters of years.

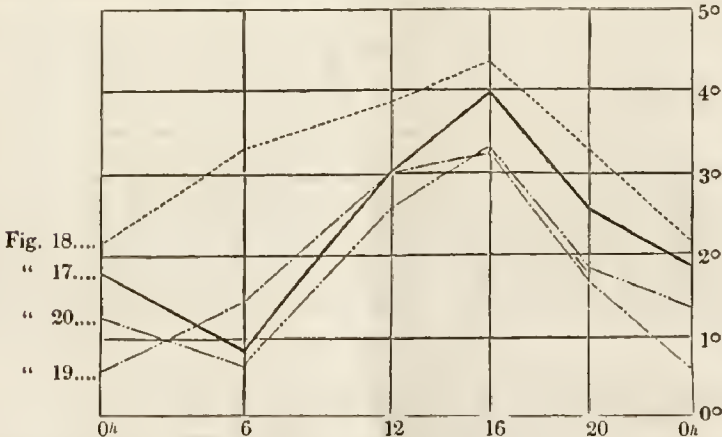


Fig. 17, Jan., Feb. and March, 1842.—Fig. 18, April, May and June, 1841.—Fig. 19, July, Aug. and Sept., 1841.—Fig. 20, Oct., Nov. and Dec., 1841.

they have been carried, serve only to confirm the conclusion that it is a general law that the difference of temperature, like the vertical intensity, is greatest between the hours of noon and 4 p. m., and least about midnight. I conclude, therefore, that the diurnal variations of the vertical intensity must be at least approximately proportional to the diurnal variations of difference of temperature.



If now we compare the curves of vertical intensity for the different quarters of the year 1841, and other years, we find that the variations are generally less for the first and last than for the other two quarters of the year. But, so far as the calculations of difference of temperature have been made, there does not appear to be an equal proportionate difference in the curves of difference of temperature. It is barely possible that this apparent discrepancy may be attributable to the fact that the data are not precisely those which the theory calls for, and that the variations of the vertical force are really the joint effect of the variations of the difference of temperature of all places situated on lines drawn through the station of the needle and at equal distances from this station; but, in all probability the principal cause is to be sought elsewhere. The first inquiry that naturally arises is whether it may not be found in the fact that, instead of taking the difference between the temperatures at the earth's surface, we should take the difference between the average temperatures of the stratum just below the surface, which is subject to a daily variation of temperature. In fact it is easy to see that if we make this correction, the vertical force ought to be less for the same difference of temperature, during the cold than during the warm months; for, the daily variation of temperature being then less, the stratum of sensible daily variation of temperature will be of less thickness. The freezing and thawing which take place in the colder months will also have the effect to diminish this thickness; since when the earth freezes at night sensible heat will be given out, which will make the cooling less than it otherwise would be, and when it thaws during the day sensible heat will be absorbed, which will have the effect to diminish the rise of temperature, and these effects are not confined to the surface of the earth, but extend to a certain depth below it. The rising and falling of vapor during the twenty-four hours will have little or no sensible effect upon the intensity of the vertical force, (unless we suppose that the vapor acts magnetically only when it is in contact with the earth's surface,) since it is chiefly the matter at a distance that is concerned in the vertical action upon the needle, and the tangential force of any particle of matter thus situated will be sensibly vertical for considerable distances both above and below the needle. The evaporation which has place during the day, and the deposition of dew during the night can have then (except upon the above supposition) little or no sensible effect upon the intensity of the vertical force, in any other way than by the heat evolved and absorbed; and this has already been tacitly allowed for, for the actual difference of temperature depends upon the deposition of dew and the evaporation, as well as upon the heating power of the sun and the radiation into space and the atmosphere.

Whether the variations in the thickness of the stratum of daily variations of temperature are sufficient to make the discrepancy above noticed, entirely disappear, must be left for future consideration. In fact, it will doubtless be necessary to have recourse to direct observations before this question can be definitively settled, and a complete explanation of all the details of the variations of the vertical intensity made out with certainty.

A farther investigation reveals the existence of other small discrepancies. These are exhibited to the eye in figs. 21 to 24, Curves of Mean Diurnal Variations of Vertical Force and Difference of Temperature.

Fig. 21.

Jan., Feb. and March, 1842.

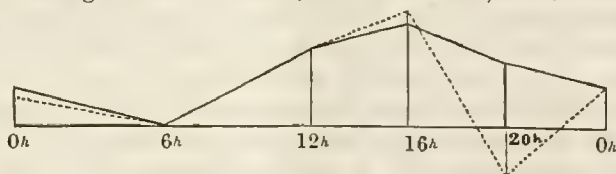


Fig. 22.

April, May and June, 1841.

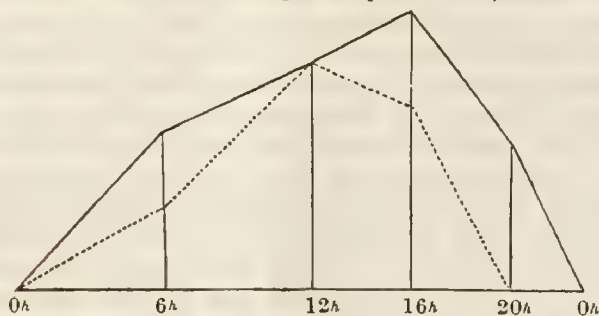


Fig. 23.

July, Aug. and Sept., 1841.

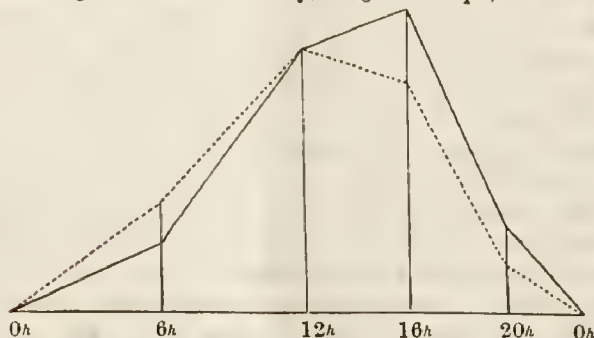
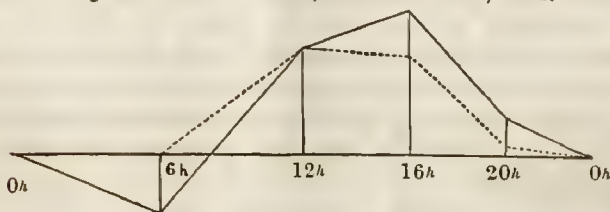


Fig. 24.

Oct., Nov. and Dec., 1841.



in which the dotted lines represent the actual variations of the vertical force, and the full lines the variations as deduced from the theory that they are proportional to the variations of the difference of temperature. On examining these figures it will be seen that there is some cause in operation, making the vertical force to decrease more rapidly in the afternoon until 8 P. M., than the difference of temperature, and to decrease less rapidly or to increase from 8 P. M. to midnight. It will be seen also that in the Spring the vertical force increases less rapidly than the difference of temperature during the latter part of the night, and more rapidly during the forenoon; and that during the latter half of the year the reverse is true. It is observable also that these discrepancies are greatest in amount during the first half of the year; that they lie continually in the same direction during the first half of the year, and also continually in the same direction during the other half of the year. As to their origin, they may be purely accidental, for the locality, or for the time; or they may arise from the fact that the variations of the difference of temperature between Washington and Philadelphia do not represent with exactness the variations of the vertical force, since these depend upon the variations of the differences of temperature of all points of the earth's surface, situated within a certain distance of the station of the needle. It would, at all events, be premature to enquire after some secondary physical cause tending to produce these effects, after so partial an examination of the facts.

The curves shown by the full lines in figs. 21 to 24, were constructed upon different scales, obtained by assuming that, for each quarter of the year, the variation of the differences of temperature from 0<sup>h</sup> to 12<sup>h</sup> be represented by the line which represents in figs. 13 to 16, the variation of the vertical force during the same interval. The coincidence of the full and dotted lines at 0<sup>h</sup> and 12<sup>h</sup> is a necessary consequence of this assumption.

#### *Diurnal Variations of the Declination.*

The general theory is, that the needle is nearly perpendicular to the isogeothermal line—that is, that the mean position of the needle is at right angles to the ideal line passing through those places which have the same mean annual temperature. But, in general, the true and mean temperature are different to a certain depth in the ground. There is a stratum of about 60 feet in depth which slowly varies in temperature from one season to another, and a portion of this stratum, of the depth of some three feet, which varies in temperature during the day. If we consider the action of this latter stratum by itself, agreeably to our general theory, its tendency at any moment will be to place the needle at right angles to the line connecting the points where the aver-

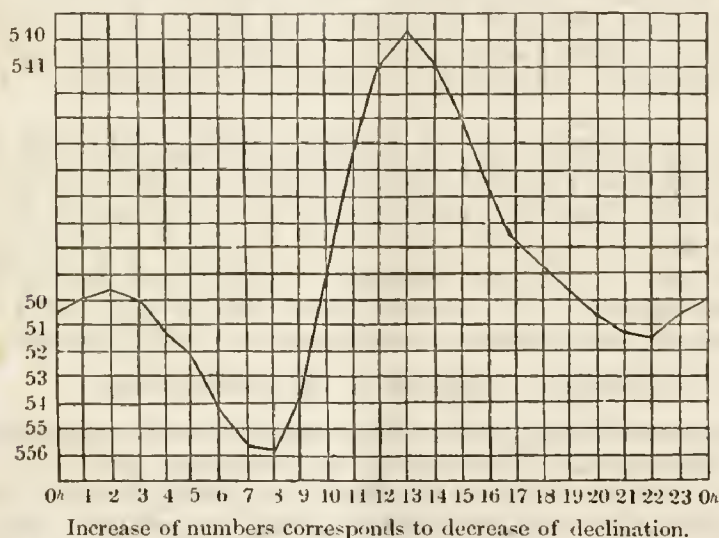


age temperature of this stratum is the same. But we have seen in the course of the present investigation, that the magnetic action of the stratum subject to daily variation of temperature, is not proportional to the temperature simply,—that it is dependent upon the evaporation of moisture and the deposition of dew. Whatever may be our explanation of the effect of dew and evaporation, the diurnal variations of the magnetic force of a particle at any place may be regarded with but little probable error, as proportional to the diurnal variations of the horizontal magnetic force there; and the tendency of these variations will be to give the needle an oscillatory movement corresponding to the continually shifting line traced through the points where that portion of the horizontal force which is due to the action of the daily stratum is the same. For the tendency of the magnetic action of this stratum, at any moment, will be to set the needle at right angles to this line; which may be regarded as identical with the line of equal molecular magnetic force.

Having presented these theoretical considerations, let us glance at some of the results of observation. Fig. 25 shows the mean

Curve showing the Mean Diurnal Variations of the Declination, for the year 1844.\*

Fig. 25.



diurnal variations of the declination, for the year 1844. The curves for the different quarters of the year are of the same form, and agree also with the curve for the year, or very nearly so, in

\* The observations of declination, (and the same is true of the observations of horizontal force and vertical force,) were made about 20m. after the Observatory hours, but as our inquiries mainly relate to the laws of the variations, this fact may be disregarded without material error.

the positions of the maxima and minima. If fig. 25 be compared with the curve of the mean daily variation of the horizontal force, (see fig. 3, p. 40, in the July number of this Journal,) it will be observed that the maxima and minima of the former fall nearly midway between the maxima and minima of the latter. Thus, the morning maximum of the horizontal force is at 5 A. M., and the minimum at 10 A. M.—and the morning minimum of declination is from 7 to 8 A. M.: again, the evening maximum of horizontal force is at about 3 P. M., and the principal maximum of declination is at 1 P. M., nearly midway between this and the morning minimum of horizontal force (at 10 A. M). The morning maximum of declination (at 2 A. M.) is also nearly midway between the evening minimum of horizontal force (about midnight), and the morning maximum of the same (at 5 A. M). There is a small deviation from this general law in the case of the evening minimum of declination, which occurs some two hours later than the middle point of time between the evening maximum and minimum of horizontal force. Besides these relations between the maxima and minima of the two curves, it may be seen that there are points of inflexion in the curves of the horizontal force near the epochs of the maxima and minima of the declination; and accordingly that when the curve of the horizontal force is concave upwards the declination (westerly) is increasing, and when it is convex upwards the declination is decreasing. These facts render it highly probable that the diurnal variations of the horizontal force and declination are linked together by some physical connection, as theory has already led us to suppose.

Let us see whether this theory, besides suggesting the fact of such a connection, can also explain the precise connection which we have now found to subsist. If we recur to the principles already laid down (p. 359), we shall see that the inquiry before us leads us, in the first place, to seek for the daily changes of position in the line of equal molecular magnetic force. If we were to neglect the effect of dew and evaporation, the line in question would be very nearly the true isothermal line passing through the station of the needle. To simplify the matter we will for the present, consider the two as the same: Now take some point (B), to the east of the station (A) of the needle, situated on the isothermal line traced through A at 5 A. M., about the time of minimum temperature: an hour later these two points would not be on the same isothermal line, for the increase of temperature at B would be greater than at A. (See curve of daily variation of temperature.) The isothermal line through A would therefore be directed to the north of B. It is obvious that this motion of the isothermal line toward the north, to the east of the station A, will continue until the increment of tempera-

ture at B, in any short interval of time, becomes the same as at A. This will happen at the hour of the most rapid rise of temperature, or about 9 A. M. After this the hourly increment of temperature will be less for B than for A, and the same portion of the isothermal line will move southward. This southerly movement will continue beyond the time of maximum temperature (3 P. M. at Philadelphia), and until the fall of temperature at B becomes less rapid than at A. This will happen about 7 P. M. It is manifest, from the concave form of the curve of daily variation of temperature, during the night, that after this the decrease of temperature at B will be continually less than at A, and therefore that the isothermal line will move northward, to the east of station A, and southward to the west of it. This motion will continue until 5 A. M.; and beyond this, as we have already seen, until towards 9 A. M. In obtaining these results, we have taken it for granted that the law and rate of the mean daily variation of temperature is the same at B as at A. This doubtless is not strictly true, and therefore the epochs of maximum and minimum of declination should be somewhat different from the times above specified. If we neglect this difference, it appears, that on the supposition which has been made, the needle would move toward the east from 9 A. M. to 7 P. M., and toward the west from 7 P. M. to 9 A. M. The actual state of things differs from this in two or three points; during the last half of this period of westerly movement, or nearly so, there is actually an easterly movement, and during the first half of this period of easterly movement there is actually a westerly movement; and the evening minimum occurs generally some two or three hours later, (about 10 P. M.) These discrepancies, (with the exception of the last, which is comparatively trifling,) disappear if we compare the curve of declination (fig. 25) with that of horizontal force (fig. 3), instead of that of temperature, as we should do. If this be done, it is found, as we have already seen, that the points of maximum variation of the horizontal force, or of inflexion in the curve, fall at the epochs of the maximum and minimum of declination.

To understand the movements, in detail, of the line of equal surface magnetic action, upon which the daily horizontal movements of the needle depend, we have only to compare the change of the horizontal force during the hour following the time considered, with the change that occurs an hour later during the same interval of one hour; for the latter is the change that occurs at a place an hour to the east of the station of the needle, contemporaneously with the change at the station itself. When these two changes are equal the line in question is stationary. When they are both decrements, if the first is greater than the second, as from 8 A. M. to 10 A. M., the line rises, to the east of the station, and the needle moves westwardly; but if it is less, as from 6 A. M.



to 8 A. M., the line moves towards the south and the needle eastwardly. When the changes are both increments, if the first is less than the second, as from 10 A. M. to 1 P. M., the line rises and the needle moves toward the west; but if it is greater than the second, as from 1 P. M. to 4 P. M., the line falls back, or toward the south, and the needle moves towards the east. When the first change is a decrement and the second an increment, as at about 10 A. M. and toward midnight, the line rises and the motion of the needle is toward the west; and when the first is an increment and the second a decrement, as near 3 P. M. and 5 A. M., it falls and the needle moves eastward. It appears therefore that the needle should move toward the west when the curve of the horizontal force is concave upward, and toward the east when the same curve is convex upward. The westerly movement should then be from 8 A. M. to 1 P. M., and from 7 or 8 P. M. to 2 or 3 A. M.; and the easterly movement from 1 P. M. to 7 or 8 P. M., and from 2 or 3 A. M. to 8 A. M. These results accord with observation, with the single exception, that the time of minimum declination is generally about two hours later than 8 P. M.

If we compare the curves of horizontal force for the different quarters of the year, we find that, while the points of maxima and minima, as well as the points of inflexion, are pretty nearly the same for all, the curvatures are in general greater for the two middle than for the first and last quarters of the year, and therefore the daily changes of declination should be greater toward the middle than toward the beginning or end of the year—a result which accords with fact. There appears, however, to be generally a more rapid variation of the horizontal force toward midnight, during the cold than during the warm months; which must be attended with corresponding differences in the small nocturnal increase of declination. This result seems also to be in accordance with fact; but it would be premature to attempt the detailed explanation of such minute differences among the variations, from a limited series of observations made only at the station of the needle. A similar remark may be made with respect to certain small discrepancies which may be observed, between theory and fact, in relation to the relative amounts of the variations at the same hour in the different quarters of any one individual year. A theory which furnishes a sufficient explanation of all the laws deducible from the observations, cannot reasonably be rejected on the score of small discrepancies in quantity, when the observations are much less extended than the theory calls for. The precise movements of the line of equal magnetic action of the surface stratum upon which the motion of the needle depends, can only be ascertained with certainty by instituting special observations at a variety of places in every direction and at various distances from the station of the needle. The

foregoing results have been obtained by taking it for granted that the changes of the molecular magnetic intensity are the same at the same hour of local time all around the station of the needle as at the station itself; which is doubtless not strictly true.

It remains for us now to enquire into the amount of the actual daily angular movement of the line of equal magnetic action, and into the intensity of the disturbing force necessary to the production of the amount of movement of the needle which actually occurs. Let us, in the first place, regard this line as identical with the true isothermal line, and suppose B to be a place situated on this line at 5 A. M.\* (or thereabouts), and one hour, say, to the east of the station A. By 9 A. M. the rise of temperature at B will be as much as  $2^{\circ}$ , on the average, greater than at A, and therefore the isothermal line through A will now pass through a point (C), to the north of B, where the temperature is  $2^{\circ}$  less than at B. In the present discussion the station A is Philadelphia, and it appears from an examination of the differences of temperature at various hours during the day, between Washington and Philadelphia, used in the discussion of the vertical force, that the difference of latitude of B and C is about equal to the difference of latitude of Washington and Philadelphia, or about  $1^{\circ}$ . Taking this result and conceiving the isothermal line to be an arc of a great circle, we find, by an easy calculation, the displacement of this line from 5 A. M. to 9 A. M. to be about  $3\frac{3}{4}^{\circ}$ . The actual isothermal line, at 9 A. M., will really lie a little to the north (as far as C) of the great circle taken for it, since the variation of temperature about 9 A. M. is nearly uniform. This angle ( $3\frac{3}{4}^{\circ}$ ) is, however, only a part of the daily angular movement of the isothermal line toward the north, to the east of A, since, as we have already seen (p. 361), this movement begins about 7 P. M. and continues until 8 or 9 A. M. The whole movement cannot be less than  $5^{\circ}$ . This calculation proceeds upon the supposition that the daily variation of temperature at B is the same as at A, but as a matter of fact the change is somewhat greater for A than for B; since, as we have seen, the difference of temperature of Washington and Philadelphia increases during the forenoon, and A is south and west of B. This will have the effect to diminish the rise of the line of equal magnetic force, from 5 A. M. to 9 A. M., (possibly to  $2^{\circ}$ ); but, as the fall of temperature will be more rapid after 4 P. M., at A than at B, the westerly movement will begin earlier in the evening and attain to a greater amount by 5 A. M. than upon the supposition made above. We may lay it down then as highly probable that the displacement of the line in question cannot be less than  $3^{\circ}$ .

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\* The time considered in this connection is the local time at the station A of the needle.

If now we take the line of equal magnetic action, as it has been accurately defined, by its relations to the horizontal force instead of the temperature, we have to consider the amount of the variation of the horizontal force from the minimum at 10 A. M. to the time of greatest hourly variation, at 1 P. M., and enquire how far to the north of B we must go to obtain a horizontal force as much less than that at B as that at B is greater than that at A. But we here meet with a difficulty, inasmuch as the horizontal forces, so called, at B and C (to the north of B), to be compared, are really only the portions of the entire horizontal force, which are due to the action of the variable stratum. The difference between these forces is very much less than that which subsists between the actual horizontal directive forces acting upon the needle, but how much less we have no means as yet of ascertaining with any certainty. For this purpose we must know the proportion which the force of the variable stratum bears to that of the whole magnetic stratum.

Conjectures might be made as to the probable value of the proportion between these forces, but without attempting to enter upon such uncertain ground, I will content myself with remarking that no proportion which, in the light of the investigations of this and the previous paper, seems probable, gives movements of the line of equal surface magnetic force materially less than those of the isothermal line, above determined.

As to the question of the intensity of the disturbing force which produces the diurnal variations of declination, it is, in the first place to be observed, that that portion of the horizontal force, which is due to the action of the variable stratum, is entirely effective in displacing the needle in a direction toward the perpendicular to the line of equal magnetic action of this stratum. Now it appears, upon calculation, that this force must be as much as thirty times its daily variation, in the summer, to produce upon the supposition of a displacement of this line to the amount of  $6^{\circ}$ , a change of declination amounting to  $12'$ .





# MEMOIR ON METEORITES.

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## A DESCRIPTION OF FIVE NEW METEORIC IRONS,

WITH SOME THEORETICAL CONSIDERATIONS ON THE ORIGIN  
OF METEORITES BASED ON THEIR PHYSICAL AND  
CHEMICAL CHARACTERS.

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(Read before the American Association for the Advancement of Science, April, 1854.)

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### 1. *Meteoric Iron from Tazewell County, East Tennessee.\**

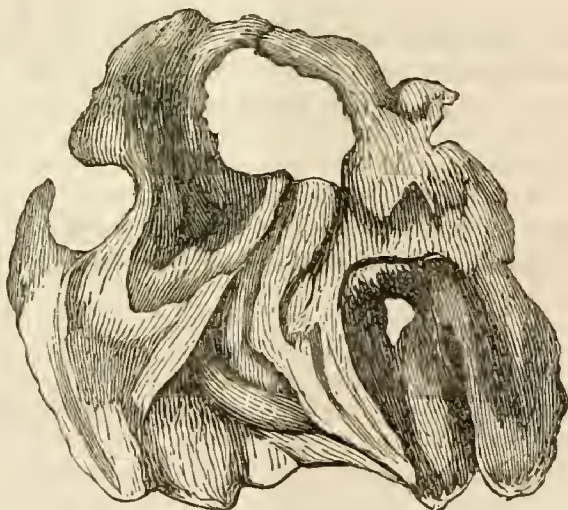
THIS meteorite was placed in my possession through the kindness of Prof. J. B. Mitchell of Knoxville, in the month of August, 1853. It was found by a son of Mr. Rogers living in that neighborhood, while engaged in ploughing a hill-side; his attention was drawn to it by its sonorous character. As it very often happens among the less informed, it was supposed to be silver or to contain a large portion of that metal. With some difficulty the mass was procured by Prof. Mitchell, and passed over to me. Nothing could be ascertained as to the time of its fall; it is stated among the people living near where the meteorite was found, that a light has been often seen to emanate from and rest upon the hill, a belief that may have had its foundation in the observed fall of this body.

The weight of this meteorite was fifty-five pounds. It is of a flattened shape, with numerous conchoidal indentations, and three annular openings passing through the thickness of the mass near

\* Notice of the discovery of this iron was given by me in 1853.—J. L. S.

the outer edge. Two or three places on the surface are flattened, as if other portions were attached at one time, but had been rusted off by a process of oxydation that has made several fissures in the mass so as to allow portions to be detached by the hammer, although when the metal is sound the smallest fragment could not be thus detached, it being both hard and tough. Its dimensions are such that it will just lie in a box 13 inches long, 11 inches broad and  $5\frac{1}{2}$  inches deep. The accompanying figure gives a correct idea of the appearance of this meteorite.

1.



The exterior is covered with oxyd of iron, in some places so thin as hardly to conceal the iron, in other places a quarter of an inch deep. Its hardness is so great that it is almost impossible to detach portions by means of a saw. Its color is white, owing to the large amount of nickel present; and a polished surface when acted on by hot nitric acid displays in a most beautifully regular manner the Widmannstättian figures. The specific gravity taken on three fragments selected for their compactness and purity, is from 7.88 to 7.91.

The following minerals have been found to constitute this meteorite: 1st. *Nickeliferous iron*, forming nearly the entire mass. 2nd. *Protosulphuret of iron*, found in no inconsiderable quantity on several parts of the exterior of the mass. 3d. *Schreibersite*, found more or less mixed with the pyrites and in the crevices of the iron, in pieces from the thickness of the blade of a penknife to that of the minutest particles. 4th. *Olivine*; two or three very small pieces of this mineral have been found in the interior of the iron. 5th. *Protochlorid of iron*; this mineral has been found in this meteorite *in the solid state*, which I believe is the first observation of this fact; it was found in a crevice



that had been opened by a sledge hammer, and in the same crevice Schreibersite was found. Chlorid of iron is also found deliquescing on the surface ; some portions of the surface are entirely free from it, while others again are covered with an abundance of rust arising from its decomposition.

Besides the above minerals two others were found, one a siliceous mineral, the other in minute rounded black particles ; both, however were in too small quantity for any thing like a correct idea to be formed of their composition.

The different minerals that admitted of it, were examined chemically, and the following are the results :

1. *Nickeliferous Iron*.—The specific gravity of this iron is as already stated, from 7.88. to 7.91. It is not readily acted on by any of the acids in the cold ; nitric acid, either concentrated or dilute, has no action on it until heated to nearly 200° Fahr., when the action commences, and continues with great vigor even after the withdrawal of heat. With reference to the action of sulphate of copper, it is *passive*, although when immersed in a solution of sulphate of copper and allowed to remain for several hours the latter metal deposits itself in spots on the surface of the iron.

Thorough digestion in hot nitric acid dissolves the iron completely. When boiled with hydrochloric acid the iron dissolves with the liberation of hydrogen, leaving undissolved the Schreibersite ; but by long continued action this latter is also dissolved with the evolution of phosphuretted hydrogen.

The following ingredients were detected on analysis of two specimens :

	1.	2.
Iron, . . . . .	82.89	83.02
Nickel, . . . . .	15.02	14.62
Cobalt, . . . . .	.43	.50
Copper, . . . . .	.09	.06
Phosphorus, . . . . .	.16	.19
Chlorine, . . . . .		.02
Sulphur, . . . . .		.08
Silica, . . . . .	.46	.84
Magnesia, . . . . .		.24
	<hr/> 98.55	<hr/> 99.57

Tin and arsenic were looked for, but neither of those substances detected. The magnesia and silica are doubtless combined, probably in the form of olivine, and disseminated in minute particles through the iron. The phosphorus is in combination with a given portion of iron and nickel, forming Schreibersite ; the 16 per cent. of phosphorus corresponds to 1.15 of Schreibersite : so the metal mass may be looked on as composed of

Nickeliferous iron 98.97, Schreibersite 1.03=100.00.

The composition of the nickeliferous iron corresponds to five atoms of iron and 1 of nickel.

Iron,	5 atoms,	.	.	.	.	82.59
Nickel,	1 "	.	.	.	.	17.41=100.00

2. *Protosulphuret of Iron*.—This variety of sulphuret of iron found with meteorites is usually designated as magnetic pyrites, leaving it to be inferred that its composition is the same as the terrestrial variety. Without alluding to the doubt among some mineralogists as to the true composition of the terrestrial magnetic pyrites, I have only to say that most careful examination of the sulphuret detached from the meteorite in question proves it to be a protosulphuret; a conclusion to which Rammelsberg had already come, with reference to the pyrites of the Seelasgen iron, which latter pyrites I have also examined, confirming the results of Rammelsberg.

This pyrites encrusts some portion of the iron, and in places is mixed with a little Schreibersite. It presents no distinct crystalline structure, has a grey metallic lustre, and a specific gravity of 4.75. The Seelasgen pyrites gave me for specific gravity 4.681.

The specimen of pyrites in question gave, on analysis:

Iron 62.38, sulphur 35.67, nickel 0.32, copper *trace*, silica 0.56, lime 0.08 = 98.91.

The formula  $\text{Fe S}$  requires sulphur 36.36, iron 63.64.

The magnetic property of this mineral is far inferior to that possessed by Schreibersite.

3. *Schreibersite*.—It is found disseminated in small particles through the mass of the iron, and is made evident by the action of hydrochloric acid; it is also found in flakes of little size, inserted as it were into the iron, and owing to the fact that in many parts where it occurs chlorid of iron also exists, this last has caused the iron to rust in crevices, and on opening these, Schreibersite was detached mechanically. This mineral as it exists in the meteorite in question, so closely resembles magnetic pyrites that it can be readily mistaken for this latter substance, and I feel confident in asserting that a great deal of the so-called magnetic pyrites associated with various masses of meteoric iron, will upon examination, be found not to contain a trace of sulphur, and will on the contrary prove to be Schreibersite that can be easily recognised by the characters to be fully detailed a little farther on.

Its color is yellow or yellowish white, sometimes with a greenish tinge; lustre metallic; hardness 6; specific gravity 7.017. No regular crystalline form was detected; its fracture in one direction is conchoidal. It is attracted very readily by the magnet, even more so than magnetic oxyd of iron; it acquires polarity and retains it. I have a piece  $\frac{3}{10}$  of an inch long,  $\frac{2}{10}$  of an inch broad, and  $\frac{1}{10}$  of an inch thick, which has retained its polarity over six months; unfortunately the polarity was not tested immediately when it was detached from the iron, and not until it had come in contact with a magnet, so that it cannot be pronounced as originally polar.

Before the blowpipe it melts readily, little blisters forming on the surface from the escape of *chlorine*, and blackens. The magnet is a most ready means of distinguishing the Schreibersite from the pyrites commonly found in meteoric irons, for although the pyrites is attracted by the magnet, it is necessary that the latter should be brought quite near to it for the effect to be produced, whereas if the particles exposed to the magnet be Schreibersite, they will be attracted with almost the readiness of iron filings.

Hydrochloric acid acts exceedingly slowly on this mineral when pulverized, with the formation of phosphuretted hydrogen. Nitric acid acts more vigorously and readily dissolves it when finely pulverized. The composition of this substance has in all cases but one, been made out from the residue of meteoric iron, after having been acted on by hydrochloric acid, which accounts for the great variation in the statements of the proportion of its constituents.

Mr. Fisher examined pieces of Schreibersite detached from the Braunau iron, with the following results: Iron 55.430, nickel 25.015, phosphorus 11.722, chrome 2.850, carbon 1.156, silic 0.985 = 98.158.

The results of my analyses do not differ very materially from this; they are as follows:

	1.	2.	3.
Iron, . . . . .	57.22	56.04	56.53
Nickel, . . . . .	25.82	26.43	28.02
Cobalt, . . . . .	0.32	0.41	0.28
Copper, . . . . .	trace	not estimated.	
Phosphorus, . . . . .	13.92		14.86
Silica, . . . . .	1.62		
Alumina, . . . . .	1.63		
Zinc, . . . . .	trace	not estimated.	
Chlorine, . . . . .	0.13		
	<hr/> 100.66		<hr/> 99.69

Nos. 1 and 2 were separated mechanically from the iron. No. 3 chemically. The silica, alumina and lime were almost entirely absent from No. 3, and in the other specimen they are due to a siliceous mineral that I have found attached in small particles to the Schreibersite. There is no essential difference in my results, yet in neither instance do I suppose the mineral was obtained perfectly pure; although enough so, it is believed, to furnish the correct chemical formula; and, as from what has been previously said, Schreibersite will be found to exist in larger quantities than it was suspected, it will not be long before the question of the uniformity of its composition will be settled, a point of interest bearing upon the theoretical consideration of meteoric stones.

The formula of Schreibersite, I consider to be  $\text{Ni}_2\text{Fe}_4\text{P}$ .

		Per cent.
Phosphorus, . . . . .	1 atom	15.47
Nickel, . . . . .	2 "	29.17
Iron, . . . . .	4 "	55.36



This mineral although not usually much dwelt upon when speaking of meteorites, is decidedly the most interesting one associated with this class of bodies, even more so than the nickeliferous iron. It has no representative in genus or species among terrestrial minerals, and is one possessed of highly interesting properties. Although among terrestrial minerals phosphates are found, not a single phosphuret is known to exist; so true is this (that with our present knowledge) if any one thing could convince me more strongly than another of the non-terrestrial origin of any natural body, it would be the presence of this or some similar phosphuret. It is commonly alluded to as a residue from the action of hydrochloric acid upon meteoric iron, when in fact it exists in plates and fragments of some size in almost all meteoric iron; and there is reason to believe that it is never absent from any of them in some form or other: what is meant by "some size" is, that it is in pieces large enough to be seen by the naked eye, and to be detached mechanically.

In an examination of the meteoric specimens in the Yale College Cabinet, more than half of them have been discovered to contain Schreibersite visible to the eye, that had been considered pyrites. Among them, the large Texas meteorite was examined, and although none was visible on the surface, a small fragment of the same mass given me by Prof. Silliman, contains a piece of Schreibersite of over a grain weight.

The reason why it has not attracted more attention, arises from its resemblance to pyrites; I will therefore state a ready manner of telling whether it be such or not.

Detach a small fragment, and hold a magnet capable of sustaining five or six ounces or more, within half an inch or an inch of the fragment, if it be Schreibersite it will be attracted with great readiness; the magnetic pyrites requiring a very close approximation of the magnet before attracted. This, with some little experience, becomes a ready method of separating the two. It is not, however, to be expected that this method alone, is to satisfy us, when other means can be appealed to for distinguishing this mineral; the following is one which is readily accomplished with the smallest fragment (half a milligramme). Melt in a small loop of platinum wire, a little carbonate of soda, add the smallest fragment of nitrate of soda and the piece of mineral, hold the mixture in the flame of a lamp for two or three minutes; place the bead of soda in a watch glass, add a little water and filter; to the filtrate add a drop or two of acid to neutralize the excess of carbonate of soda; evaporate nearly to dryness; add a drop of ammonia, and then a drop of ammoniacal sulphate of magnesia, when the double phosphate of magnesia and ammonia will show itself, and the crystalline form will be recognised under the microscope. If the piece examined be several milligrammes in weight,

the operation can be carried on in a small platinum capsule. This reaction can also be had by acting on the mineral, however small the piece, by aqua-regia, evaporate until only a little of the liquid is left, add a little tartaric acid, then a drop or two of ammonia to supersaturate the acid, and lastly a little ammoniacal sulphate of magnesia, when the crystals of the double phosphate of magnesia and ammonia will appear.

4. *Protochlorid of Iron*.—In breaking open one of the fissures of this meteoric iron, a small amount of a green substance was obtained, that was easily soluble in water, and although not analyzed quantitatively, it left no doubt upon my mind as to its being protochlorid of iron; and the manner of its occurrence gave strong evidence of its being an original constituent of the mass, and not formed since the fall of the mass. Chlorid of iron was apparent on various parts of the iron by its deliquescence on the surface.

## 2. *Meteoric Iron from Campbell County, Tenn.*

This meteorite was discovered in July, 1853, in Campbell County, Tennessee, in Stinking Creek, which flows down one of the narrow valleys of the Cumberland mountains. It was found by a Mr. Arnold in the channel of this stream, and having been obtained by Prof. Mitchell of Knoxville, he kindly presented it to me. It is a small oval mass  $2\frac{1}{4}$  inches long,  $1\frac{3}{4}$  broad, and  $\frac{3}{4}$  thick, with an irregular surface and several cavities perforating the mass. It was covered with a thin coat of oxyd; and on one half of it chlorid of iron was deliquescent from the surface, while on another portion there was a thin siliceous coating.

The iron composing the mass was quite tough, highly crystalline, and exhibited small cavities on being broken, resembling very much in this respect, as well as in many other points, the Hommony Creek iron; a polished surface when etched, exhibited distinct irregular Widmannstättian figures.

The weight is  $4\frac{1}{2}$  ounces. Specific gravity, 7.05. The lowness of the specific gravity is accounted for by its porous nature. Composition—

Iron,	97.54
Nickel,	0.25
Cobalt,	0.6
Copper, too small to be estimated.	
Carbon,	1.50
Phosphorus,	0.12
Silica,	1.05
	<hr/> 100.52

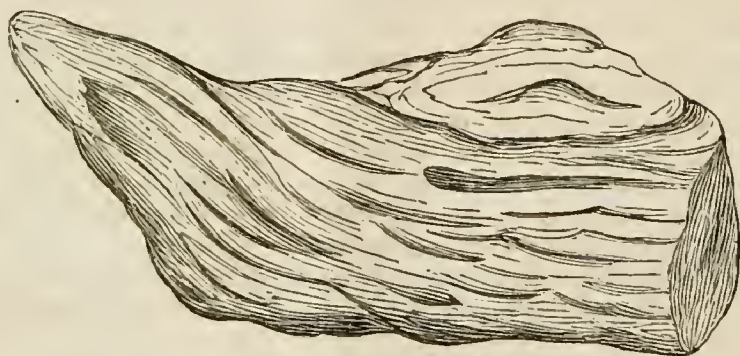
Chlorine exists in some parts in minute proportion. The amount of nickel, it will be seen is quite small, but its composition is nevertheless perfectly characteristic of its origin.

### *3. Meteoric Iron from Coahuila, Mexico.*

This meteorite was brought to this country by Lieut. Gouch, of the U. S. Army, he having obtained it at Saltillo. It was said to have come from the Sancha estate, some fifty or sixty miles from Santa Rosa in the north of Coahuila; various accounts were given of the precise locality, but none seemed very satisfactory. When first seen by Lieut. Gouch, it was used as an anvil, and had been originally intended for the Society of Geography and Statistics in the city of Mexico. It is stated that where this mass was found, there are many others of enormous size; these stones, however, it is well known, are to be received with many allowances. Mr. Weidner, of the mines of Freiberg, states that near the southwestern edge of the Balson de Mapimi, on the route to the mines of Parral, there is a meteorite near the road of not less than a ton weight. Lieut. Gouch also states that the intelligent but almost unknown Dr. Berlandier, writes in his journal of the commission of limits, that at the Hacienda of Venagas there was (1827) a piece of iron that would make a cylinder one yard in length with a diameter of ten inches. It was said to have been brought from the mountains near the Hacienda. It presented no crystalline structure, and was quite ductile.

The meteoric mass in question, which is at the Smithsonian Institution, is of the form represented in the figure, and one well

2.



adapted for an anvil. Its weight is 252 lbs., and from several flattened places, I am led to suppose that pieces have become detached. The surface, although irregular in some places, is rather smooth, with only here and there thin coatings of rust, and, as might be expected, but very feeble evidence of chlorine, and that only on one or two spots on the surface. Specific gravity 7.81. It is highly crystalline, quite malleable, and not difficult to cut with the saw. Its surface etched with nitric acid, presents the Widmannstättian figures, with a finely specked surface between



the lines, resembling the representation we have of the etched surface of Hauptmannsdorf iron. Schreibersite is visible in the iron, but so inserted in the mass, that it cannot be readily detected by mechanical means. Hydrochloric acid leaves a residue of beautifully brilliant patches of this mineral.

Subjected to analysis, it was found to contain

Iron,	95.82	Which corresponds to	
Cobalt,	.35	Nickeliferous Iron,	98.45
Nickel,	3.18	Schreibersite,	1.55
Copper, minute quantity not estimated.			
Phosphorus,	0.24		
	<hr/>		<hr/>
	99.59		100.00

The iron is remarkably free from other constituents. It is especially interesting as the largest mass of meteoric iron in this country next to the Texas meteorite at Yale College.

#### 4. *Meteoric Iron from Tucson, Mexico.*

We have had several accounts of meteoric masses which exist at Tucson; Dr. J. L. LeConte having made them known some few years ago. Since that time Mr. Bartlett, of the Boundary Commission, has seen them and made a drawing of one which he has kindly allowed me the use of, as well as the manuscript\* notice of them, which is however, quite brief. This mass is used for an anvil, resembles native iron, and weighs about six hundred pounds. Its greatest length is five feet. Its exterior is quite smooth, while the lower part which projects from the larger leg is very jagged and rough. It was found about twenty miles distant towards Tubac, and about eight miles from the road where we are told are many larger masses. The following figure (3) represents the appearance of that meteorite.

Since my communication last April, I have obtained fragments of the meteorite from Lieut. Jno. G. Parke, of the U. S. Topographical Engineers, who cut them from the mass at Tucson, and to whose kindness I feel much indebted.

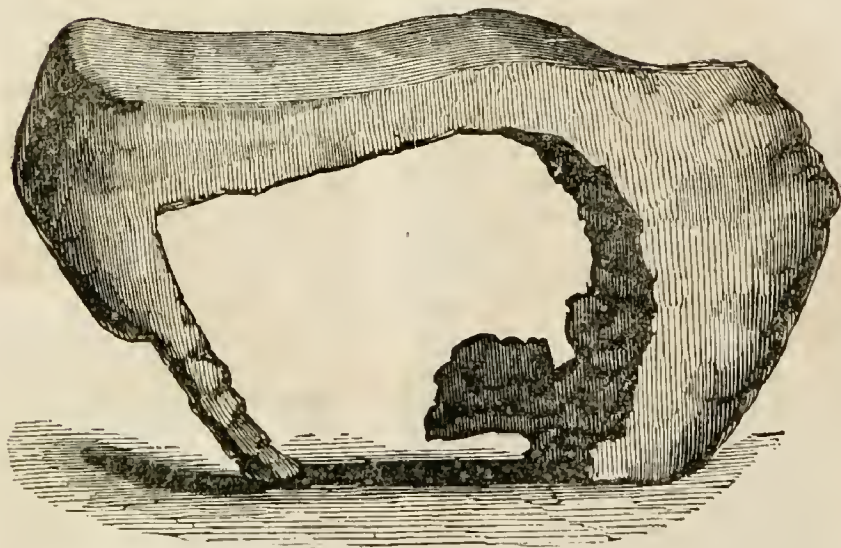
Some of the fragments were entirely covered with rust, and in some parts, little blisters existed, arising from chlorid of iron. Portions of the broken surface retain their metallic lustre untarnished. The Widmannstätten figures are very imperfectly developed, owing to the porous nature of the iron, the pores of which are filled with a stony mineral. The specific gravity taken on three specimens were 6.52—6.91—7.13. The last was the most compact and free from stony particles that could be found, and upon that the chemical examination was made.

\* Since this was communicated to the American Association for the Advancement of Science, Mr. Bartlett's valuable and instructive work, entitled "Personal Narrative of Explorations in Texas, New Mexico, California, Sonora, and Chihuahua," has been published in two handsome octavo volumes, by the Messrs. Appleton's, New York: and we are indebted to the publishers for the use of Mr. Bartlett's fine cuts on the following pages.—J. L. S.

On examination it is seen to consist of two distinct parts, metallic and stony; the latter was only in minute particles, yet it was impossible, among the specimens at my disposal, to find a piece that was without it. On analysis, the following ingredients were found :

Iron, . . . .	85.54	Which represent the following minerals :	
Nickel, . . . .	8.55		
Cobalt, . . . .	.61	Nickeliferous iron, . . . .	93.81
Copper, . . . .	.03	Chrome iron, . . . .	.41
Phosphorus, . . . .	.12	Schreibersite, . . . .	.84
Chromic oxyd, . . . .	.21	Olivine, . . . .	5.06
Magnesia, . . . .	2.04		
Silica, . . . .	3.02		100.12
Alumina. . . .	<i>trace</i>		
	100.12		

3.



Some few particles of olivine were separated mechanically, and readily recognised as such under the magnifying glass in connection with the action of acids, which readily decompose it, furnishing silica and magnesia. Some of the olivine is in a pulverulent condition, resembling that of the Atacama iron. The nickeliferous iron of this Tucson meteorite also resembles that of the Atacama iron; calculated from the above results, it consists of

Iron 90.91, nickel 8.46, cobalt 63, copper, *trace*=100.00.

'This meteorite\* is one of much interest, and it is to be hoped that some of our enterprising U. S. Topographical Engineers

\* Since my notice of this meteorite, Prof. C. U. Shepard has published (*Am. Journal of Science*, Nov. 1854) an account of it, not aware of my communication to the *Am. Assoc.* He seems inclined to think that the stony material might be chladnite, although he could form no definite conclusion on this head. From what has

will yet be able to persuade the owners to part with it and bring it to this country.

#### 5. *Meteoric Iron of Chihuahua, Mexico.*

For the description of this meteorite, I am indebted to the manuscript of Mr. Bartlett, and had hoped to have obtained a fragment of it for examination from Dr. Webb, who detached pieces from the mass; but when applied to, they were no longer

4.



in his possession. It exists at the *Hacienda de Concepcion*, about ten miles from Zapata. "The form is irregular. Its greatest height is forty-six inches; greatest breadth thirty-seven inches; circumference in thickest part eight feet three inches. Its weight as given by Senor Urquida, is about three thousand eight hundred and fifty-three pounds. It is irregular in form, as seen by the figure; and one side is filled with deep cavities, generally round and of various dimensions. At its lower part, as it now stands, is a projecting leg, quite similar to the one on the meteorite at Tucson. The back or broadest part is less jagged than the other portions, and contains fewer cavities, yet, like the rest, is very irregular."

been said in the text, it will be seen to be olivine, the chladnite of the Bishopville stone not being attacked by acid, or only to a very feeble extent, by boiling sulphuric acid. And I would here remark that from some investigations just made, chladnite is likely to prove a pyroxene.—J. L. S.



SOME THEORETICAL CONSIDERATIONS CONNECTED  
WITH METEORITES.

Under this head no mention will be made of the phenomena accompanying the fall of meteorites, as their light, noise, bursting, and their black coating; which arise after the bodies have entered the atmosphere, and are brought about by its agency. This omission will affect in no way the theoretical views under consideration, and the introduction of these particulars would uselessly increase the length of this memoir.

The lessons to be learned from meteorites, both stony and metallic, are probably not as much appreciated as they ought to be; we are usually satisfied with an analysis of them and surmises as to their origin, without due consideration of their physical and chemical characters.

The great end of science is to generalize facts that are observed. Thus terrestrial gravitation has been extended to the solar system, and in fact to the whole universe. The astronomer by his discoveries only proves the universality of this one law of nature operating on matter; he has found no evidence that any other force pertaining to terrestrial matter displays itself in a similar manner in other spheres. However true and self-evident it may appear that all matter in space is under the same laws, be they those of gravitation, cohesion, chemical affinity, etc., it is none the less interesting to have the fact proved, and meteorites when looked upon as bringing these proofs acquire additional interest.

Meteorites studied in the way just mentioned, lead us to the inference that the materials of the earth are exact representatives of the materials of our system, for up to the present time, no element has been found in a meteorite that has not its counterpart on the earth; or if we are not warranted in making such a broad assumption, we certainly have the proof, as far as we may ever expect to get it, that materials of other portions of the universe are identical with those of our earth.

Meteorites also show that the *laws of crystallization* in bodies foreign to the earth, are the same as those affecting terrestrial matter, and in this connection we may instance pyroxene, olivine and chrome iron affording in their crystalline form angles identical with those of terrestrial origin.

But perhaps of all the interesting facts under this head developed by meteorites, is the universality of the laws of chemical affinity, or the truth, that all the laws of chemical combination and atomic

constitution are to be equally well seen in extra-terrestrial and terrestrial matter; so that were Dalton or Berzelius to seek for the atomic weights of iron, silica or magnesia they might learn them as well from meteoric minerals as from those taken from the bowels of the earth. The atomic constitution of meteoric anorthite or of pyroxene is the same as that which exists in our own rocks.

Keeping in view then the physical and chemical characters of meteorites, I propose to offer some theoretical considerations which to be fully appreciated must be followed step by step. These views are not offered, because they individually possess particular novelty; it is the manner in which they are combined, to which especial attention is called.

*Physical Characteristics to be noted in Meteorites.*—The first physical characteristic to be noted is their form. No masses of rock, however rudely detached from a quarry, or blasted from the side of a mountain, or ejected from the mouth of a volcano, would present more diversity of form than meteoric stones: they are rounded, cubical, oblong, jagged, flattened, and in fine they present a great variety of fantastic shapes. Now the fact of form I conceive to be a most important point for consideration in regard to the origin of these bodies; as the form alone is strong proof that the individual meteorites have not always been cosmical bodies, for had they been, their form must have been spherical or spheroidal; as this is not so it is reasonable to suppose that at one time or another, they must have constituted a part of some larger mass. But as this subject will be taken up again, I pass to another point—namely the crystalline structure; more especially that of the iron, and the complete separation in nodules, in the interior of the iron, of sulphuret and phosphuret of the metals constituting the mass. When this is properly examined, it is seen that these bodies must have been in a plastic state for a great length of time, for nothing else could have determined such crystallization as we see in the iron, and allow such perfect separation of sulphur and phosphorus from the great bulk of the metal, combining only with a limited portion to form particular minerals; and did we aim to imitate such separation by artificial processes, we could only hope to do it by retaining the iron in a plastic condition for a great length of time. Also, no other agent than fire can be conceived of by which this metal could be kept in the condition requisite for the separation.

If these facts with reference to the crystalline structure be admitted, the natural suggestion is that they could only have been thus heated while a part of some large body.

Another physical fact worthy of being noticed here, is the manner in which the metallic iron and stony parts are often interlaced and mixed, as in the Pallas and Atacama irons, where nickeliferous iron and olivine in nearly equal portions (by bulk)

are intimately mixed, so that when the olivine is detached the iron resembles a very coarse sponge. This is an additional fact in proof of the great heat to which the meteorites must have been submitted, for with our present knowledge of physical laws, there is no other way in which we can conceive that such a mixture of iron and olivine could have been produced.

Other physical points might be noticed, but as they are familiar to all, and would add nothing to the theoretical considerations, they will be passed over.

*Mineralogical and Chemical points to be noted in Meteorites.*—The rocks or minerals of meteorites are not of a sedimentary character, not such as are produced by the action of water. This is obvious to any one who will examine these bodies. A mineralogist will also be struck with the thin dark-colored coating on the surface of the stony meteorites. The coating, in most, if not in all, instances is of atmospheric origin, being acquired after the meteorite enters the atmosphere, and as such, no further notice will be taken of it; but I will proceed at once to notice the most interesting peculiarities under this head. First of all, metallic iron alloyed with more or less nickel and cobalt is of constant occurrence in meteorites,—with but three or four exceptions,—in some instances constituting the entire mass, at other times disseminated in fine particles through stony matter. The existence of this highly oxydizable mineral in its metallic condition is a positive indication of a scarcity, or total absence, of oxygen (in its gaseous state or in the form of water) in the locality from whence it came.

Another mineralogical character of significance is, that the stony portions of the meteorites resemble the older igneous rocks, and in even a more striking manner, the volcanic rocks belonging to various active and extinct volcanoes. It is useless to dwell on this fact, as it is one well known to all mineralogists who may have examined this matter, and none have given more especial attention to it than Rammelsberg who in a paper published in 1849, details his examination of a great variety of lavas, and traced the perfect parallelism between them and stony meteorites. He showed that the Juvenas stone has the same constitution as the Thjorsa lava of Heckla, both consisting substantially of augite and anorthite, even in nearly the same relative proportions; while the Chateau Renard and Nordhausen stones, have labradorite replacing the anorthite; and the Blansko, Chantonmay and Utrecht stones have oligoclase as the feldspar, and resemble the lavas of *Ætna*, *Stromboli* and the newer lavas of Heckla.

The inference to be drawn from the last character is very evident, it is highly significative of the igneous origin of these bodies, and of an igneous action similar to that now existing in our volcanoes.



Yet another point of resemblance to certain of our terrestrial igneous rocks is the presence of metallic iron, for lately Mr. Andrews has proved the existence of metallic iron in basaltic rocks, but this will not be insisted on, as the quantity of iron discovered in basaltic rocks is so minute as only to be detected by the most delicate means of investigation.

Ever since the labors of Howard in 1802, the chemical constitution of meteorites have attracted much attention, more especially the elements associated in the metallic portion, and although we find no new elements, still their association, so far as yet known, is peculiar to this class of bodies. Thus nickel is a constant associate of iron in meteorites, (if we except the Walke Co., Ala., and Oswego, N. Y., meteorites upon whose claims to meteoric origin there yet remains some doubt); and although cobalt and copper are mentioned only as occasional associates in my examination of near thirty known meteorites (in more than one-half of which these constituents were not mentioned), I have found both of the last mentioned metals as constantly as the nickel. With our more recent method of separating cobalt from nickel, very accurate and precise results can be obtained as relates to the cobalt; the copper exists always in so minute proportion that the most careful manipulation is required to separate it.

Another element frequently, but not always, mentioned as associated with the iron, is phosphorus. Here again my testing of thirty specimens lead me to a similar generalization concerning phosphorus, namely, that no meteoric iron is to be expected without it; my examination has extended as well to the metallic particles separated from the stony meteorites as to the meteoric irons proper. It may be even further stated that, in most instances, the phosphorus was traceable directly to the mineral Schreibersite.

These four elements then, Iron, Nickel, Cobalt and Phosphorus, I consider remarkably constant ingredients. First in the meteoric irons proper, and secondly in the metallic particles of the stony meteorites; there being only some three or four meteorites among hundreds that are known, in which they are not recognized.

As regards the combination of these elements, it is worthy of remark that no one of them is associated with oxygen, although all four of them have strong affinity for this element, and are never found (except copper) in the earth uncombined with it, except where some similar element (as sulphur, &c.) supplies its place.\*

The inference of the absence of oxygen in a gaseous condition, or in water, is drawn from such substances as iron and nickel being in their metallic state, as has been just mentioned:

\* The traces of iron found in basaltic rock already alluded to, forms too insignificant an exception to be insisted on.—J. L. S.

but it must not be inferred that oxygen is absent in all forms at the place of origin of the meteorites; for the silica, magnesia, protoxyd of iron, &c., contain this element. The occurrence of one class of oxyds and not another would indicate a limited supply of the element oxygen, the more oxydizable elements as silicon, magnesium, &c., having appropriated it in preference to the iron.

Many other elements worthy of notice might be mentioned here, and some of them for aught we know may be constant ingredients, but in the absence of strong presumption at least on this head, they will be passed over, as those already mentioned suffice for the support of all theoretical views to be advanced.

I cannot, however, avoid calling attention to the presence of *carbon* in certain meteorites, for although its existence is denied by some chemists, it is nevertheless a fact that can be as easily established as the presence of the nickel. The interest to be attached to it, is due to the fact that it is so commonly regarded in the light of an organic element. It serves to strengthen the notion that carbon can be of pure mineral origin, for no one would be likely to suppose that the carbon found its way into a meteorite either directly or indirectly from an organic source.

Having thus noted the predominant physical, mineralogical and chemical characteristics of meteorites I pass on to the next head.

*Marked points of similarity in the Constitution of Meteoric Stones.*—Had this class of bodies not possessed certain properties distinguishing them from terrestrial minerals, much doubt would even now be entertained of their celestial origin, and various would be the explanations made even in those cases where the bodies were seen to fall and afterwards collected. Chemistry has entirely dissipated all doubts in the matter, and now, an examination in the laboratory of the chemist is entitled to more credit than evidence from any other source in pronouncing on the meteoric origin of a body. No question need be asked as to whether it was seen to fall, or whether this or that rock or mineral exists in the neighborhood where it may have been collected. The reagents of the chemist alone are unerring indications that suffice to set aside all caviling in the matter.

It is the object of this part of the paper to explain more prominently perhaps than has yet been done, how it is that chemistry pronounces with such unerring certainty on the celestial origin of certain bodies; and I propose to go even a step farther, and see if the chemical constitution of the meteorites can indicate from what part of the heavens they may have come.

When the mineralogical and chemical composition of these bodies are regarded, the most ordinary observer will be struck

with the wonderful family likeness running through them all, however unlike at first sight. There will be seen to be three great divisions of meteoric bodies (omitting three or four), namely—metallic; stony with small particles of metal; and a mixture of metallic and stony in which the former predominates, as in the Pallas and Atacama meteorites.

As regards external appearances, these three classes differ in a very marked manner from each other. The *meteoric iron* being ordinarily of a compact structure, more or less corroded externally, and when cut showing a dense structure with most of the peculiarities of pure iron, only a little harder in texture and whiter in color. The *stony meteorites* are usually of a grey or greenish grey color, granular structure, readily broken by a blow of the hammer, and exteriorly are covered with a thin coating of fused material. The *mixed meteorite* presents characters of both of the above; a large portion of it is constituted of the kind of iron already mentioned, cellular in its character, and the cells filled up with stony materials, similar in appearance to those constituting the second class.

Although there are some instances of bodies of undoubted meteoric origin not properly falling under either of the above three heads, still they will be seen upon close investigation not to interfere in any way with the general conclusions that are attempted to be arrived at; for these constituents are represented in the stony materials of the second class from which their only essential difference consists in the absence of metallic particles.

If we now examine chemically the three classes mentioned, we find them all possessed of certain common characteristics that link them together and at the same time separate them from every thing terrestrial. Take first the metallic masses: and in very many instances, in some fissure or cavity, exposed by sawing or otherwise, stony materials will frequently be found, and a stony crystal is sometimes exposed; now examine the composition of these, and then compare the results with what may be known of the stony meteorites, and in every instance, it will agree with some mineral or minerals found in this latter class, as olivine or pyroxene, most commonly the former; but in no instance is it a mineral not found in the stony meteorites. If these last, in their turn, be examined, differing vastly in their appearance from the metallic meteorites, they will with but two or three exceptions be found to contain a malleable metal identical in composition with the metal constituting the metallic meteorites.

As to those mixed meteorites in which the metallic and stony portions seem to be equally distributed; their two elements are but representatives of the two classes just described. Examined in this way there will be no difficulty in tracing the same signa-



ture on them all, endorsing the above as their true character, and almost serving to tell us whence they came. They may emphatically be said to have been linked in their origin by a chain of iron.

There is one mineral which there is every reason to believe constantly accompanies the metallic portions, and which may be regarded as a most peculiar mark of difference between meteorites and terrestrial bodies. It is the mineral *Schreibersite* (see first part of this memoir) to which the constant presence of phosphorus in meteoric iron is due. This mineral as already remarked has no parallel on the face of the globe, whether we consider its specific or generic character, there being no such thing as phosphuret of iron and nickel or any other phosphuret found among minerals. These facts render the consideration of *Schreibersite* one of much interest, running as it probably does through all meteorites, and forming another point of separation between meteorites and terrestrial objects.

Another striking similarity in the composition of meteorites is the limited action of oxygen on them. In the case of the purely metallic meteorites we trace an almost total absence of this element. In the stony meteorites, the oxygen is in combination with silicon, magnesium, &c., forming silica, magnesia, &c., that combine with small portions of other substances to form the predominant earthy minerals of meteorites. When iron is found in combination with oxygen, it is found in its lowest state of oxydation as in the protoxyd of the olivine and chrome iron, and as magnetic oxyd.

Without going further into detail as regards the similarity of composition of meteorites, they will be seen to have as strongly marked points of resemblance as minerals coming from the same mountain, I might almost say from the same mine, and it is not asking much to admit their having a *common centre of origin* and that whatever the body from which they originate, it must contain no uncombined oxygen and I might even add none in the form of water.

What is this centre of origin? Physics does not point it out, and although the chemist cannot explore the elementary constitution of any other great celestial bodies than the earth, he can examine those smaller celestial masses which come to the earth and from his results stand on a firmer basis for theoretical conclusions.

*Origin of Meteoric Stones.*—In taking up the theoretical considerations of the origin of meteoric stones, it is of the utmost consequence, to reflect well before we confound shooting stars and meteoric stones as all belonging to the same class of bodies; a view entertained by many distinguished observers. It is doubtless owing to the fact of their having been confounded that but

little advance has been made in settling upon the origin of these bodies; in fact, owing to this manner of viewing the subject, observers such as Arago, Bissel, Olbers and others have turned away from the original conception of the origin of meteoric stones to views of a different character based on observations of the shooting stars.

It may be a broad assumption to start with, that there is not a single evidence of the identity of shooting stars (as exemplified by the periodical meteors of August and November) and these meteors which give rise to meteoric stones, and this conclusion is one arrived at by as full an examination of the subject as I am capable of making.\* Some of the prominent reasons for such a conclusion will be mentioned.

Were shooting stars and meteoric stones the same class of bodies, it is natural to suppose that the fall of the latter would be most abundant when the former are most numerous. In other words these periodic occurrences of shooting stars in August and November and more particularly those immense showers that have been sometimes seen, ought to have been attended with the falling of one or more meteoric stones; whereas there is not a single instance on record where these showers have been accompanied with the falling of a meteoric stone. Again, in all instances where a meteoric body has been seen to fall and has been observed even from its very commencement, it has been alone and not accompanied by other meteors. Very little reflection will serve to convince any one that an objection to the identity of the two classes of bodies based upon the above fact is of great weight.

Another strong objection to considering the bodies of the same nature, is based on the want of proof of their velocities being the same. It is a pretty well established fact that the average velocity of shooting stars is  $16\frac{1}{2}$  miles a second, a result arrived at by different observers, and doubtless a close approximation to the truth, as from the constant occurrence of shooting stars, thousands of observations may be made with comparative ease by different observers noting the same stars: not so with meteoric stones, these occurrences being rare, sudden and unexpected, and no two observers being ever prepared to note the data requisite for calculating their velocities; besides I am prepared to prove that the two or three cases of supposed determination of velocities of meteoric stones cannot be considered even gross approximation to the truth: in fact the difficulties in the way are so great that we probably never shall arrive at a knowledge of their

\* Prof. D. Olmsted in a most interesting article on the subject of meteors, to be found in the 26th volume of the *Am. Journal of Science*, p. 132, insists upon the difference between shooting stars and meteorites, and the time and attention he has devoted to the phenomena of meteors give weight to his opinion..

velocities.\* Not even their effect on striking the earth, will furnish any data whereby to calculate their velocities before entering the atmosphere, for this medium must offer such enormous resistance to bodies penetrating at great velocities, that these velocities must be reduced to but a fraction of what they originally were, and it is a question whether a body entering our atmosphere at ten miles a second would penetrate the soil to a much greater depth than one entering it at five miles a second, for the increased velocity of the former would cause an increased resistance in the atmosphere and therefore have received proportionally a greater check before striking the earth.

Another fact tending to prove a dissimilarity between shooting stars and meteoric stones, is that the velocity of no one of the shooting stars has been observed to be so low as to allow of their being considered satellites to the earth; their average velocity is  $16\frac{1}{2}$  miles a second and it requires a reduction to less than six miles a second for them to assume a path around the earth. Now, assume what we may as to the original orbit of the meteoric stones, and as to their original velocity—let their orbit be around the sun and their velocity 16 miles a second—there is one thing we know, namely that these bodies do enter our atmosphere, and it is but right to assume, often pass through the atmosphere without falling to the earth, sometimes passing through the very uppermost portion of that medium, at other times lower. What becomes of their original assumed velocity after this passage? As it can be so checked as to be drawn to the earth's surface, and thus stopped altogether in its passage, their velocities may be changed to any velocity from 16 miles a second to zero, according to the amount of resistance it meets with; and what is equally true in this connection, is, that when the velocity falls below six miles a second (or thereabouts) they can no longer escape from the attraction of the earth and resume their solar orbit, but must revolve as a satellite around the earth until ultimately brought to its surface by repeated disturbances.

The deduction from the above fact, is as follows: that as the most correct observations have never given a velocity of less than

\* Under this head I will merely note what is considered one of the best established cases of the determination of velocity of a meteoric stone—namely that of the Weston meteorite the velocity of which Dr. Bowditch estimated to “*exceed three miles a second.*” Mr. Herrick considers the velocity very much greater, and writes among other things what follows. “The length of its path from the observations made at Rutland, Vt., and at Weston was at least 107 miles. This space being divided by the duration of the flight as estimated by two observers, viz., 30 seconds, we have for the meteor's relative velocity about *three and a half miles a second.* The observations made at Wenham, Mass., are probably less exact in this respect and need not be mentioned here. An experienced observer, however intelligent, will give the time ten or even twenty fold too large. One not unversed in science who saw the meteor is confident it could not have been in sight as long as ten seconds.” The above is given as a specimen of the uncertain data we are to proceed upon in estimating the velocity of meteoric stones.



nine miles a second to a shooting star, it is reasonable to suppose that none have ever entered our atmosphere, or what is perhaps still more reasonable, that the matter of which they are composed is as subtle as that of Encke's comet, and any contact with even the uppermost limit of the atmosphere, destroys their velocity and disperses the matter of which they are composed. Other grounds might be mentioned for supposing a difference between shooting stars and meteoric stones, and I have dwelt on it thus much because it is conceived of prime importance in pursuing the correct path that is to lead to the discovery (if it can be made) of their origin. It is also of no small value to the beautiful and probable theory of shooting stars that we should separate every thing from it that may tend to affect its plausibility.

Various theories have been devised to account for their origin. One is that they are small planetary bodies revolving around the sun, and that at times they become entangled in our atmosphere lose their orbital velocity by the resistance of the atmosphere and are finally attracted to the earth. They are also supposed to have been ejected from the volcanoes of the moon: and lastly they are considered as formed from particles floating in the atmosphere. The exact nature of this last theory, is understood by reading the views of Prof. C. U. Shepard, as expressed in an interesting report on meteorites published in 1848. The author\* says—"The extra-terrestrial origin of meteoric stones and iron masses, seems likely to be more and more called in question with the advance of knowledge respecting such substances and as additions continue to be made to the connected sciences. Great electrical excitation is known to accompany volcanic eruptions, which may reasonably be supposed to occasion some chemical changes in the volcanic ashes ejected; these being wafted by the ascensional force of the eruption into the regions of the magnetopolar influence, may there undergo a species of magnetic analysis. The most highly magnetic elements, (iron, nickel, cobalt, chromium, &c.,) or compounds in which these predominate, would thereby be separated, and become suspended in the form of metallic dust, forming those columnar clouds so often illuminated in auroral displays, and whose position conforms to the direction of the dipping needle. While certain of the diamagnetic elements, (or combinations of them,) on the other hand, may under the control of the same force be collected into different masses, taking up a position at right angles to the former, (which Faraday has shown to be the fact in respect to such bodies,) and thus produce those more or less regular arches, transverse to the magnetic meridian, that are often recognized in the phenomena of the aurora borealis.

\* I must in justice to Prof. Shepard say that since his paper was written he has informed me that he no longer entertains these views; and I would now omit the criticism of them did they not exist in his memoir uncontradicted and also were they not views still entertained by some.

"Any great disturbance of the forces maintaining these clouds of meteor-dust, like that produced by a magnetic storm, might lead to the precipitation of portions of the matter thus suspended. If the disturbance was confined to the magnetic dust, iron masses would fall; if to the diamagnetic dust, a non-ferruginous stone; if it should extend to both classes simultaneously, a blending of the two characters would ensue in the precipitate, and a rain of ordinary meteoric stones would take place.

"The occasional raining of meteorites might therefore on such a theory, be as much expected, as the ordinary deposition of moisture from the atmosphere. The former would originate in a mechanical elevation of volcanic ashes and in matter swept into the air by tornadoes, the latter from simple evaporation. In the one case, the matter is upheld by magneto-electric force; in the other, by the law of diffusion which regulates the blending of vapors and gases, and by temperature. A precipitation of metallic and earthy matter would happen on any reduction of the magnetic tension; one of rain, hail or snow, on a fall of temperature. The materials of both originate in our earth. In the one instance they are elevated but to a short distance from its surface, while in the other, they appear to penetrate beyond its farthest limits, and possibly to enter the inter-planetary space; in both cases, however, they are destined, through the operation of invariable laws, to return to their original repository."

This theory, coming as it does from one who is justly entitled to high consideration, from the fact of the special attention he has given to the subject of meteorites, may mislead, and for that reason objections will be advanced which will doubtless entirely set aside this notion of terrestrial origin, and to this end I would consider two fundamental principles of it. First of all it must be proved that terrestrial volcanoes contain all the varieties of matter found in the composition of meteoric bodies; there is no doubt that many of the varieties are ejected from volcanoes, as olivine, &c., but then the principal one, nickeliferous iron has never in a single instance been found in the lava or other matter coming from volcanoes although frequently sought for.

But the physical obstacles are a still more insuperable difficulty in the way of adopting this theory. In the first place it is considered a physical impossibility for tornadoes or other currents of air to waft matter, however impalpable, "beyond the farthest limits of the earth and possibly into interplanetary space." Again if magnetic and diamagnetic forces cause the particles to coalesce and form solid masses, by the cessation of those forces the bodies would crumble into powder. Another strong physical objection to the theory is, that as the consolidation of these masses is expected to take place in "magneto-polar regions" their fall should only be in those portions of the earth, for like rain and hail (to

which the consolidation of these bodies are assimilated in this theory) they should fall perpendicularly or nearly so, from their points of condensation. And lastly (under the head of physical objections) how can bodies so formed be precipitated in such very oblique directions as many are known to have, and that too from East to West and not from the North.

We pass on to a concise statement of some of the chemical objections to this theory of atmospheric origin, and if possible, they are more insuperable than the last mentioned. Contemplate for a moment the first meteorite described in this paper;—here is a mass of iron of about sixty pounds of a most solid structure, highly crystalline, composed of nickel and iron chemically united, containing in its centre a crystalline phosphuret of iron and nickel, and on its exterior surface a compound of sulphur and iron also in atomic proportions, and then see if the mind can be satisfied in supposing that the dust wafted from the crater of a volcano into the higher regions of the atmosphere, could *in a few moments of time* be brought together by any known forces so as to create the body in question. However finely divided this volcanic dust might be, it can never be subdivided into atoms, a state of things that must exist to form bodies in atomic proportions, where no agency is present to dissolve or fuse the particles concerned. One other objection and I am done with this theory.

The particles of iron and nickel supposed to be ejected from the volcano, must pass from the heated mouth of a crater ascend through the oxygen of the atmosphere without undergoing the slightest oxydation, for if there be any one thing which marks the meteorites more strongly than any other it is the freedom of the masses of iron from oxydation except on the surface. But a still more remarkable abstinence from oxydation would be the ascent of the particles of phosphorus to form the Schreibersite traceable in so many meteorites.

Having noticed the prominent objections to this theory I pass on to consider in as few words as possible the other two theories.

A very commonly adopted theory of the origin of meteoric bodies, is that they are small planetary bodies revolving around the sun, one portion of their orbit approaching or crossing that of the earth, and from the various disturbing causes to which these small bodies must necessarily be subjected, their orbits are constantly undergoing more or less variation, until intersected by our atmosphere when they meet with the most serious derangement and fall to the earth's surface in whole or in part; this may not occur in their first passage through the atmosphere, but repeated obstructions in this medium at different times must ultimately bring about the result. In this theory their origin is supposed to be the same as that of other planetary bodies, and they are regarded as always having had an individual cosmical existence. Now, how-



ever reasonable the admission of this orbital motion immediately before and for some time previous to their contact with the earth, the assumption of their original cosmical origin would appear to have no support in the many characteristics of meteoric bodies as enumerated some pages back. The form alone of these bodies is any thing but what ought to be expected from a gradual condensation and consolidation; all the chemical and mineralogical characters are opposed to this supposition. If the advocates of this theory do not insist on the last feature of it, then the theory amounts to but little else than a statement that meteoric stones fall to us from space while having an orbital motion. In order to entitle this planetary theory to any weight it must be shown, how, bodies formed and constructed as these are, could be other than fragments of some very much larger mass.

As to the existence of meteoric stones in space, travelling in a special orbit prior to their fall, there can be but little doubt when we consider their direction and velocity; their composition proving them to be of extra-terrestrial origin. This, however, only conducts in part to their origin, and those who will examine them chemically will feel convinced that the earth is not the first great mass that meteoric stones have been in contact with, and this conviction is strengthened when we reflect on the strong marks of community of origin so fully dwelt upon.

It is then in consideration of what was the connection of these bodies prior to their having an independent motion of their own that this memoir will be concluded.

*Lunar Origin of Meteoric Stones.*—It only remains to bring forward the facts already developed, to prove the plausibility of this origin of meteorites.

It is a theory that was proposed as early as 1660 by an Italian philosopher, Terzago, and advanced by Olbers in 1795, without any knowledge of its having been before proposed; it was sustained by Laplace with all his mathematical skill from the time of its adoption to his death; it was also advocated on chemical grounds by Berzelius, whom I have no reason to believe ever changed his views in this matter, and to these we have to add the following distinguished mathematicians and philosophers: Biot, Brandes, Poisson, Quetelet, Arago and Benzenberg who have at one time or another advocated the Lunar origin of meteorites.

Some of the above astronomers abandoned the theory, among them Olbers and Arago, but they did not do so, from any supposed defect in it, but from adopting the assumption that shooting stars and meteorites were the same; and on studying the former and applying the phenomena attendant upon them to meteorites, the supposed lunar origin was no longer possible.

On referring to the able researches of Sears C. Walker on the periodical meteors of August and November (Trans. Am. Phil.

Soc., Jan., 1841), that astronomer makes the following remarks about Olbers's change of views. "In 1836, Olbers, the original proposer of the theory of 1795, being firmly convinced of the correctness of Brandes's estimate of the relative velocity of meteors, renounces his *selenic* theory, and adopts the *cosmical* theory as the only one which is adequate to explain the established facts before the public."

For reasons already stated, it appears wrong to assume the identity of meteorites and shooting stars, so that whatever difficulty the phenomena of shooting stars may have interposed in conceiving this or that to have been the origin of meteoric stones, it now no longer exists, and we are fully authorized in forming our conclusions concerning them to the utter disregard of the phenomena of shooting stars. Had Olbers viewed the matter in this light, he would doubtless have retained his original convictions, to which no material objection appears to have occurred to him for forty years.

It is not my object to enter upon all the points of plausibility of this assumed origin, or to meet all the objections which have been urged to it; for most of them have already been ably treated of. The object now, is simply to urge such points as have been developed in this memoir, that appear to give strength to the lunar theory; they may be summed up under the following heads:

- 1st. That all meteoric masses have a community of origin.
- 2nd. At one period they formed parts of some large body.
- 3d. They have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes.
- 4th. That their source must be deficient in oxygen.
- 5th. That their average specific gravity is about that of the moon.

From what has been said under the head of common characters of meteorites, it would appear far more singular that these bodies should have been formed separately from each other than that they should have at one time or another constituted parts of the same body; and from the character of their formation, that body should have been of great dimensions. Let us suppose all the known meteorites assembled in one mass, and regarded by the philosopher, mindful of our knowledge of chemical and physical laws. Would it be considered more rational to view them as the great representatives of some one body that had been broken into fragments, or as small specks of some vast body in space that at one period or another has cast them forth? The latter it seems to me is the only opinion that can be entertained in reviewing the facts of the case.

As regards the igneous character of the minerals composing meteorites, nothing remains to be added to what has already

been said ; in fact no mineralogist can dispute the great resemblance of these minerals to those of terrestrial volcanoes, they having only sufficient difference in association, to establish that although igneous they are extra-terrestrial. The source must also be deficient in oxygen either in a gaseous condition or combined as in water : the reasons for so thinking have been clearly stated as dependent upon the existence of *metallic iron* in meteorites ; a metal so oxydizable, that in its terrestrial associations it is almost always found combined with oxygen and never in its metallic state.

What then is that body which is to claim common parentage of these celestial messengers that visit us from time to time ? Are we to look at them as fragments of some shattered planet whose great representatives are the thirty-three asteroids between Mars and Jupiter and that they are " minute outriders of the asteroids " (to use the language of R. P. Greg, Jr., in a late communication to the British Association), which have been ultimately drawn from their path by the attraction of the earth ? For more reasons than one this view is not tenable ; many of our most distinguished astronomers do not regard the asteroids as fragments of a shattered planet, and it is hard to believe if they were, and the meteorites the smaller fragments, that these latter should resemble each other so closely in their composition ; a circumstance that would not be realized if our earth was shattered into a million of masses large and small.

If then we leave the asteroids and look to the other planets we find nothing in their constitution, or the circumstances attending them, to lead to any rational supposition as to their being the original habitation of the class of bodies in question. This leaves us then but the *moon* to look to as the parent of meteorites, and the more I contemplate that body, the stronger does the conviction grow, that to it all these bodies originally belonged.

It cannot be doubted from what we know of the moon that it is in all likelihood constituted of such matter as compose meteoric stones ; and that its appearances indicate volcanic action, which when compared with the combined volcanic action on the face of the globe, is like contrasting *Ætna* with an ordinary forge, so great is the difference. The results of volcanic throws and outbursts of lava are seen, for which we seek in vain any thing but a faint picture on the surface of our earth. Again in the support of the present view it is clearly established that there is neither atmosphere nor water on the surface of that body, and consequently no oxygen in those conditions which would preclude the existence of metallic iron.

Another ground in support of this view is based on the specific gravity of meteorites, a circumstance that has not been insisted on, and although of itself possessing no great value, yet in conjunction with the other facts it has some weight.



In viewing the cosmical bodies of our system with relation to their densities, they are divided into two great classes—planetary and cometary bodies (these last embracing comets proper and shooting stars), the former being of dense, and the latter of very attenuated matter; and so far as our knowledge extends, there is no reason to believe that the density of any comet approaches that of any of the planets: this fact gives some grounds for connecting meteorites with the planets. Among the planets there is also a difference, and a very marked one, in their respective densities; Saturn having a density of 0.77 to 0.75, water being 1.0; Jupiter 2.00–2.25; Mars 3.5–4.1; Venus 4.8–5.4; Mercury between 7 and 36; Uranus 0.8–2.9; that of the Earth being 5.67.\* If then from specific gravity we are to connect meteorites to the planets, as their mean density is usually considered about 3.0,† they must come within the planetary range of Mars, Earth and Venus. In the cases of the first and last we can trace no connection, from our ignorance of their nature and of the causes that could have detached them.

This reduces us then to our own planet consisting of two parts, the planet proper with a density of 5.76, and the moon with a density of about 3.62.‡ On viewing this, we are at once struck with the relation that these bear to the density of meteorites, a relation that even the planets do not bear to each other.

As before remarked, I lay no great weight on this view of the density, but call attention to it as agreeing with conclusions arrived at on other grounds.

The chemical composition is also another strong ground in favor of their lunar origin. This has been so ably insisted on by Berzelius and others that it would be superfluous to attempt to argue the matter any further here; but I will simply make a comment on the disregard that astronomers usually have for this argument. In the memoir on the periodic meteors by Sears C. Walker, already quoted from, it is stated, "The chemical objection is not very weighty, for we may as well suppose a uniformity of constituents in cosmical as in lunar substances." From this conclusion it is reasonable to dissent, for as yet we are acquainted with the materials of but two bodies, those of the earth and those of meteorites, and their very dissimilarity of constitution is the strongest argument of their belonging to different

\* For these estimates of the densities of the Planets, the author is indebted to Prof. Peirce.

† Although the average specific gravity of the metallic and stony meteorites is greater, yet the latter exceeding the former in quantity, the number 3.0 is doubtless as nearly correct as can be ascertained.

‡ Although the densities of the earth and moon differ, these two bodies may consist of similar materials, for the numbers given represent the density of bodies as wholes; the solid crust of the earth for a mile in depth cannot average a density of 3.0.

spheres. In further refutation of this idea it may be asked, Is it to be expected that a mass of matter detached from Jupiter (a planet but little heavier than water) or from Saturn (one nearly as light as cork) or from Encke's comet (thinner than air), would at all accord with each other or with those of the earth. It is far more rational to suppose that every cosmical body, without necessarily possessing elements different from each other, yet are so constituted that they may be known by their fragments. With this view of the matter, our specimens of meteorites are but multiplied samples of the same body, and that body, with the light we now have, appears to have been the moon.

This theory is not usually opposed on the ground that the moon is not able to supply such bodies as the meteoric iron and stone; it is more commonly objected to from the difficulty that there appears to be in the way of this body's projecting masses of matter beyond the central point of attraction between the earth and moon. Suffice it to say, that Laplace, with all his mathematical acumen, saw no difficulty in the way of this taking place, although we know, that he gave special attention to it at three different times during a period of thirty years, and died without discovering any physical difficulty in the way. Also for a period of forty years, Olbers was of the same opinion, and changed his views as already stated for reasons of a different character: and to these two we add Hutton, Biot, Poisson and others whose names have been already mentioned.

Laplace's view of the matter was connected with present volcanic action in the moon, but there is every reason to believe that all such action has long since ceased in the moon. This, however, does not invalidate this theory in the least, for the force of projection and modified attraction to which the detached masses were subjected, only gave them new and independent orbits around the earth, that may endure for a great length of time before coming in contact with the earth.

The various astronomers cited concur in the opinion, that a body projected from the moon with a velocity of about eight thousand feet per second would go beyond the mutual point of attraction between the earth and moon, and already having an orbital velocity may become a satellite of the earth with a modified orbit.

The important question then for consideration is, the force requisite to produce this velocity. The force exercised in terrestrial volcanoes varies. According to Dr. Peters, who made observations on *Ætna*, the velocity of some of the stones was 1250 feet a second, and observations made on the peak of *Teneriffe* gave 3000 feet a second. Assuming, however, the former velocity to be the maximum of terrestrial volcanic effects, the velocity with

which the bodies started (stones with a specific gravity of about 3.00) must have exceeded 2000 feet a second to permit of an absorbed velocity of 1250 feet through the denser portions of our atmosphere. Now suppose the force of the extinct volcanoes of the moon to have equalled that of *Ætna*, the force would have been more than sufficient to have projected masses of matter at a velocity exceeding 8000 feet a second; for, the resistance to be overcome by the projectile force, is the attractive force of the moon, which is from 5 to 6 times less than that of the earth, so that the same projectile force in the two bodies would produce vastly greater velocities on the moon than on the earth, discarding of course atmospheric resistance of which there is none in the moon.\*

But doubtless, were the truth of the matter known, the projectile force of lunar volcanoes far exceeded that of any terrestrial volcanoes extinct or recent, and this we infer from the enormous craters of elevation to be seen upon its surface, and their great elevation above the general surface of the moon, with their borders thousands of feet above their centre; all of which, point to the immense internal force required to elevate the melted lava that must have at one time poured from their sides. I know that Prof. Dana in a learned paper on the subject of lunar volcanoes (*Am. J. Sci.*, [2], ii, 375, argues that the great breadth of the craters is no evidence of great projectile force, the pits being regarded as boiling craters where force for lofty projection could not accumulate. Although his hypothesis is ingeniously sustained, still, until stronger proof is urged, we are justified I think in assuming the contrary to be true, for we must not measure the convulsive throes of nature at all periods by what our limited experience has enabled us to witness.

As regards the existence of volcanic action in the moon without air or water, I have nothing at present to do, particularly as those who have studied volcanic action concede that neither of these agents is absolutely required to produce it; moreover, the surface of the moon is the strongest evidence we have in favor of its occurring under those circumstances.

But it may be very reasonably asked, Why consider the moon the source of these fragmentary masses called meteorites? May not smaller bodies, either planets or satellites, as they pass by the earth and through our atmosphere, have portions detached by the mechanical and chemical action to which they are subjected? To this, I will assent, as soon as the existence of that body or those bodies is proved. Are we to suppose that each meteorite falling to the earth is thrown off from a different sphere

\* It would require at the moon the same force to produce an *initial* velocity of 8000 feet a second as at the earth; and the difference of rate at the end of the first second would be slight (discarding from consideration the atmosphere).—Ers.



which becomes entangled in the atmosphere? If so, how great the wonder that the earth has never intercepted one of those spheres, and that all should have struck the stratum of air surrounding our globe (some fifty miles in height) and escaped the body of the globe 8000 miles in diameter. It is said that the earth has never intercepted one of these spheres; for if we collect together all the known meteorites, in and out of cabinets, they would hardly cover the surface of a good sized room, and no one of them could be looked upon as the maternal mass upon which we might suppose the others to have been grafted; and this would appear equally true, if we consider the known meteorites as representing not more than a hundredth part of those which have fallen.

If it be conceived that the same body has given rise to them, and is still wending its path through space, only seeming by its repeated shocks with our atmosphere to acquire new vigor for a new encounter with that medium, the wonder will be greater, that it has not long since encountered the solid part of the globe; but still more strange, that its velocity has not been long since destroyed by the resistance of the atmosphere, through which, it must have made repeated crossings of over 1000 miles in extent.

But it may be said that facts are stronger than arguments, and that bodies of great dimensions (even over one mile in diameter) have been seen traversing the atmosphere, and have also been seen to project fragments and pass on. Now of the few instances of the supposed large bodies, I will only analyze the value of the data upon which the Wilton and Weston meteorites were calculated; and they are selected, because the details connected with them are more accessible. The calculations concerning the latter were made by Dr. Bowditch; but his able calculations were based on deceptive data,—and this is stated without hesitation knowing the difficulty admitted by all of making correct observation as to size of luminous bodies passing rapidly through the atmosphere. Experiments, that would be considered superfluous, have been instituted to prove the perfect fallacy of making any but a most erroneous estimate of the size of luminous bodies, by their apparent size, *even when their distance from the observer and the true size of the object are known*; how much more fallacious then, any estimate of size made, where the observer does not know the true size of the body, and not even his distance very accurately.

In my experiments, three solid bodies in a state of vigorous incandescence were used; 1st, charcoal points transmitting electricity; 2nd, lime heated by the oxy-hydrogen blowpipe; 3d, steel in a state of incandescence in a stream of oxygen gas. They were observed on a clear night at different distances, and the body of light (without the bordering rays) compared with the disk of the moon, then nearly full, and  $45^{\circ}$  above the horizon. With-

out going into details of the experiment the results will be tabulated,

	Actual diam. as seen at 10 in.	Apparent diam. at 200 yds.	Apparent diam. at $\frac{1}{4}$ mile.	Apparent diam. at $\frac{1}{2}$ mile.
Carbon points,	$\frac{3}{10}$ of an inch,	$\frac{1}{2}$ the diam. moon's disk,	3 diam. do.do.	$-3\frac{1}{2}$ diam. do.do
Lime light,	$\frac{4}{10}$ " "	$\frac{1}{3}$ " " " "	2 " " "	2 " " "
Incandes. steel,	$\frac{2}{10}$ " "	$\frac{1}{4}$ " " " "	1 " " "	1 " " "

If then the apparent diameter of a luminous meteor at a given distance is to be accepted as a guide for calculating the real size of these bodies the

Charcoal\* points would be 80 feet in diam. instead of  $\frac{3}{10}$  of an inch.

Lime " " 50 " " "  $\frac{4}{10}$  "

The steel globule " " 25 " " "  $\frac{2}{10}$  "

It is not in place to enter into any explanation of these deceptive appearances, for they are well known facts, and were tried in the present form only to give precision to the criticism on the supposed size of these bodies. Comments on them are also unnecessary, as they speak for themselves. But to return to the two meteorites under review.

That of Wilton was estimated by Mr. Edward C. Herrick, (Am. Journ. of Science, vol. xxxvii, p. 130) to be about 150 feet in diameter. It appeared to increase gradually in size until *just before the explosion*, when it was at its largest apparent magnitude of  $\frac{1}{4}$ th the moon's disk—exploded  $25^{\circ}$  to  $30^{\circ}$  above the horizon with a heavy report, that was heard about 30 seconds after the explosion was seen. One or more of the observers saw luminous fragments descend toward the ground. When it exploded, it was three or four miles above the surface of the earth; immediately after the explosion, it was no longer visible. The large size of the body is made out of the fact of its appearing one-fourth the apparent disk of the moon at about six miles distant. After the experiments just recorded, and easy of repetition, the uncertainty of such a conclusion must be evident; and it is insisted on as a fact easy of demonstration, that a body in a state of incandescence, (as the ferruginous portions of a stony meteorite,) might exhibit the apparent diameter of the Wilton meteorite at six miles distance, and not be more than a few inches or a foot or two in diameter according to the intensity of the incandescence.†

Besides, if that body was so large, where did it go to after throwing off the supposed small fragments? The fragments were seen to fall, but the great ignited mass suddenly disappeared, at  $30^{\circ}$  above the horizon, four miles from the earth, when it could

\* Estimate made according to a table given by Prof. Olmsted (Am. Journal of Science, vol. xxvi, p. 155) for estimating the diameter of meteors on comparison with the moon.

† It ought however to be stated, that in the paper above referred to, Mr. Herrick expressly mentioned this and other sources of fallacy, endeavored as far as practicable to guard against them, and gave his final careful result as necessarily open to some uncertainty.—Ebs.

not have had less than six or seven hundred miles of atmosphere to traverse, before it reached the limit of that medium; it has already acquired a state of ignition in its passage through the air prior to the explosion, and should have retained its luminous appearance consequent thereupon, at least while remaining in the atmosphere: but as this was not the case, and a sudden disappearance of the entire body took place in the very lowest portions of the atmosphere, and descending luminous fragments were seen, the natural conclusion appears to be, that the whole meteorite was contained in the fragments that fell.

As to the Weston meteorite, it is stated that its direction was nearly parallel to the surface of the earth at an elevation of about 18 miles; was one mile farther when it exploded; the length of its path from the time it was seen until it exploded was at least 107 miles; duration of flight estimated at about 30 seconds, and its relative velocity three and a half miles a second; it exploded; three heavy reports were heard; *the meteorite disappeared at the time of the explosion.*

As to the value of the data upon which its size was estimated, the same objection is urged as in the case of the Wilton meteorite; and it is hazarding nothing to state that the apparent size may have been due to an incandescient body a foot or two in diameter. Also, with reference to its disappearance, there is the same inexplicable mystery. It is supposed from its enormous size that but minute fragments of it fell; yet it disappeared at the time that this took place, which it is supposed occurred 19 miles above the earth, (an estimate doubtless too great when we consider the heavy reports). Accepting this elevation, what do we have? A body one mile and a half in diameter in a state of incandescence, passing in a curve almost parallel to the earth, and while in the very densest stratum of air that it reaches with a vigorous reaction between the atmosphere and its surface, and a dense body of air in front of it, is totally eclipsed; while, if it had a direction only tangential to the earth, instead of nearly parallel, it would at the height of 19 miles have had upwards of 500 miles of air of variable density to traverse, which at the relative velocity of  $3\frac{1}{2}$  miles a second (that must have been constantly diminishing by the resistance) would have taken about 143 seconds. It seems most probable that if this body was such an enormous one, that it should have been seen for more than ten minutes after the explosion, for the reasons above stated. The fact of its disappearance at the time of the explosion, is strong proof that the mass itself was broken to fragments, and that these fragments fell to the earth;—assuring us that the meteorite was not the huge body represented, but simply one of those irregular stony fragments which, by explosion from heat and great friction against the atmosphere, become shattered. I say irregular, because we have strong evidence of this irregularity in its motion, which was



“scalloping,” a motion frequently observed in meteorites, and doubtless due to the resistance of the atmosphere upon the irregular mass, for a spherical body passing through a resisting medium at great velocity would not show this. In fact, if almost any of the specimens of meteorites in our cabinets were discharged from a cannon, even in their limited flight the scalloping motion would be seen.

This then will conclude what I have to say in contradiction to the supposition of large solid cosmical bodies passing through the atmosphere, and dropping small portions of their mass. The contradiction is seen to be based ; first, upon the fact that no meteorite is known of any very great size, none larger than the granite balls to be found at the Dardanelles along side of the pieces of ordnance from which they are discharged ; secondly, on the fallacy of estimating the actual size of these bodies from their apparent size ; and lastly from its being opposed to all the laws of chance that these bodies should have been passing through an atmosphere for ages and none have yet encountered the body of the earth.

To sum up the theory of the lunar origin of meteorites, it may be stated—*That the moon is the only large body in space of which we have any knowledge, possessing the requisite conditions demanded by the physical and chemical properties of meteorites ; and that they have been thrown off from that body by volcanic action, (doubtless long since extinct,) and, encountering no gaseous medium of resistance, reached such a distance as that the moon exercised no longer a preponderating attraction—the detached fragment, possessing an orbital motion and an orbital velocity, which it had in common with all parts of the moon, but now more or less modified by the projectile force and new condition of attraction in which it was placed with reference to the earth, acquired an independent orbit more or less elliptical. This orbit, necessarily subject to great disturbing influences may sooner or later cross our atmosphere and be intercepted by the body of the globe.*

In concluding this lengthy examination, I must say that a discussion of the phenomena accompanying the falling of meteorites has been avoided, as well as many points connected with their history. This has been done from its having no immediate connection with the object of this memoir, which is intended simply to present to the Association some new views, and many old views in a new light, so as to awaken attention to the study of this most interesting class of bodies.

I take pleasure in acknowledging my obligations to the Secretary of the Smithsonian Institution, for the ready manner in which he placed the Laboratory of that Institution at my disposal for the purpose of making the analytical investigations contained in this memoir as well as other researches yet unpublished.



THE  
CHRISTIAN EXAMINER  
AND  
RELIGIOUS MISCELLANY.

JULY, 1853.

ART. I. — SPIRITUAL MECHANICS.\*

WE have printed below the titles of some books, not because we intend to review them in detail, but as suggesting the subject which we have in mind to discuss. They all agree in professing to give the rigid results of scientific observations, made in a province of research which falls partly under the jurisdiction of physics, and partly under that of physiology. The work of Reichenbach shows a candid and laborious purpose of its author to reduce to natural, though hitherto unregistered laws of matter, the fitful lights and the uncertain mirage of ani-

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\* 1. *Physico-Physiological Researches on the Dynamics of Magnetism, Electricity, Heat, Light, Crystallization, and Chemism, in their Relations to Vital Force.* By BARON CHARLES VON REICHENBACH. *The complete Work, from the German Second Edition, with the Addition of a Preface and Critical Notes,* by JOHN ASHBURNER, M. D. London. 1851.

2. *Untersuchungen über Thierische Electricität.* Von EMIL DU BOIS-REYMOND. Berlin. 1848.

3. *On Animal Electricity, being an Abstract of the Discoveries of Emil du Bois-Reymond,* made by DR. J. MÜLLER, Professor of Physics at Freiburg. Edited by H. BENGE JONES. London. 1852.

4. *Traité des Phénomènes Electro-physiologiques des Animaux,* par C. MATTEUCCI: *suiçi d'Études Anatomiques sur le System Nerveux et sur l'Organe Electrique de la Torpille,* par PAUL SAVI. Paris. 1844.

5. *Leçons sur les Phénomènes Physiques des Corps vivants.* Edition française publiée avec des Additions considérables sur 2<sup>e</sup> Edition italienne, par C. MATTEUCCI. Paris. 1847.



mal magnetism; and for this purity of intention it is to be treated with respect. When we examine this book, however, in detail, we do not find in it the same wise precautions against disturbing influences which characterize remarkably the investigations of Matteucci and Bois-Reymond.

Reichenbach thinks he has proved that *sensitive* persons see an objective light round the poles of a magnet, and at the ends of the axis of a crystal; that the magnet and the crystal affect the nerves and attract the human hand; that the *patient* can distinguish *magnetized* from *unmagnetized* water; that terrestrial magnetism disturbs the nerves; that the restless sleeper is least disturbed when he stretches himself out on the magnetic meridian, but tosses and dreams when his head and feet point to the east and west; that the power which the crystal and the magnet have of acting on those well disposed can be imparted to living men; that a similar influence is associated with solar radiations, chemical action, and electricity. This force, which always exists where magnetism is, but is not identical with magnetism, because it is found where magnetism is not found, is universal and potent, and deserves a name of its own. Reichenbach, therefore, for reasons as odd as the name itself, calls it "Od." There is an "od-negative" and an "od-positive." The left side of man is in *od*ic opposition to the right. This force is centralized in the hands and feet, especially in the hands. The mouth, with the tongue, is od-negative. "We have arrived," says Reichenbach, "at a not uninteresting explanation of a hitherto obscure matter,—the import of the kiss. The lips form one of the foci of the biod, and the flames which our poets describe do actually blaze there. This will be clearly elucidated in the next treatise. It may be asked how this can agree with the circumstance that the mouth is od-negative. This, however, does harmonize very well with the fact; for the kiss gives nothing, it desires and strives merely, it sucks and sips, and while it revels, longing and desire increase. The kiss is therefore not a negation, but a physical and moral negativity." (p. 257.)

It is remarkable, that whenever the *patients* and the *sensitive* were the *subjects* of experiment, there was a positive result. But when they were called on to *act*

and not to *feel*, the experiment generally failed. For Reichenbach frankly confesses that they could not lift iron filings by the odic attraction of their fingers, or deflect the galvanometer, or magnetize needles.

There certainly is no subject, connected with religion, philosophy, science, or the practical concerns of life, which at the present time occupies and disturbs more minds, in this and neighboring communities, than certain alleged phenomena, the reality of which many altogether deny; but which others, forced, as they think, to admit by the overwhelming evidence of their senses, either attempt to resolve into the operation of familiar, physical laws, or regard as spiritual manifestations of beings, once indeed moving like themselves upon the earth, but now removed beyond the ordinary reach of the human senses. The discovery, or at any rate the new application, of a fresh motive power by Ericsson, with its alleged economy and efficiency, and the consequent influence which is predicted for it on the useful arts and the grand march of civilization, meteoric as was the flash with which it first burst upon the public gaze, and all-important as it would become if its ample claims were justified, has produced a feeble and evanescent impression, compared with the more dubious phenomena just mentioned. The strong hold which a motley collection of hastily assumed or imperfectly investigated facts, known under the incongruous names of "spiritual manifestations," "table-movings," "rappings," "knockings," and other *aliases*, has taken of the more impressible portion of the community, may be explained to a certain degree by the novelty of the subject, and by an impertinent human curiosity which is ever ready to gnaw at any apple of forbidden knowledge; but at the same time it proclaims the violent reaction which the imaginative, the superstitious, the religious element of man's nature, is striving to effect against the dead weight of materialism and utilitarianism by which it is so heavily pressed down in this age and country.

In more than one city, town, and village, these phenomena, these experiments, and the agencies they are thought to engage, have been for months the engrossing subject of conversation, the sole relaxation for a body and mind wearied with life's toils, and in some cases the

only consolation for a spirit oppressed with the troubles and sorrows of life. Many repair daily to these exhibitions as a necessary excitement, and few can be so happily retired as wholly to escape them. They are the acknowledged attraction of many a social gathering in the gay city, and all the luxuries and adornments of the dinner or evening party are incomplete without them. It can hardly be doubted that a subject in which some are painfully engaged, and of which all hear or talk incessantly, must exert considerable influence on health, morality, and happiness.

Under these circumstances, we have thought it might not be without interest and profit to look at the scientific aspects of this strange matter. What we propose to consider is, firstly, whether the mode of investigation adopted in reference to it is calculated to inspire confidence in any positive result, or is such as is recommended and followed by universal consent in other scientific researches; and, secondly, in case strange phenomena do appear, for example, motions which are not caused by ordinary mechanical forces, whether these motions are explicable on natural principles by the intervention of extraordinary forces in nature, such as electricity or magnetism; or whether they are to be regarded as supernatural or spiritual manifestations.

We would remark, first, with regard to the method of investigation, that any person, of whatever education or profession, or if he have neither, feels perfectly competent to undertake it on his own responsibility. Those who have never made a scientific experiment before, are not deterred thereby from venturing upon these experiments. The results to which inexperienced investigators in any department of research, scientific, literary, or practical, may come, are not usually clothed with authority, whatever may be the general intelligence of these men, and however much above suspicion may be their truth and conscientiousness. In a question which relates to the motions of the heavenly bodies, astronomers do not rely on the observations, much less on the conjectures of a chemist, although he may stand at the head of his own science; and the chemist, in his turn, would not place a high value on the first raw experiments of the mathematician, especially in a research involving difficult organic



analysis. When we are sick, we do not consult the lawyer, except perhaps to help us in making our will; and when we are sued at the law, we do not send for the doctor.

Furthermore, what is the calm retreat in which this miscellaneous group of investigators have chosen to conduct their researches? Who ever heard of a party of ladies and gentlemen enlivening the festivities of an after-dinner hour by repairing to the dome of an observatory to determine the proper motion of a star, or its parallax? Who ever heard of any serious scientific investigation undertaken by a company of various tastes and qualifications, convened in the evening for that purpose? A scientific academy may sometimes consent to give a verdict in a question of great experimental nicety; but the examination would be held and the evidence taken, not at a general meeting, but by a select and special committee. Indeed, we think it will appear on inquiry, that nature reserves her most precious secrets, and bestows them not on any committee, though it were the selectest portion of the wisest body on earth. She shuns publicity and parade. He who worships nature, as well as he who worships the God of nature, will find it to his advantage and honor to worship her in secret. The *results* of scientific scrutiny may have been presented to the Florentine Academy, or the French Academy, or the Royal Society of London or Berlin; they may be expounded in popular lectures, illustrated at the corners of the streets, and scattered broadcast over continents by the public press; they may be rewarded by governments and applauded by the people; — but none of the valuable truth which they contain came at first by *public* observation; *that* is the result of a skilful, careful, quiet, and persevering series of experiments and deductions.

Let us pass to an examination of the facts, or the supposed facts, which are given by the experiments. They can generally be included under the description of sounds or of motions. In either case, they belong to mechanics: and the first inquiry should be, whether they can be explained by the action of ordinary mechanical forces. Those who answer this question in the negative, who resort to electricity or some more sublimated influence about which they know as much as of electricity,

have certainly seen to it, we may suppose, that the most obvious explanation is insufficient or inapplicable. They have examined the subject, we must presume, quantitatively and qualitatively. They have measured to a grain the precise force required to produce the motion; they have also measured no less carefully the sum total of pressure which accidentally or intentionally has been exerted by the fifty or hundred muscles in contact with the table; they have ascertained how many muscles were pulling in one direction, and how many were pulling in other directions, and having drawn a parallelogram of forces, they have calculated the resultant power; they have decomposed this resultant so as to find that part of it which is destroyed by the resistance of the floor or otherwise, and the balance which remains effective; they have allowed also for friction. They have done all this, not merely with truth and conscientiousness, but with prudence and care. And after their best efforts to preserve inviolate the existing laws of nature as registered in the annals of science, they still find left upon their hands residual phenomena, which require that our mechanical ideas should be enlarged and our mechanical forces augmented, or else call for the interposition of superhuman agents. In a trivial question of mechanics, unless all this were done and repeated many times, the new view or the new theory would not command a *hearing* in any court of science; still less go off with a favorable verdict. But in the case which we are considering, which in certain aspects of it is more than a dry scientific abstraction, being no less than a matter of life and death to some, all usual and reasonable precautions against deception have been neglected, and upon a degree of evidence insufficient to decide an atomic weight, the most astounding conclusions have been built, the dead have been raised to life, and the heavens have been opened to the ear of mortals.

Some of the disciples of the spurious science have the habit of rebutting any objection to their experiments and theory by this question: "Can I not believe my own senses?" or by this: "Do you not think that I tell you the truth?"

We hope to do no personal injury to any one when we say that the human senses are not, any of them or

the best of them, above suspicion. Who does not know that in courts of justice men have sworn, and honestly too, to seeing things or persons which it afterwards appeared as plain as the day they could not have seen? No doubt, in the final resort, we must rely for the results of observation and experiment on the veracity of the observer and the testimony of his senses. But cultivation imparts to the senses activity and delicacy in a measure not inferior to that in which it confers strength and acuteness on the mind. In physical science, the Epicurean philosophy is better than that of the Stoics; and nothing more distinguishes modern science and gives it its present advantage over the physical acquisitions of antiquity, than the exaltation it inculcates on the senses, refining to the nicest temper their naked edge, and then rendering them mighty instrumental assistance. Where would practical astronomy or the natural history of the stars be to-day without the telescope? The everlasting furrows which the elder Herschel laid open, when his great telescope ploughed into space one hundred years ago, have not *yet* been exhausted of their first harvest? What would be left for chemistry to do without the balance? And how prematurely would the insight of natural history into organic and inorganic structures be arrested, if her eye were not pointed with the microscope. In every department of physical science, new researches outrun the degree of excellence which belonged to the old instruments, and the invention of a nicer piece of apparatus is the era often, if it is not the occasion, of a great discovery.

Imagine Melloni continuing his researches upon heat without his marvellous heat-measurer, or without even any thermometer but his wayward sense of feeling. Imagine the great Humboldt turning scientific iconoclast and breaking in pieces the thermometers, barometers, hygrometers of modern meteorology, gauging the temperature three times a day with his bare skin, and poisoning the column of mercury which weighs the atmosphere upon his little finger. Suppose the domes of our observatories to be blocked in their revolutions, and their masterly appointments to be dismantled, and the unrivalled opticians and mechanicians of Munich to be set adrift to seek some more useful occupation. Suppose the astronomer,



as of old, to examine the sky through the soot of the tallest chimney, or to watch for the reflection of a star in the water at the bottom of a well (where certainly some truths, if not truth itself, may be found). Suppose the chemists to come to an agreement that their priceless balances are a useless extravagance, and that the atomic weights of the rarest organic compound are obtained quite well enough by the bodily arms and without the arms of the scale-beam, especially if the personal equation of right-handedness or left-handedness is eliminated by shifting the atoms from left to right and right to left. If we can suppose these various classes of scientific men to be weak enough and foolish enough to do all this, we suppose nothing more incongruous or absurd than the experiments of those who aspire to discover the laws of the natural or spiritual body by a vulgar alphabet of thumps, by the movements of rickety tables (where the virtue of the pine wood would seem to be only in the sap), or at best by the unpolished manipulations of the rough hand.

We do not always discriminate with care between the little which our senses directly teach us, and the varied, more perfect, and more valuable knowledge for which we are indirectly obliged to them. The eye may immediately inform us of the presence and color of a body, and of the direction in which it is situated. But it cannot travel off and measure its distance, it cannot go round and survey the figure of the body; neither does the eye possess, among all its marvellous and exquisite machinery, any contrivance for giving the size of what it sees, and it leaves us nothing better than a guess at the relative brightness of objects. So far as the simple eye is concerned, the magnitude and distance of the trees and stones are as immeasurable and incalculable as those of the stars. If we criticize the capabilities of the ear with the same severity, we shall find this organ wonderfully quick and alive to the colors of sound, as the eye is to those of light. But a whisper, almost inaudible by day, seems to pierce the ear at night; and this shows how much surrounding influences modify the original impressions on the senses. There is no adaptation between the mechanical character of sounds and the structure of the ear, through which the *direction* of the former is revealed

to the latter. So unstable is the tenure by which we hold our knowledge of the place whence a sound proceeds, that the ventriloquist can throw, as it were, his voice into any place, and by a look, a gesture, or an alteration of tone, unsettle our judgment. Experiment shows that when the air of a room is vibrating to a sound, there are grand nodal sections wholly at rest, and that the sound appears to come from the right or the left according as the ear is moved to the right or the left of these mechanical divisions in the vibrating column. There is also a sense which takes cognizance of muscular exertion, and which Dr. Brown has called our "muscular sense." The muscular sense measures muscular exertion, and it assists us in estimating the magnitude of other forces, such as weight and elasticity, by the effort we experience in counterpoising them by a certain amount of muscular exertion. There is no opportunity in this case for the exercise of that fine qualitative analysis for which the other senses are highly distinguished. The comparison of mechanical forces is only the balancing of *quantities*, and none of the senses are trustworthy in arithmetical investigations. The eye which distinguishes one from another the seventy-two thousand differently colored stones in the storehouses of mosaic composition, is speedily at fault in establishing a scale of relative brightness. The ear which is shocked at a slight discord pronounces faintly its decision in regard to the relative intensity of sounds. The adjudication of the senses upon *qualities* is not wholly independent; each color or note owes a part of its effect to the colors or notes with which it is associated, or to the state of the organ, whether quiescent or excited, which it addresses. The history of music and painting alike proves that natural inorganic standards, such as the tuning-fork and the prismatic colors, are needed to save the senses from rapid degeneracy. Taking their departure from these immutable standards, the eye and the ear may build their analysis of qualities upon a sure basis. In comparing *magnitudes*, the same difficulties are felt; namely, the want of standards, the influence of bodily health, and all the antecedents of the experiment. The appearance of the star Sirius in the field of the telescope, while Sir William Herschel was gazing at those faint specks in the heavens

which stand on the confines of the invisible, seemed to his strained eye as glorious as the rising of the sun in the morning, or the exit of this bright orb from a total eclipse.

Moreover, under similar circumstances, the organs can discriminate more finely as to *how* they are affected, than as to *how much* they are affected. For want of dynamometers to measure the force of light and sound, the element of intensity scarcely enters *yet* into the existing sciences of optics and acoustics. Now the muscular sense, whose function is necessarily confined to quantities to the neglect of qualities, is not exempt from the limitations which control the other organs in quantitative analysis. The same weight which almost bounds from the *fresh* finger, drags heavily at last upon its *wearied* muscle. Men most accustomed to practise with their muscular sense are not able to perceive any difference in two weights until it amounts to one thirtieth of the whole quantity. To suppose that a man can place his hand upon a table and keep it there for half an hour or longer without unconsciously exerting pressure, is to suppose a felicity of organization which contradicts all former experience. Let any one undertake to hold his hand in mid-air without raising it or lowering it for half an hour, or to place his finger against the point of a suspended needle, always touching and never pushing it, and he will see how much reason there is for believing that he can touch a table for the same length of time and not press it. The muscles will always, we may be assured, exert pressure which the muscular sense in its best state does not notice, and if we trust to the unaided sense we can never know how great this pressure may become. We must, therefore, adopt in this case the precautions suggested by the experience of all the exact sciences. The senses cannot gauge with fidelity the *magnitude* of the forces by which they are addressed. When the quantities under examination relate to space or time, the microscope comes to the aid of the eye, and then the decisions of the latter attain a high degree of numerical exactness. Hence the proverbial accuracy of astronomy. Hence, in every department of science, the effort to realize some instrument which shall make its final award in regard to any natural phenomenon of nature, relating as it may



to light, heat, electricity, magnetism, sound, elasticity, gravitation, in the language of spaces accurately divided by the micrometer and magnified by the strong help of the microscope.

Enough, we think, has been said, to show that we might with scientific propriety dismiss the whole subject of marvellous phenomena until those who pretend to have investigated it come forward with the results of careful experiment, such as alone would satisfy in cases of much less practical importance. Inasmuch, however, as a large class of disciples are not unwilling, upon slight evidence, to admit the strange phenomena as exceptions to ordinary motion produced by muscular exertion, thinking to avoid the admission of any thing marvellous or miraculous in their nature by ascribing them to electricity, we shall undertake, in the second place, to maintain that this alternative is not admissible.

There seems to be no middle course between an obvious and simple explanation of the facts and the most extreme and extravagant hypothesis. Many sounding bodies could be kept in motion by electricity; but not in a single instance by animal electricity, originating in the operator or the medium. The electrical theory is inconsistent with honesty in the parties concerned in the experiment. Admitting their veracity, whenever a case is presented of motion, or even sound, such as is found in most of the recited instances of wonderful phenomena, and which after a rigid scrutiny cannot be resolved into the ordinary causes of motion, we are ready to confess to the mystery or even the miracle of the transaction; and we utterly discard the idea of seeking refuge from this conclusion in any pretended ignorance of the laws and nature of electricity. We desire to place this view of the subject in a strong light. It is not surprising that those who have never carefully studied electricity and kindred forces should rank in the same class of marvellous and perhaps fabulous stories the dynamics of table-movements and the discoveries of Franklin, Volta, Davy, Oersted, and Faraday. Science, they say, is full of wonders. The trite propositions of the text-book astonished the world, when they were first spoken. Every new discovery comes at first in the guise of a miracle. What, they ask, do the *wisest* know about electricity or mag-

netism? Nothing whatever, we are ready to answer, about its essence or its last hiding-places; but a great deal about its manner of conducting itself. The scientific student may know as much in regard to the electric and magnetic forces as he can know of gravitation, elasticity, light, or heat. Astronomers say that planets attract one another; but *why* or *how* they attract is a blank to them, which they can fill out no better than the shepherds who watched the stars thousand of years since. They believe, either that God has made them to attract each other, by touching them originally with his finger and imparting power to them as the magnet to the iron which it touches, or that he is moving them every moment by a perpetual miracle, but in such a way that man most easily describes the motions by imagining the bodies to be gifted with a delegated power of mutual gravitation. When we say that no man is intimately acquainted with electricity or gravitation, what can we mean except that no man altogether understands God? "Who by searching can find out God? Who can find out the Almighty to perfection?" A profound and wholly satisfactory science of matter implies a perfect knowledge of its Creator and Lawgiver. No one sees or acknowledges more quickly than the student of nature, how much the highest flights of science have fallen short of this perfection. But why should any one by a false humility depreciate human discovery, or sink the Newtons and Franklins of the race to the level of common men or common children, merely because God has not made man equal to himself, or able wholly to understand himself or even his works?

We repeat it, therefore,—if we do not know the quintessence of electricity, we certainly know how it is produced and how it operates; we know what it costs and what it can do; and we also know what it cannot do. Men are constantly talking about the wonders of science and the miracles of art; and books are written upon the magic of the telescope and the microscope, the steam-engine and the telegraph. *Scientific* men also are infected with this vainglory of science; and are tempted to catch the vulgar gaze by presenting simple truths in the language of paradoxes. But we cannot look one of these paradoxes steadily in the face, not even that most

venerable of all, the hydrostatic paradox, without its dwindling into the merest truism.

Five different ways are known to science at the present day, by either of which electricity may be excited ;—  
1. Friction or mechanical action. 2. Chemical action. 3. Heat or physical action. 4. Magnetic action. 5. Animal influence. Electricity, however procured, is one and the same product, so far as we may judge from the behavior of it when at work. Among other qualifications, it is capable of producing mechanical motions. The principal mechanical forces which have a mercantile value among mankind, such as weights, springs, the wind, water, steam, are derived more or less immediately from gravitation and heat or elasticity. And although, in the economy of the physical world, the strongest force cannot say even to the weakest, "I have no need of thee," nevertheless, some of these forces, and electricity among the rest, play a minor part in the harmony of nature. Electricity in the guise of electro-magnetism has been harnessed to engines of about ten horsepower, and thus the force which plays in the aurora and the lightning has submitted to become a day-laborer for man, planing boards and doing other drudgery. Boats have been propelled by it with a velocity of three miles an hour, and an appropriation by our Congress of forty thousand dollars for making experiments on a large scale has resulted in the production of a locomotive, weighing about ten tons, and moving with the velocity of about eight miles an hour on the track.

No principle is established upon a broader induction than this: that every mechanical force which stands ready to act at the working point requires a corresponding expenditure of means at the generating point. Sometimes a large share of the power employed is lost in the machine, being of the nature of a commission imposed on man for doing the business of converting available force into useful force. Whoever has had the privilege of turning the plate to a large friction electrical machine knows how hard he works to produce the mechanical effects of electricity. In the voltaic battery an intense chemical force on the inside is the cost of the power which can scarcely move a strip of gold-leaf introduced into the outside circuit. The same chemical affinity, con-



strained by the combinations of the battery to give up its usual local disposition and become a travelling agent, is conducted round the circumvolutions of the electromagnet, and appears as the prime mover in the electromagnetic engine. This engine as well as the steam-engine is a contrivance for transmuting chemical and local force into mechanical and transferable force. Both have selected the universal and almost omnipotent affinity of oxygen. In the steam-engine oxygen promotes combustion, combustion produces heat, heat evokes elasticity, and elasticity drives the machine. In the electromagnetic engine, oxygen leads to oxidation, oxidation excites electricity, electricity produces magnetism, and magnetism pulls and pushes, and so gives the motion. In both cases, the destruction chemically of the raw material or fuel used is the price paid for the mechanical power which does the work. Experience thus far has shown that twenty-five cents' worth of steam will go as far for mechanical purposes as two dollars' worth of electricity. When the work to be done is light, so that the cost of the power is an unimportant item, or when the service required is too delicate for the gross touch of common machinery, or where, as in telegraphic operations, the working point is separated by large distances from the point of application, so that rapidity and simplicity of conveyance are indispensable recommendations, in all these cases electricity has unrivalled facilities and privileges.

Not only does the mechanical power of electricity, inconsiderable as it generally is, require a corresponding outlay of mechanical or chemical force for its production; but the field for its development is a very restricted one, and a moderate display of activity exacts a peculiar array of ways and means. It is not sufficient to say, that, as electricity is generated by friction, chemical action, heat, magnetism, and the vital functions, therefore the means are always at hand, consisting in fact of every physical, chemical, organic, and inorganic disturbance to which matter is liable. All this is true; but these accidental excitements are as momentary in their effects as they are frequent, and we can see a manifest plan in nature to prevent great electrical accumulations and out-breaks. The infrequency of the thunderclap, and its to-

tal absence in certain places and seasons, is a guaranty of the innumerable, insignificant, inaudible, and invisible discharges which ordinarily exhaust the redundant electricity; and the slyness with which the electricity fabricated by human machines slips through the fingers of the manipulator, is a match often for the most circum-spect insulation. Whenever, therefore, you see or hear that pine tables are sliding along the floor, hitching up stairs, waltzing round their axes, or cutting up other undignified antics for such venerable furniture, and are told that these are the freaks of electricity, the least that you can expect is to be shown the vast disc of glass, with the grinders at the wheel, or some foaming and suffocating voltaic battery, or an enormous electro-magnet, with the wires and other appurtenances suited to such magnificent electrical exertions. Few physical cabinets in any country contain the means for so brilliant displays of electricity, and some of the feats recorded are altogether beyond any thing found in the annals of science. There are several persons with their hands upon the table; but they, we are assured, do not push it, and cannot therefore cause it to move. There are no secret wires, no disguised batteries. We readily admit this to be true, but at the same time we deny that electricity is a conspirator in the transaction. Otherwise we must surrender all the analogies of science, and call any thing and every thing electricity.

There may be those who, with a glimmering remembrance of Galvani's celebrated frog experiment, may believe that we have, in the apparently new phenomena, another case of animal electricity. Upon this hypothesis it is not necessary to hunt for apparatus; for the operators are themselves the batteries, and the electricity oozes into the pine table from the extremities of their fingers. This we regard as the meanest and most poverty-stricken of all the attempts which have been made to meet the difficulty. We are far from denying the reality of animal electricity. On the contrary, we enumerated it as one of the five authenticated sources of electricity. Whatever in the last analysis we may come to regard as the origin of animal electricity, whether we take it for another type of thermo-electricity and so reducible to animal heat, or as a novel species of galvanism and thus

traceable to the organic chemistry of living structures, or whether it depends on some peculiarity of life not referable to common forces, the laws of animal electricity have been studied in the most profoundly scientific manner by Matteucci and Emil du Bois-Reymond, and the fruits of their investigations enrich the scientific literature of many languages.

We will repeat here a remark already made, because it is applicable to every modification of the electrical hypothesis; which is, that if the animal body, or half a dozen animal bodies, were a competent source of power for the commission assigned to it, there is nothing in the arrangement of the experiments we are discussing, to give this power a purchase upon the work to be done. To call that electricity which transgresses the established laws of electricity is, to say the least, an infelicity of expression. But the special objection to accepting animal electricity as the cause of what we wish now to explain, is the utter inadequacy of the means to the end. It would be as easy for an elephant to carry the earth on its shoulders, as for animal electricity, properly so called, to move tables or other furniture. We occasionally hear of individuals who are in themselves portable Leyden jars, always armed and charged, and ready to give a snap and a spark to whatever they approach. These cases of electrical excitement in living bodies, though the strongest, are disposed of most easily. The electricity which is so forward to exhibit itself in these persons is not animal electricity, but electricity *upon* animals. It is produced by friction, such, for example, as the brushing of the feet along the carpet, and the only peculiarity in those who can greet their friends with an electrified kiss is a dry skin, which operates like the glass columns of the electrical machine to retain upon the body the electricity ground out by rubbing. It is not impossible, by shuffling up and down a dry room, covered with a velvety carpet, to charge the body with electricity, and afterwards, by applying the lips to a gas jet, to light it into a flame. A few cases are on record of a high electrical excitement in the human body, where the effect could not be traced to any obvious friction, not even the friction of the clothes upon the wearer of them. In 1837, a lady in New Hampshire became suddenly possessed of this gift, and



retained it for several months. While in this state of electrical tension, she gave sparks to her needle, scissors, knife, pencil, the stove, and every other good conductor which she touched. A spark would pass every second from her to a metallic body within one sixteenth of an inch, and four sparks a minute would leave her for a body an inch and a half distant. Finding so many sparks too much of a good thing, she only obtained relief by establishing some good connection with the ground by a kind of lightning discharger. If we allow that the electricity accumulated on this individual was the product of her organic being, and not of some unsuspected friction, it will be of no avail in explaining the motions which so many attribute to electricity. An electrical machine of plate glass, which gives three hundred sparks two feet long as often as this lady gave four sparks an inch and a half long, would be no match for the sort of work which many ascribe to electricity.

If we pass now to proper animal electricity, our first recourse will be had to the torpedo and the gymnotus, popularly known as the electrical flounder and the electrical eel. These animals have a distinct electrical generator or battery in their bodies, and an extra lobe to the brain, which serves as a rudder to guide it; and they are easily coaxed or provoked to show off their divinely bestowed privilege by using it to decompose water into its elemental oxygen and hydrogen, to magnetize steel needles, to burn gold-leaf, or, best of all, to give shocks. The torpedo was notorious for its formidable electrical blows in the time of Pliny; and, if we may judge by the fears of our own fishermen, the world does not love them any better, the more it knows of them. But numbers would fail to state the comparison between the mechanical power required to rack a nerve, and what will suffice to move a table. The legitimate degree of mechanical exertion to be expected from the electrical gifts of the best-endowed fishes is fairly represented when we state that Mr. Faraday, after saddling with copper plates a pet electrical eel exhibited in London a few years since, succeeded in making it deflect an insignificant compass needle through an arc of forty degrees from the magnetic meridian.

If we dismiss these fishes and call up animals pro-

miscously, we make a sudden and extreme descent in the scale of electrical availability. In the ordinary structure of the animal frame, there exists no special electric organ with its nervous counterpart in the brain. The production of electricity by animals at large is incidental to the structure of the living tissue, or to the functions of the vital organism, and goes on by degrees wholly inappreciable, except to instruments having the last touch of artistic finish. How efficient is animal electricity, and of how much service it is likely to prove in the existing embarrassment, may be conjectured from this circumstance or historical fact, namely, that for fifty years after Galvani announced his discovery of animal electricity, its reality and existence were in dispute. During the first quarter of this century, only a voice here and there was lifted in favor of the "lost cause of animal electricity." One reason, undoubtedly, was the brilliant occupation which the scientific world found in chemical electricity, under the lead of what Dove calls "Volta's incomprehensible talent." Animal electricity (which ought to be called galvanism now) was no match in its feebleness for the giant strength of its rival, voltaic electricity. And if men of science had not been preëngaged, animal electricity could not have been successfully pursued, without first inventing those inconceivably delicate instruments, which in the course of time were suggested by that very department of the subject which originally supplanted animal electricity. "Thus," as the great living expounder of the animal constitution has said, "metallic electricity was able to atone for the wrong she had done to her more tender twin-sister in their earlier years." At first it was necessary to construct animal batteries out of the joints of various individuals, arranging them in systematic order, after the model of the zinc and copper plates of the ordinary chemical batteries. It was hailed as a great triumph for animal electricity when Matteucci, with his battalion of frogs, drawn up in a regular line, was able to levy an electrical force sufficient to deflect by an angle of thirty degrees a delicate sewing-needle suspended by a fibre of silk. If, still more recently, Emil du Bois-Reymond has driven a similar needle through a quadrant by employing only a microscopical bit of muscle, our thanks are due, not to animal electricity, because it

has rallied from its previous insignificance, but to the skill of the Berlin investigator, who has surpassed all others in the construction of delicate electrical tests.

The power to deflect a magnetized cambric needle from the magnetic meridian may be put down as the present *ultimatum* of the force of animal electricity. Moreover, rabbits, guinea-pigs, mice, pigeons, sparrows, tortoises, lizards, adders, slow-worms, frogs, toads, tadpoles, land and water salamanders, fresh-water crabs, and earth-worms, all have been tried on this new service, and some will answer the purpose better than a circle of men and women.

It is for each person to settle in his own mind, whether this power, which is the only one in degree or kind which belongs to animal electricity, can be the force which moves tables, excites sounds, and does other work of this description. If this is all the power that is wanted, how much simpler the supposition, that it is the result of the accidental and unconscious impulse of some unruly finger among the hundred which touch the table! Is it not impossible for the most careful and experienced manipulator to hold the hand for a short time even in contact with a body, and not push it, over and over again, with a force vastly superior to the full energy of animal electricity? But forces no larger than these we have been considering, however derived, could not move a table, nor even a pin upon the table. The electrical hypothesis must be abandoned, as more extravagant and unfounded than any that has been substituted for it.

Many persons speak of the pretended new development in physics and physiology, as if it were a simple question of veracity, and as if a denial of the presumed facts amounted to an impeachment of the word of the observer or experimenter. We may, however, we think, admit the perfect honesty of all concerned, and yet reject with propriety every one of their conclusions. They have taken no pains to eliminate disturbing forces, and how do they know whether any new forces are demanded? They have taken no pains to measure quantities, and in what way do they know how much new force is required. With such total disregard of precision in their own minds, and with such looseness in experimentation, their honesty is no palliation for their superficiality of investigation. If heaven and earth are to be searched for new



causes and fresh forces, the subject is too serious to be treated with a carelessness not befitting the meanest problem in the lowliest of all the sciences.

There are, no doubt, lurking on the confines of science, loose facts which have not yet been reclaimed and settled in their proper relations. With respect to new truths and analogies in the physical world, we cannot tell what a day may bring forth. Unless, however, we believe that the method of philosophizing practised by Galileo and Kepler, and eloquently expounded and defended by Lord Bacon, is delusive, and that the fabric of modern science, which has been two hundred years building according to this plan, rests upon the sand and not upon a rock, it is not probable that any discovery will be made which not merely enlarges, but contradicts, man's past experience. New ways of deriving forces, and of combining and applying them, have been discovered; but within the last two thousand years, what absolutely new force has been introduced into the sphere of man's physical science? Hitherto his loftiest triumphs have come from the invention of wonderful machines, by which to conquer and use the palpable forces of nature. But now visionaries in science are dreaming of the happy time when forces shall be invented or discovered which can act *without* any machine, or fulcrum, or purchase. The reluctance of the professional men of science to indulge in such fanciful expectations is often attributed to an excess of conservatism. But this conservatism only covers what they believe to be the exact method of interrogating nature. Men attach a value to the inductive method above all other methods, or the want of any method, from seeing the unfaltering and beneficent progress of all those sciences which bow to its dictation. This conservatism in the method of research does not hinder them from adopting the most eccentric and radical overturnings in science, when recommended by careful experiment. The name of a single individual, illustrious for the care, accuracy, and comprehensiveness of his research, is of sufficient authority to carry the immediate assent of the whole scientific world to a new discovery, however unexpected and marvellous. When the same authority is directed to put down loose experiment and frivolous speculation, the cry of persecution is raised. It is error, however, we believe, much oftener than the truth, which

is persecuted. The persecution of Galileo has proved the greatest misfortune to science, not so much by the injury it did to that illustrious pioneer, as by leading every scientific, or rather unscientific, innovator who has lived since, to imagine that he is persecuted whenever his novelties are resisted, and then to suppose because he is persecuted that he is another Galileo.

If scientific men are dissatisfied with the course of procedure in spiritual mechanics, why, it may be asked, do they not take the subject in hand themselves, and set it at rest? During the last hundred years they have tried to engage with this class of subjects; but the matter has recoiled from their touch, and those implicated have appealed from their decisions. The greatest authority in science has no weight with those who do not subscribe to the code and maxims of science. When we see the different impressions which the same experiments on this subject, make on the minds of those who witness them, we have little hope that any scientific decision, however solemnly pronounced, will have strength to put the spirits to rest. Moreover, to one who is accustomed to deal with the frank, honest, though inexorable laws of matter, there is no scientific pleasure in a study which places him at the mercy of the folly, or the insincerity, or the credulity of others, who must coöperate in the experiment. While scientific men admit the existence of phenomena in this connection to be investigated, they regard them as belonging to psychological, metaphysical, or moral more than to physical science; and if to physical science at all, to the physician more than to physics. But those who attribute an objective as well as a subjective reality to the spiritual manifestations have no right to convert it into a trade or a pastime; they are in duty bound to study it with proper severity and seriousness, or else not to agitate so dangerous a subject at all. We do not believe that nature will play the coquette with any one who shall sue her patiently and respectfully. Above all, trifle not with that longing which expresses itself thus: "We call, but they answer us not again," — and shun that spurious philosophy which has the conceit of spiritualizing the gross matters of earth, while it in fact materializes and brings into contempt the best and greatest spirits of heaven.

ART. II. — RELIGION, CIVILIZATION, AND SOCIAL  
STATE OF THE JAPANESE.\*

It is well known that for a little more than two centuries the Japanese government has interdicted all intercourse between its citizens and the *nan-ban*, which is a phrase in their language meaning the southern barbarians, but which, in this case, is applied to all the world. To this exclusion of foreign intercourse, however, there are two exceptions. The Chinese are allowed to enter the single port of Nagasaki, to the number of twelve junks annually, and the Dutch East India Company with two ships in a year; both being limited to certain assigned bounds within the port, and under many and most rigorous restrictions. The Dutch factory is a small insular wharf, or artificial island, communicating with the shore by a narrow draw, which is kept guarded, from which the officers are forbidden to go into the town except upon rare occasions, by special leave of the authorities, and attended by a body of the police.

The Japanese had always been extremely guarded and jealous in their intercourse with other people. The present exclusive system arose out of these circumstances. A few years before the close of the sixteenth century, the Ziogoon or generalissimo, Tayko, (this officer, though nominally second in the realm, has in fact supreme authority in the administration of the government, with some exceptions,) having died, leaving an infant son, his wife's father, Icyas, grandsire of the child, to whom he had committed the regency of the empire, endeavored to usurp the office of Ziogoon, and for this purpose made war upon the infant. Some years previously the Jesuit

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\* 1. *Manners and Customs of the Japanese in the Nineteenth Century, from Accounts of Recent Dutch Visitors of Japan, and from the German Work of DR. PH. FR. VON SIEBOLD.* London: John Murray. 1841. Harper & Brothers. Amer. Ed. 1848. 18mo. pp. 298.

2. *Japan. An Account, Geographical and Historical, from the Earliest Period at which the Islands composing this Empire were known to Europeans, down to the Present Time,—and the Expedition fitted out in the United States, &c.* By CHARLES MCFARLANE, ESQ. London. Reprinted, New York: Putnam & Co. 1852. 1 vol. 12mo. pp. 365.

3. *L'Univers, Histoire et Description de tous les Peuples — Japon, Indo-Chine, Ceylan.* Par M. DUBOIS DE JANCIGNY, Aide de Camp du Roi d'Oude. Paris: Firmin Didot Frères, Éditeurs. 1850. 8vo. pp. 662.



NEW METHOD  
OF  
CORRECTING LUNAR DISTANCES,  
AND  
IMPROVED METHOD  
OF FINDING THE  
ERROR AND RATE OF A CHRONOMETER  
BY  
EQUAL ALTITUDES.

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REPRINTED FROM THE APPENDICES OF THE AMERICAN EPHEMERIS AND NAUTICAL ALMANAC  
FOR 1855 AND 1856, BY PERMISSION OF THE HONORABLE SECRETARY OF THE NAVY.

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# DIRECTIONS

FOR USING

## THE TABLES FOR CORRECTING LUNAR DISTANCES.

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THE object of these Tables is to give the *true* correction of a lunar distance in all cases when, with the apparent distance of the moon from the sun, a planet, or star, the apparent altitudes of the two objects have also been obtained by observation. They enable us readily to take into account,—1st, the parallax of the moon in the latitude of the observer, allowing for the spheroidal figure of the earth; 2d, the parallax of the sun or a planet; 3d, the true atmospheric refraction, allowing for the actual state of the air as shown by the barometer and thermometer; and 4th, that effect of refraction which gives the apparent discs of the moon and sun an oval or elliptical figure.

The longitude deduced from a lunar observation, when no attention is paid to the spheroidal figure of the earth, to the barometer and thermometer, and the elliptical figure of the discs, may in certain cases be in error *a whole degree*. It is true these extreme cases are rare in practice; but cases are common in which from such neglect the error in the longitude is 10', 15', or 20'. Since lunars are now chiefly valuable as checks upon the chronometer, it is absolutely necessary to get rid of such errors, and to leave no other inaccuracy in the result than that which unavoidably follows from the observations. This is accomplished by means of these Tables, with an amount of labor very little greater than that which is required by the inaccurate methods in common use.

### THE OBSERVATION.

The record of a *complete* observation embraces,—

1. The latitude and approximate longitude of the place of observation.
2. The approximate local time.
3. The time of observation as shown by a chronometer, and the error of the chronometer, or its difference from mean Greenwich time.
4. The apparent distance of the moon's bright limb from a star or planet, or from the nearest limb of the sun.
5. The apparent altitude of the moon's upper or lower limb above the sea horizon.
6. The apparent altitude of the star, planet, or lower limb of the sun above the sea horizon.

## CORRECTION OF

7. The height of the barometer and thermometer.
8. The height of the eye above the level of the sea.
9. The index correction of the sextant, if a sextant is used.

The index correction of the sextant may be supposed to be previously determined; but, since even in the best instruments it is not constant, its determination should be considered a necessary part of the observation; and when the greatest precision is sought, it should be found both before and after the measurement of the distance, and its mean value taken.

The error of the chronometer above alluded to is that which is obtained by applying the daily rate (multiplied by the proper number of days) to the error found before leaving port. The agreement or disagreement of the error thus found with that found by the lunar observation will be the test of the good or bad going of the chronometer.

### PREPARATION OF THE DATA.

*Greenwich Date.* — Correct the chronometer time for its error from Greenwich time and deduce the Greenwich Date, i. e. the Greenwich day and hour (mean time), reckoning the hours in succession from 0 to 24, beginning at noon.

*Nautical Almanac.* — With the Greenwich Date enter the Almanac and take out the moon's semidiameter and horizontal parallax; and if the sun is observed, its semidiameter and horizontal parallax; \* but if a planet is observed, its horizontal parallax only.

*Apparent Altitude of the Moon.* — To the altitude given by the sextant apply the index correction of the instrument and subtract the dip of the horizon, Table I. If the lower limb is observed, add the semidiameter augmented by Table II.; if the upper limb is observed, subtract the augmented semidiameter. The result is the apparent altitude of the moon's centre, denoted " $\odot$ 's *App. Alt.*"

*Apparent Altitude of the Sun, Planet, or Star.* — To the observed altitude apply the index correction of the sextant, and subtract the dip, Table I.; and if the sun is used, add its semidiameter when the lower limb is observed, or subtract it when the upper limb is observed. The result is the apparent altitude required, denoted by " $\odot$ 's or  $\star$ 's *App. Alt.*"

*Apparent Distance.* — 1st. When the sun is used, to the observed distance (corrected for index error when necessary) add the moon's augmented semidiameter and the sun's semidiameter. 2d. When a planet or star is used, add the moon's augmented semidiameter if its nearest limb is observed, but subtract it if its farthest limb is observed. The result is "*App. Dist.*"

*Moon's Reduced Parallax and Refraction.* — Enter Table III. with the latitude of the place of observation and the moon's horizontal parallax, and take out the correction, which add to the horizontal parallax. Call the result the moon's reduced parallax, or " $\odot$ 's *Red. P.*"

Enter Table IV. with the moon's apparent altitude, and take out the mean reduced

\* The sun's horizontal parallax may be assumed as  $8''.5$ .



## LUNAR DISTANCES.

refraction, and apply to this mean refraction the corrections given in Tables IV. A. and IV. B., adding or subtracting these corrections according to the directions in the Tables. The result is the moon's reduced refraction or " $\zeta$ 's *Red. R.*"

Subtract the " $\zeta$ 's *Red. R.*" from the " $\zeta$ 's *Red. P.*" and mark the result as " $\zeta$ 's *Red. P. and R.*"

*Reduced Parallax and Refraction of Sun, Planet, or Star.\** — With the apparent altitude of the sun, planet, or star, take from Table IV. the mean reduced refraction, which correct by Tables IV. A. and IV. B. If the sun is observed, subtract its horizontal parallax (which may be always taken at  $8''.5$ ) from its reduced refraction, and mark the result as " $\odot$ 's *Red. P. & R.*" If a planet is observed subtract its horizontal parallax, and mark the result as " $\ast$ 's *Red. P. & R.*" If a star is observed its reduced refraction is at once the required " $\ast$ 's *Red. P. & R.*"

### COMPUTATION OF THE TRUE DISTANCE.

Take from Table V. the four logarithms *A, B, C, D*,† and place these logs. each at the head of a column, marking the columns *A, B, C*, and *D*, respectively; then put the

log. of $\zeta$ 's <i>Red. P. &amp; R.</i>	(Table IX.)	in Columns <i>A</i> and <i>B</i>
log. of $\odot$ 's <i>Red. P. &amp; R.</i>	"	" <i>C</i> " <i>D</i>
log. sine $\zeta$ 's <i>App. Alt.</i>	(Bowd. Table XXVII.)	" <i>A</i> " <i>D</i>
log. sine $\odot$ 's <i>App. Alt.</i>	"	" <i>B</i> " <i>C</i>
log. cotangent <i>App. Dist.</i>	"	" <i>A</i> " <i>C</i>
log. cosecant <i>App. Dist.</i>	"	" <i>B</i> " <i>D</i> .

The sum of the four logs. in Col. *A* is the log. (Table IX.) of the *First Part of  $\zeta$ 's Correction*, which is to be marked  $+$  when the *App. Dist.* is less than  $90^\circ$ , but  $-$  when the *App. Dist.* is greater than  $90^\circ$ .

The sum of the four logs. in Col. *B* is the log. (Table IX.) of the *Second Part of  $\zeta$ 's Correction*, which is always to be marked  $-$ .

The sum of the four logs. in Col. *C* is the log. (Table IX.) of the *First Part of the  $\odot$ 's or  $\ast$ 's Correction*, which is to be marked  $-$  when the *App. Dist.* is less than  $90^\circ$ , but  $+$  when the *App. Dist.* is greater than  $90^\circ$ .

The sum of the four logs. in Col. *D* is the log. (Table IX.) of the *Second Part of the  $\odot$ 's or  $\ast$ 's Correction*, which is always to be marked  $+$ .

Combine the first and second parts of the  $\zeta$ 's correction according to the signs ( $+$  or  $-$ ) prefixed; that is, take their *sum* if they have the *same* sign, but their *difference* if they have *different* signs, and prefix the sign of the greater to the result, which call " $\zeta$ 's *whole Correction.*"

In the same manner form the  $\odot$ 's or  $\ast$ 's *whole Correction.*

*First Correction of Distance.* — Combine the  $\zeta$ 's *whole corr.* and the  $\odot$ 's or  $\ast$ 's

\* The parallax of a star being zero, its "reduced parallax and refraction" become, of course, merely its "reduced refraction"; but as no mistake can arise from marking it as " $\ast$ 's *P. & R.*," this designation has been retained in order to give simplicity and uniformity at once to the rules and the tables.

† No interpolation is required in taking out these logarithms.

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*whole corr.* according to their signs; the result is the *First Correction of Distance*, which is to be added to or subtracted from the apparent distance, according as its sign is + or —.

*Second Correction of Distance.* — Enter Table VI. with the Apparent Distance and the First Correction of Distance, and take out the *Second Correction of Distance*, which is to be applied to the distance according to the directions in the side columns of the Table.

*Correction for the Elliptical Figure of the Moon's Disc, or Contraction of the Moon's Semidiameter* (Table VII.). — Enter Table VII. A. with the ☾'s App. Alt. and ☾'s Red. P. & R., and take out the number. With this number and the ☾'s whole correction enter Table VII. B. and take the required *contraction*, which is to be *added* to the App. Dist. when the *farthest* limb is observed, but *subtracted* when the *nearest* limb is observed.

*Correction for the Elliptical Figure of the Sun's Disc, or Contraction of the Sun's Semidiameter* (Table VIII.). — Enter Table VIII. A. with the ☉'s App. Alt. and ☉'s Red. P. & R. and take out the number. With this number and the ☉'s whole corr. enter Table VIII. B. and take out the required *contraction*, which is always to be *subtracted* from the distance (the *nearest* limb of the sun being always observed).

*Correction for Compression or for the Spheroidal Figure of the Earth.* — Take from the Nautical Almanac for the Greenwich Date the value of  $\log. N$ , given with the lunar distance observed.\* To  $\log. N$  add the  $\log.$  sine of the latitude of the place of observation; the sum is the  $\log.$  (Table IX.) of the required *correction for compression*. In North latitude *add* this correction to the distance if  $\log. N$  in the Nautical Almanac is marked +, or *subtract* it if  $\log. N$  is marked —; in South latitude subtract the correction when  $\log. N$  is +, and add it when  $\log. N$  is —.

All these corrections being applied to the Apparent Distance, the result is the *True Distance*.

### TO FIND THE ERROR OF THE CHRONOMETER.

Find in the Nautical Almanac the two distances between which the true distance falls. Take out the first of these together with the Prop. Log. following it and the hours of Greenwich time over it. Find the difference between the distance taken from the Almanac and the true distance, and to the  $\log.$  of this difference (Table IX.) *add* the Prop. Log. from the Almanac; the sum is the  $\log.$  (Table IX.) of an interval

\* The values of  $\log. N$  are given in this number of the Almanac for the distances from the sun only, and will be found in Table XI. of this Appendix. For other distances  $\log. N$  may be found with sufficient accuracy for ordinary purposes from Table XII. by the following rule.

From the Nautical Almanac take the moon's and star's (or planet's) declinations to the nearest whole degree. With the moon's declination and apparent distance take from Table XII. A. the *first part of N*, and mark it with the sign in the table if the declination is *North*; but if the declination is *South*, change the sign from + to — or from — to +. With the star's (or planet's) declination and apparent distance, take from Table XII. B. the *second part of N*, and mark it + when the declination is *North*, and — when *South*. Take the *sum*, or *difference*, of the two parts, according as their signs are the *same* or *different*, and to the resulting number prefix the sign of the greater. The logarithm of this number of seconds, taken in Table IX., with its sign prefixed, is the required  $\log. N$ , to be used as directed in the text.

## LUNAR DISTANCES.

of time to be added to the hours of Greenwich time taken from the Almanac. The result is the *approximate* Greenwich time.

To correct this Greenwich time, take the difference between the two Prop. Logs. in the Almanac which stand against the two distances between which the true distance falls. With this difference and the interval of time just found, enter Table X. of this Appendix, and take out the seconds, which are to be *added* to the approximate Greenwich time when the Prop. Logs. are *decreasing*, but *subtracted* when the Prop. Logs. are *increasing*. The result is the *true Greenwich time*.

The difference between the true Greenwich time and the time shown by the chronometer is the error of the chronometer as determined by the lunar observation.

### DEGREE OF DEPENDENCE.

If the error thus determined agrees with that deduced by means of the rate and original error, the chronometer has run well, and its rate is confirmed; if otherwise, more or less doubt is thrown upon the chronometer, according to the degree of accuracy of the lunar observation itself. An error of  $10''$  in the measurement of the distance produces about  $20^s$  error in the Greenwich time; and since, even with the best observers, a single set of distances is subject to a possible error of  $10''$ , it may be well to consider the chronometer as still to be trusted so long as it does not differ from the lunar by more than  $20^s$ . Since, however, so much depends upon skill in measuring the distance, the observer can only form a correct judgment of the degree of dependence to be placed upon his own observations by repeated trials and a careful comparison of his several results.



## CORRECTION OF

### EXAMPLE I.

In Lat.  $35^{\circ} 30' N.$ , Long.  $30^{\circ} W.$ , by account, on September 7th, 1855, about 6<sup>a</sup>. A. M., the Greenwich chronometer showing 8<sup>h</sup>. 29<sup>m</sup>. 57<sup>s</sup>.5 and supposed to be *fast* 21<sup>m</sup>. 1<sup>s</sup>.5; the observed distance of  $\odot$ 's and  $\zeta$ 's nearest limbs is  $43^{\circ} 52' 10''$ ; observed alt.  $\underline{\odot}$   $49^{\circ} 32' 50''$ ; observed alt.  $\underline{\odot}$   $5^{\circ} 27' 10''$ ; barometer 29.1 inches; thermometer  $75^{\circ}$ ; height of the eye above the sea 20 feet; index correction of the sextant 0. What is the error of the chronometer on Greenwich time according to these observations?

### Preparation of the Data.

Chronometer (Fast)	h. m. s. 8 29 57.5 — 21 1.5	☿'s semid. N. A. Aug. Tab. II.	14 50.0 +11.2	☿'s Par. N. A. Aug. Tab. III.	54 19.4 +3.6
Greenw. date Sept. 6	20 8 56.0	☿'s aug. Semid.	15 1.2	☿'s Red. P.	54 23.0
Obs'd Alt. ☿	49 32 50	Obs'd alt. ☉	5 27 10	Obs'd distance ☉   ☿	43 52 10
Dip Tab. I.	— 4 23	Dip	— 4 23	☿'s aug. semid.	+ 15 1
☿'s aug. semid.	+15 1	☉'s semid.	+15 55	☉'s semid.	+15 55
☿'s App. Alt.	49 43 28	☉'s App. Alt.	5 38 42	App. Dist.	44 23 6
☿'s Red. R. Tab. IV.	1 16	☉'s Red. R. Tab. IV.	8 57		
Barom. Tab. IV. A.	—3	Barom. Tab. IV. A.	—16		
Therm. Tab. IV. B.	—4	Therm. Tab. IV. B.	—28		
☿'s Red. R.	1 9	☉'s Red. R.	8 13		
☿'s Red. P.	54 23	☉'s Par.	8		
☿'s Red. P. & R.	53 14	☉'s Red. P. & R.	8 5		

### Computation of the True Distance.

A.		C.		
log. A. Tab. V.	0.0021	log. C. Tab. V.	9.9949	
log. ☐'s Red. P. & R. Tab. IX.	3.5043	log. ☉'s Red. P. & R. Tab. IX.	2.6857	
log. sin. ☐'s App. Alt.	9.8825	log. sin. ☉'s App. Alt.	8.9929	
log. cot. App. Dist.	0.0093	log. cot. App. Dist.	0.0093	
{ log. Tab. IX.	3.3982	{ log. Tab. IX.	1.6828	
{ 1st Part ☐'s corr.	+41' 42"	{ 1st Part ☉'s corr.	-0' 48"	
B.		D.		
log. B. Tab. V.	9.9951	log. D. Tab. V.	9.9992	
log. ☐'s Red. P. & R.	3.5043	log. ☉'s Red. P. & R.	2.6857	
log. sin. ☉'s App. Alt.	8.9929	log. sin. ☐'s App. Alt.	9.8825	
log. cosec. App. Dist.	0.1552	log. cosec. App. Dist.	0.1552	
{ log. Tab. IX.	2.6475	{ log. Tab. IX.	2.7226	
{ 2d Part ☐'s corr.	- 7' 24"	{ 2d Part ☉'s corr.	+8' 48"	
☐'s whole corr.	+34' 18"	☉'s whole corr.	+8' 0"	
log. N.* Tab. XI.		-0.764		
log. sin. Lat. 35° 30' N.		+9.764		
log. Tab. IX.		-0.528		
		. . . . .		
		App. Dist.		44 23 6
		1st corr.		+42 18
		2d corr. Tab. VI.		- 16
		Contraction of ☐'s }		0
		Semid. Tab. VII. }		
		Contraction of ☉'s }		- 20
		Semid. Tab. VIII. }		
		Corr. for compression		- 3
		True Distance		45 4 45

\* This log. may be found by the rule in the note on p. 18 of this Appendix, in case it is desired to apply the correction for compression to observations taken in former years.

# LUNAR DISTANCES.

*To find the Error of the Chronometer.*

True distance	45° 4' 45"			
Distance N. A. at XVIII <sup>h</sup> .	46 3 17	P. L.	0.3433	Diff. P. logs. +5
Difference	58 32	log. Tab. IX.	3.5456	
Approximate interval	2 <sup>h</sup> . 9 <sup>m</sup> . 3 <sup>s</sup> .	log. Tab. IX.	3.8889	
Add	18			
Approx. Gr. time	20 9 3			
Corr. Tab. X.	—2			
True Gr. time	20 9 1			
Gr. time by Chronom.	20 8 56			
Chronom. and lunar differ only	5			

and therefore the chronometer may be considered as going well.

This example, worked by Bowditch's First Method, gives the true distance 45° 54 44", differing from the above 59", in consequence of the omission by Bowditch of the small corrections. This difference would produce an error of 2<sup>m</sup>. 10" in the Greenwich time, and consequently the longitude in this case deduced by Bowditch's method would be in error 32'.5; that is, *more than half a degree*.

## EXAMPLE II.

In Lat. 55° 20' S., Long. 120° 25' W., by account, on August 29th, 1855, about 9<sup>h</sup>. 40<sup>m</sup>. P. M., the Greenwich chronometer showing 5<sup>h</sup> 35<sup>m</sup>. 46<sup>s</sup>.2 and from previous rate supposed *slow* 5<sup>m</sup>. 12<sup>s</sup>.; the following distance and altitudes are found, being the mean of six observations corrected for index errors. Observed distance of Fomalhaut and ♄'s farthest limb 46° 30' 23"; observed alt. ♄ 6° 26' 10"; observed alt. Fomalhaut 52° 34' 40"; barometer 31 inches; thermometer 20°; height of the eye above the sea 18 feet. What is the error of the chronometer according to these observations?

### *Preparation of the Data.*

Chronometer	h. m. s.	♄'s semid. N. A.	16' 26.3"	♄'s Par. N. A.	60' 11.8"
(Slow)	+ 5 12	Aug. Tab. II.	+2.0	Aug. Tab. III.	+8.3
Greenw. date Aug. 29 <sup>d</sup> .	17 40 58.2	♄'s aug. Semid.	16 28.3	♄'s Red. P.	60 20.1
Obs'd Alt. ♄	6 26' 10"	Obs'd alt. *	52 34' 40"	Obs'd distance * ♄	46 30' 23"
Dip Tab. I.	— 4 9	Dip	— 4 9	♄'s aug. semid.	— 16 28
♄'s aug. semid.	+16 28	*'s App. Alt.	52 30 31	App. Dist.	46 13 55
♄'s App. Alt.	6 38 29				
♄'s Red. R. Tab. IV.	7 48	*'s Red. R. Tab. IV.	1 13		
Barom. Tab. IV. A.	+16	Barom. Tab. IV. A.	+ 2		
Therm. Tab. IV. B.	+32	Therm. Tab. IV. B.	+ 5		
♄'s Red. R.	8 36	*'s Red. R.	1 20		
♄'s Red. P.	60 20	*'s Par.	0		
♄'s Red. P. & R.	51 44	*'s Red. P. & R.	1 20		

# CORRECTION OF LUNAR DISTANCES.

## Computation of the True Distance.

A.		C.			
og. A. Tab. V.	0.0274	log. C. Tab. V.	9.9999		
og. $\zeta$ 's Red. P. & R. Tab. IX.	3.4919	log. $\star$ 's Red. P. & R. Tab. IX.	1.9031		
og. sin. $\zeta$ 's App. Alt.	9.0632	log. sin. $\star$ 's App. Alt.	9.8995		
og. cot. App. Dist.	9.9813	log. cot. App. Dist.	9.9813		
{ log. Tab. IX.	2.5638	{ log. Tab. IX.	1.7838		
{ 1st Part $\zeta$ 's corr.	+6' 6"	{ 1st Part $\star$ 's corr.	-1' 1"		
B.		D.			
og. B. Tab. V.	0.0001	log. D. Tab. V.	0.0267		
og. $\zeta$ 's Red. P. & R.	3.4919	log. $\star$ 's Red. P. & R.	1.9031		
og. sin. $\star$ 's App. Alt.	9.8995	log. sin. $\zeta$ 's App. Alt.	9.0632		
og. cosec. App. Dist.	0.1414	log. cosec. App. Dist.	0.1414		
{ log. Tab. IX.	3.5329	{ log. Tab. IX.	1.1344		
{ 2d Part $\zeta$ 's corr.	-56' 51"	{ 2d Part $\star$ 's corr.	+0' 14"	App. Dist.	46° 13' 55"
$\zeta$ 's whole corr.	-50' 45"	$\star$ 's whole corr.	-0' 47"	1st corr.	-51' 32"
log. N.*	-1.230			2d corr. Tab. VI.	-22
log. sin. Lat. 55° S.	-9.913			Contraction of $\zeta$ 's	} + 17
log. Tab. IX.	+1.143			Semid. Tab. VII.	
				Corr. for compression	+ 14
				True Distance	45° 22' 32"

## To find the Error of the Chronometer.

True Distance	45° 22' 32"		
Dist. N. A. at XV. <sup>b</sup>	43 51 59	P. L.	0.2527 Diff. P. Logs. -6
Difference	1 30 33	log. Tab. IX.	3.7350
Approx. interval	2 <sup>h</sup> . 42 <sup>m</sup> . 1 <sup>s</sup> .	log. Tab. IX.	3.9877
Add	15		
Approx. Gr. time	17 42 1		
Corr. Tab. X.	+1		
True Gr. time	17 42 2		
Gr. time by chronom.	17 40 58		
Chron. and lunar differ	1 4		

and, the distances having been observed with care, the chronometer has probably changed its rate. A second observation confirming this, we must, from repeated lunars, determine a new rate, which may be used until an opportunity occurs of rating at a fixed place whose longitude is tolerably well known.

This example, worked by Bowditch's Second Method, gives the true distance 45° 21' 31", which is in error 1' 1", and would produce in the longitude deduced from it in this case an error of about 28'.

\* This value of log. N. is formed from Tab. XII. by the rule in the note on page 18 of this Appendix: thus

$$\begin{array}{l} \zeta \text{'s dec. } 4^{\circ} \text{ N., App. Dist. } 46^{\circ} \text{ in Tab. XII. A. gives 1st Part of N } - 1'' \\ \star \text{'s dec. } 30^{\circ} \text{ S., App. Dist. } 46^{\circ} \text{ in Tab. XII. B. gives 2d Part of N } - 16 \\ \text{N } - 17 \end{array}$$

the log. of which is in Tab. IX. 1.230.



## EXPLANATION OF THE TABLES.\*

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TABLE I. — *Dip of the Sea Horizon*, computed by Delambre's formula (*Astronomie*, Vol. III., Chap. XXXVI.), which, when feet are substituted for metres, is

$$D = 58''.8 \sqrt{F},$$

where  $F$  = height of the eye above the level of the sea, in feet,

$D$  = depression of the sea horizon, in seconds.

TABLE II. — *Augmentation of the Moon's Semidiameter*, computed by the formula (Francœur, *Astron. Pratique*, p. 58)

$$x = c s^2 \sin h + \frac{1}{2} c^2 s^3 \sin^2 h + \frac{1}{2} c^2 s^3,$$

where  $h$  = moon's apparent altitude,

$s$  = moon's horizontal semidiameter,

$x$  = augmentation of semidiameter for the altitude  $h$ ,

$\log c = 5.25021$ .

TABLE III. — *Augmentation of the Moon's Horizontal Parallax*, or correction to reduce the moon's equatorial horizontal parallax to that point of the earth's axis which lies in the vertical of the observer in any given latitude, computed by the formulas

$$\Delta \pi = \pi (b - 1), \quad b = \frac{1}{\sqrt{1 - e^2 \sin^2 \phi}},$$

where  $\pi$  = equatorial horizontal parallax,

$\phi$  = latitude,

$e$  = eccentricity of the meridian,  $\log e^2 = 7.81602$ ,

$\Delta \pi$  = augmentation of the horizontal parallax for the latitude  $\phi$ .

TABLE IV. — *Mean Reduced Refraction for Lunars*, computed by the formula

$$r' = \frac{r}{\cos h} = \frac{k}{\sin h},$$

\* Tables I., II., and IX., which are not peculiar to my method, are inserted to save a reference to other works for them; so that the computer requires, in connection with this volume, only a table of logarithmic sines and tangents. These three tables have, however, been recomputed, as stated in the "Explanation."

## EXPLANATION

where  $h$  = the apparent altitude,

$r$  = mean refraction, barometer 30 inches, Fahrenheit's thermometer  $50^{\circ}$ ,

$r'$  = mean reduced refraction for lunars.

The refractions employed are BESSEL's, and were taken from his table (*Astronomische Untersuchungen*, Vol. I. p. 200), which gives directly  $K = r \tan h$ .

TABLES IV. A. AND IV. B. — *Corrections of the Mean Refraction for the Height of the Barometer and Thermometer*, deduced also from Bessel's table above cited. These tables serve for correcting either  $r$  or  $r'$ , according as the one or the other is taken as the argument.

TABLE V. — *Logs. of A, B, C, and D, for computing the First Correction of the Lunar Distance*, computed by the formulas

$$A = K'^2 \frac{\sin (h + \frac{1}{2} \Delta h)}{\sin h}, \quad C = \frac{\sin (H - \Delta H)}{\sin H},$$

$$B = K' \frac{\sin (2 H - \Delta H)}{\sin 2 H}, \quad D = \frac{\sin (2 h + \Delta h)}{\sin 2 h},$$

where  $h$  = moon's apparent altitude,

$H$  = sun's, planet's, or star's apparent altitude (denoted in the tables by  $\odot$ 's or  $\star$ 's App. Alt.),

$\Delta h$  = difference of  $\odot$ 's apparent and true altitudes,

$\Delta H$  = difference of  $\odot$ 's or  $\star$ 's apparent and true altitudes,

$\log K' = .000126$ ,

and  $\Delta h$ ,  $\Delta H$  were computed from the arguments "apparent altitude" and "reduced parallax and refraction" by the formulas

$$\Delta h = (p - r') K' \cos h, \quad \Delta H = (R' - P) \cos H,$$

where  $p - r' = \odot$ 's reduced parallax and refraction,

$R' - P = \odot$ 's or  $\star$ 's reduced parallax and refraction,

$p = \odot$ 's horizontal parallax  $+ \Delta \pi$  (Table III.),

$P = \odot$ 's or  $\star$ 's horizontal parallax (for a star  $P = 0$ ),

$r' = \odot$ 's reduced refraction (Table IV.),

$R' = \odot$ 's or  $\star$ 's reduced refraction (Table IV.).

When  $h$  and  $H$  become  $90^{\circ}$ , the values of  $B$  and  $D$  assume the indeterminate forms

$$B = \frac{0}{0}, \quad D = \frac{0}{0},$$

and therefore, for computing their logarithms near the end of the table, the formulas were transformed as follows:

$$B = C K' [1 + \frac{1}{2} (R' - P) \sin 1'' \sin H],$$

$$D = \frac{A}{K'} [1 - \frac{1}{2} (p - r') K' \sin 1'' \sin h].$$

By means of Logs.  $A$ ,  $B$ ,  $C$ , and  $D$ , the first correction of the distance is found by the following formulas: —

# OF THE TABLES.

if

$$\begin{aligned} d &= \text{apparent distance of } \mathfrak{C} \text{ from } \odot \text{ or } *, \\ \Delta_1 d &= \text{first correction,} \\ A' &= A (p - r') \sin h \cot d, \\ B' &= -B (p - r') \sin H \operatorname{cosec} d, \\ C' &= -C (R' - P) \sin H \cot d, \\ D' &= D (R' - P) \sin h \operatorname{cosec} d, \end{aligned}$$

then

$$\Delta_1 d = (A' + B') + (C' + D'),$$

and in the directions for using the tables,  $A' + B'$  is called the “whole correction for  $\mathfrak{C}$ ,” and  $C' + D'$  the “whole correction for  $\odot$  or  $*$ .”

TABLE VI. — *Second Correction of the Lunar Distance*, computed by the formula

$$\Delta_2 d = -\frac{1}{2} \Delta_1 d^2 \sin 1'' \cot d.$$

Strictly, this formula should be

$$\Delta_2 d = -\frac{1}{2} \Delta d^2 \sin 1'' \cot d,$$

where  $\Delta d = \text{whole correction of distance} = \Delta_1 d + \Delta_2 d$ , so that by entering the table first with  $\Delta_1 d$  we find only an approximate value of  $\Delta_2 d$ . But if with this approximate value we form the approximate whole correction  $\Delta_1 d + \Delta_2 d$ , and enter with this as the argument in the place of  $\Delta_1 d$ , we have the true value of  $\Delta_2 d$ . It is evident, however, from the table itself, that, in most practical cases, this degree of precision is unnecessary.

TABLE VII. — *For finding the Correction of the Lunar Distance for the Contraction of the Moon's Semidiameter* (by refraction), computed by the formula

$$\Delta s = \Delta s_0 \frac{(A' + B')^2}{(p - r')^2 \cos^2 h}$$

where the notation already employed is preserved, and

$\Delta s_0 = \text{contraction of the moon's vertical semidiameter at the altitude } h,$

$\Delta s = \text{contraction of the inclined semidiameter, or of that which lies in the direction of the lunar distance.}$

This table is subdivided into Tables VII. A. and VII. B. If we put

$$g = \frac{\Delta s_0}{(p - r')^2 \cos^2 h} \times f,$$

then

$$\Delta s = (A' + B')^2 \times \frac{g}{f},$$

where  $f$  is an arbitrary factor employed to give  $g$  convenient integral values, and in these tables we have taken  $f = 18000000$ ;  $\Delta s_0$ ,  $\Delta s$ ,  $p - r'$ , and  $A' + B'$  being all expressed in seconds. Table VII. A. gives the value of  $g$  with the arguments  $p - r'$  and  $h$ ; and Table VII. B. gives  $\Delta s$  with the arguments  $A' + B'$  and  $g$ .

To find  $\Delta s_0$  from the arguments  $p - r'$  and  $h$ , the *mean* value of  $r'$  for the altitude  $h$  was added to  $p - r'$ , so that  $p$  became known; whence also the semidiameter, —



## EXPLANATION

and consequently, by means of the refraction tables, the contraction  $\Delta s_0$ . The value of  $\Delta s$  given by Table VII. B. is therefore that which corresponds to a mean state of the air, and in *extreme* cases may be in error 4'', but in no *probable* case will it be in error more than 1''. The true value of  $\Delta s$  might, however, always be found by correcting it, as refraction, by Tables IV. A. and IV. B.

TABLE VIII. — *For finding the Correction of the Lunar Distance for the Contraction of the Sun's Semidiameter* (by refraction), computed by the formula

$$\Delta S = \Delta S_0 \frac{(C' + D')^2}{(R' - P)^2 \cos^2 H},$$

where, in addition to the notation already employed,  $\Delta S_0$  = contraction of the sun's vertical semidiameter at the altitude  $H$ ;  $\Delta S$  = contraction of the inclined semidiameter. Table VIII. A., with the arguments  $R' - P$  and  $H$ , gives a number

$$G = \frac{(R' - P)^2 \cos^2 H}{\Delta S_0} \times F,$$

in which  $\Delta S_0$  is taken for a mean value of the sun's semidiameter,  $P$  is assumed at 8''.5, and  $F$  is an arbitrary factor =  $\frac{1}{200}$ ;  $R' - P$ ,  $C' + D'$ ,  $\Delta S_0$ , and  $\Delta S$  being all expressed in seconds. Table VIII. B., with the arguments  $C' + D'$  and  $G$ , gives

$$\Delta S = (C' + D')^2 \times \frac{F}{G}.$$

This value of  $\Delta S$  is that which belongs to the actual state of the air, but is for a mean value of the sun's semidiameter, the variations of which would not change the tabular value by more than 0''.5 in any case.

TABLE IX. — *Logarithms of Seconds*. This table contains the common logarithm answering to the arc (either space or time) in the argument. For the convenience of the navigator, this table is given with the argument, degrees, minutes, and seconds, or hours, minutes, and seconds, for every ten seconds, at the side, the unit figure of the seconds being found at the top.

The logarithm is given with the proper characteristic prefixed.

TABLE X. — This table contains the correction for second differences of the moon's motion.

TABLE XI. contains the values of  $\log. N$  for the distances from the sun, computed by the following formula: —

$$N = N' \pi \left( \frac{\sin \Delta}{\sin d} - \frac{\sin \delta}{\tan d} \right);$$

where  $\pi$  =  $\odot$ 's equatorial horizontal parallax,

$\Delta$  = sun's declination,

$\delta$  =  $\odot$ 's declination,

$d$  = angular distance of the moon and sun referred to the centre of the earth,

$N' = \frac{\epsilon^2}{\sqrt{(1 - \epsilon^2 \sin^2 \phi)}}$ , for which the value of  $N'$  is taken corresponding to

$\phi = 45^\circ$ , or  $\log. N' = 7.8170$ .

## OF THE TABLES.

TABLE XII. — *For finding the Value of N.* By this table an approximate value of  $N$  may be readily found, with sufficient accuracy for ordinary purposes. When very great accuracy is desired, the formula given in the explanation of Table XI. should be used. In Table XII. the moon's horizontal parallax is assumed at its mean value  $= 57' 30''$ , and the two parts of  $N$  are separately tabulated by the formulas

$$a (= \text{first part of } N) = \frac{1}{2} N \pi \sin \delta \cotan d,$$

$$b (= \text{second part of } N) = N \pi \sin \Delta \operatorname{cosec} d;$$

from which  $N$  is found by the formula

$$N = a + b.$$

NOTE. — The demonstration of most of the formulas cited in the preceding explanation is given in the *Astronomical Journal* (Cambridge, Mass.), Vol. II., Nos. 3 and 4.





# TABLES.

TABLE I.			TABLE II.							TABLE III.			
Dip of the Sea Horizon.			Augmentation of the Moon's Semidiameter.							Augmentation of the Moon's Hor. Parallax.			
Height of the Eye.	Dip of the Horizon.	Appar-ent Altitude of $\mathcal{D}$ .	$\mathcal{D}$ 's Semidiameter.						Latitu- de of Ob- serva- tion.	$\mathcal{D}$ 's Horizontal Parallax.			
			14'	15'		16'		17'		53'	57'	61'	
			30"	0"	30"	0"	30"	0"					
Feet.	' "	°	"	"	"	"	"	"	°	"	"	"	
0	0 00	0	0.1	0.1	0.1	0.1	0.2	0.2	0	0.0	0.0	0.0	
1	0 59	2	0.6	0.6	0.7	0.7	0.8	0.8	2	0.0	0.0	0.0	
2	1 23	4	1.0	1.1	1.2	1.3	1.4	1.5	4	0.1	0.1	0.1	
3	1 42	6	1.5	1.6	1.7	1.9	2.0	2.1	6	0.1	0.1	0.1	
4	1 58	8	2.0	2.1	2.3	2.4	2.6	2.7	8	0.2	0.2	0.2	
5	2 11	10	2.4	2.6	2.8	3.0	3.2	3.4	10	0.3	0.3	0.4	
6	2 24	12	2.9	3.1	3.3	3.6	3.8	4.0	12	0.5	0.5	0.5	
7	2 36	14	3.4	3.6	3.9	4.1	4.4	4.7	14	0.6	0.7	0.7	
8	2 46	16	3.8	4.1	4.4	4.7	5.0	5.3	16	0.8	0.9	0.9	
9	2 56	18	4.3	4.6	4.9	5.2	5.6	5.9	18	1.0	1.1	1.1	
10	3 06	20	4.7	5.1	5.4	5.8	6.1	6.5	20	1.2	1.3	1.4	
11	3 15	22	5.2	5.5	5.9	6.3	6.7	7.1	22	1.5	1.6	1.7	
12	3 24	24	5.6	6.0	6.4	6.8	7.3	7.7	24	1.7	1.9	2.0	
13	3 32	26	6.0	6.5	6.9	7.4	7.8	8.3	26	2.0	2.2	2.3	
14	3 40	28	6.5	6.9	7.4	7.9	8.4	8.9	28	2.3	2.5	2.6	
15	3 48	30	6.9	7.3	7.9	8.4	8.9	9.5	30	2.6	2.8	3.0	
16	3 55	32	7.3	7.8	8.3	8.9	9.4	10.0	32	2.9	3.1	3.4	
17	4 02	34	7.7	8.2	8.8	9.4	10.0	10.6	34	3.3	3.5	3.8	
18	4 09	36	8.1	8.6	9.2	9.8	10.5	11.1	36	3.6	3.9	4.1	
19	4 16	38	8.4	9.0	9.7	10.3	10.9	11.6	38	4.0	4.3	4.6	
20	4 23	40	8.8	9.4	10.1	10.7	11.4	12.1	40	4.3	4.6	5.0	
21	4 29	42	9.2	9.8	10.5	11.2	11.9	12.6	42	4.7	5.0	5.4	
22	4 36	44	9.5	10.2	10.9	11.6	12.3	13.1	44	5.0	5.4	5.8	
23	4 42	46	9.8	10.5	11.3	12.0	12.8	13.6	46	5.4	5.8	6.2	
24	4 48	48	10.2	10.9	11.6	12.4	13.2	14.0	48	5.8	6.2	6.6	
25	4 54	50	10.5	11.2	12.0	12.8	13.6	14.4	50	6.1	6.6	7.1	
26	5 00	52	10.8	11.5	12.3	13.1	14.0	14.9	52	6.5	7.0	7.5	
27	5 06	54	11.1	11.8	12.7	13.5	14.4	15.3	54	6.8	7.4	7.9	
28	5 11	56	11.3	12.1	13.0	13.8	14.7	15.6	56	7.2	7.7	8.3	
29	5 17	58	11.6	12.4	13.3	14.1	15.1	16.0	58	7.5	8.1	8.6	
30	5 22	60	11.8	12.7	13.5	14.4	15.4	16.3	60	7.8	8.4	9.0	
35	5 48	62	12.1	12.9	13.8	14.7	15.7	16.6	62	8.1	8.8	9.4	
40	6 12	64	12.3	13.2	14.1	15.0	16.0	16.9	64	8.4	9.1	9.7	
45	6 34	66	12.5	13.4	14.3	15.2	16.2	17.2	66	8.7	9.4	10.0	
50	6 56	68	12.7	13.6	14.5	15.5	16.5	17.5	68	9.0	9.7	10.3	
55	7 16	70	12.9	13.8	14.7	15.7	16.7	17.7	70	9.2	9.9	10.6	
60	7 35	72	13.0	13.9	14.9	15.9	16.9	17.9	72	9.5	10.2	10.9	
65	7 54	74	13.1	14.1	15.0	16.0	17.1	18.1	74	9.7	10.4	11.1	
70	8 12	76	13.3	14.2	15.2	16.2	17.2	18.3	76	9.8	10.6	11.3	
75	8 29	78	13.4	14.3	15.3	16.3	17.4	18.4	78	10.0	10.8	11.5	
80	8 46	80	13.5	14.4	15.4	16.4	17.5	18.6	80	10.1	10.9	11.7	
85	9 02	82	13.5	14.5	15.5	16.5	17.6	18.7	82	10.3	11.0	11.8	
90	9 18	84	13.6	14.6	15.6	16.6	17.6	18.7	84	10.3	11.1	11.9	
95	9 33	86	13.6	14.6	15.6	16.6	17.7	18.8	86	10.4	11.2	12.0	
100	9 48	88	13.7	14.6	15.6	16.7	17.7	18.8	88	10.4	11.2	12.0	
		90	13.7	14.6	15.6	16.7	17.7	18.8	90	10.5	11.3	12.0	

# TABLE IV.

Mean *Reduced Refraction* for Lunars. Barometer 30 inches. Fahrenheit's Thermometer 50°.

Apparent Altitude.	Reduced Refraction.	Diff to P.	Apparent Altitude.	Reduced Refraction.	Apparent Altitude.	Reduced Refraction	Apparent Altitude.	Reduced Refraction.
0 0	9 54.2	1.6	10 0	5 24.1	15 0	3 41.7	27 0	2 7.8
5 5	9 46.3	1.5	5 5	5 21.6	10 10	3 39.4	27 30	2 5.7
10 10	9 38.6	1.5	10 10	5 19.2	20 20	3 37.1	28 0	2 3.7
15 15	9 31.0	1.5	15 15	5 16.8	30 30	3 34.9	28 30	2 1.7
20 20	9 23.7	1.4	20 20	5 14.4	40 40	3 32.7	29 0	1 59.8
25 25	9 16.5	1.4	25 25	5 12.1	50 50	3 30.6	29 30	1 58.0
5 30	9 9.5	1.4	10 30	5 9.8	16 0	3 28.5	30 0	1 56.2
35 35	9 2.7	1.3	35 35	5 7.5	10 10	3 26.5	30 30	1 54.5
40 40	8 56.0	1.3	40 40	5 5.3	20 20	3 24.5	31 0	1 52.8
45 45	8 49.5	1.3	45 45	5 3.1	30 30	3 22.6	31 30	1 51.2
50 50	8 43.1	1.2	50 50	5 0.9	40 40	3 20.7	32 0	1 49.7
55 55	8 36.9	1.2	55 55	4 58.8	50 50	3 18.8	32 30	1 48.2
6 0	8 30.9	1.2	11 0	4 56.7	17 0	3 16.9	33 0	1 46.7
5 5	8 24.9	1.2	5 5	4 54.6	10 10	3 15.1	33 30	1 45.3
10 10	8 19.1	1.1	10 10	4 52.5	20 20	3 13.4	34 0	1 44.0
15 15	8 13.4	1.1	15 15	4 50.5	30 30	3 11.6	34 30	1 42.7
20 20	8 7.8	1.1	20 20	4 48.5	40 40	3 9.9	35 0	1 41.4
25 25	8 2.4	1.1	25 25	4 46.6	50 50	3 8.2	35 30	1 40.2
6 30	7 57.0	1.0	11 30	4 44.6	18 0	3 6.6	36 0	1 39.0
35 35	7 51.8	1.0	35 35	4 42.7	10 10	3 5.0	36 30	1 37.8
40 40	7 46.7	1.0	40 40	4 40.8	20 20	3 3.4	37 0	1 36.7
45 45	7 41.7	1.0	45 45	4 38.9	30 30	3 1.8	37 30	1 35.6
50 50	7 36.7	1.0	50 50	4 37.1	40 40	3 0.3	38 0	1 34.5
55 55	7 31.9	0.9	55 55	4 35.3	50 50	2 58.8	38 30	1 33.5
7 0	7 27.2	0.9	12 0	4 33.5	19 0	2 57.3	39 0	1 32.5
5 5	7 22.6	0.9	5 5	4 31.7	10 10	2 55.9	39 30	1 31.5
10 10	7 18.0	0.9	10 10	4 30.0	20 20	2 54.4	40 0	1 30.6
15 15	7 13.6	0.9	15 15	4 28.3	30 30	2 53.0	40 30	1 29.6
20 20	7 9.2	0.9	20 20	4 26.6	40 40	2 51.6	41 0	1 28.7
25 25	7 4.9	0.8	25 25	4 24.9	50 50	2 50.3	41 30	1 27.8
7 30	7 0.8	0.8	12 30	4 23.2	20 0	2 49.0	42 0	1 27.0
35 35	6 56.6	0.8	35 35	4 21.6	10 10	2 47.6	42 30	1 26.2
40 40	6 52.6	0.8	40 40	4 20.0	20 20	2 46.4	43 0	1 25.4
45 45	6 48.6	0.8	45 45	4 18.4	30 30	2 45.1	43 30	1 24.6
50 50	6 44.8	0.8	50 50	4 16.8	40 40	2 43.8	44 0	1 23.8
55 55	6 40.9	0.7	55 55	4 15.2	50 50	2 42.6	44 30	1 23.1
8 0	6 37.2	0.7	13 0	4 13.7	21 0	2 41.4	45 0	1 22.4
5 5	6 33.5	0.7	5 5	4 12.2	10 10	2 40.2	46 0	1 21.0
10 10	6 29.9	0.7	10 10	4 10.7	20 20	2 39.0	47 0	1 19.6
15 15	6 26.3	0.7	15 15	4 9.2	30 30	2 37.9	48 0	1 18.4
20 20	6 22.8	0.7	20 20	4 7.7	40 40	2 36.7	49 0	1 17.2
25 25	6 19.4	0.7	25 25	4 6.3	50 50	2 35.6	50 0	1 16.0
8 30	6 16.0	0.7	13 30	4 4.8	22 0	2 34.5	51 0	1 15.0
35 35	6 12.7	0.6	35 35	4 3.4	10 10	2 33.4	52 0	1 13.9
40 40	6 9.5	0.6	40 40	4 2.0	20 20	2 32.4	53 0	1 13.0
45 45	6 6.3	0.6	45 45	4 0.6	30 30	2 31.3	54 0	1 12.0
50 50	6 3.1	0.6	50 50	3 59.3	40 40	2 30.3	55 0	1 11.1
55 55	6 0.0	0.6	55 55	3 57.9	50 50	2 29.2	56 0	1 10.3
9 0	5 57.0	0.6	14 0	3 56.6	23 0	2 28.2	57 0	1 9.5
5 5	5 54.0	0.6	5 5	3 55.3	20 20	2 26.3	58 0	1 8.7
10 10	5 51.1	0.6	10 10	3 54.0	40 40	2 24.4	59 0	1 8.0
15 15	5 48.2	0.6	15 15	3 52.7	24 0	2 22.5	60 0	1 7.3
20 20	5 45.3	0.6	20 20	3 51.4	20 20	2 20.7	62 0	1 6.0
25 25	5 42.5	0.5	25 25	3 50.1	40 40	2 18.9	64 0	1 4.9
9 30	5 39.8	0.5	14 30	3 48.9	25 0	2 17.2	66 0	1 3.8
35 35	5 37.0	0.5	35 35	3 47.6	20 20	2 15.5	68 0	1 2.9
40 40	5 34.4	0.5	40 40	3 46.4	40 40	2 13.9	70 0	1 2.0
45 45	5 31.7	0.5	45 45	3 45.2	26 0	2 12.3	73 0	1 1.0
50 50	5 29.2	0.5	50 50	3 44.0	20 20	2 10.8	76 0	1 0.1
55 55	5 26.6	0.5	55 55	3 42.8	40 40	2 9.3	80 0	0 59.2
10 0	5 24.1		15 0	3 41.7	27 0	2 7.8	90 0	0 58.3

# TABLE IV. A.

Correction of the Mean Refraction for the Height of the Barometer.

Barometer.	MEAN REFRACTION.																					Barometer.
Subtract.	0'		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'	Add.
	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	
	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	
27.50	0	2	5	7	10	12	15	17	20	23	25	28	30	33	35	38	40	43	45	48	51	
27.55	0	2	5	7	10	12	15	17	20	22	25	27	30	32	35	37	40	42	45	47	50	
27.60	0	2	5	7	10	12	14	17	19	22	24	27	29	31	34	36	39	41	44	46	49	
27.65	0	2	5	7	9	12	14	16	19	21	24	26	28	31	33	36	38	40	43	45	48	
27.70	0	2	5	7	9	11	14	16	18	21	23	25	28	30	32	35	37	39	42	44	47	
27.75	0	2	4	7	9	11	13	16	18	20	23	25	27	29	32	34	36	39	41	43	46	
27.80	0	2	4	7	9	11	13	15	18	20	22	24	27	29	31	33	35	38	40	42	45	
27.85	0	2	4	6	9	11	13	15	17	19	22	24	26	28	30	32	35	37	39	41	44	
27.90	0	2	4	6	8	10	13	15	17	19	21	23	25	27	30	32	34	36	38	40	43	
27.95	0	2	4	6	8	10	12	14	16	18	21	23	25	27	29	31	33	35	37	39	42	
28.00	0	2	4	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	41	
28.05	0	2	4	6	8	10	12	14	16	18	20	22	24	25	27	29	31	33	35	37	39	
28.10	0	2	4	6	8	9	11	13	15	17	19	21	23	25	27	29	31	33	34	36	38	
28.15	0	2	4	6	7	9	11	13	15	17	19	20	22	24	26	28	30	32	34	36	37	
28.20	0	2	4	5	7	9	11	13	14	16	18	20	22	24	25	27	29	31	33	35	36	
28.25	0	2	3	5	7	9	10	12	14	16	18	19	21	23	25	26	28	30	32	34	35	
28.30	0	2	3	5	7	8	10	12	14	15	17	19	21	22	24	26	27	29	31	33	34	
28.35	0	2	3	5	7	8	10	12	13	15	17	18	20	22	23	25	27	28	30	32	33	
28.40	0	2	3	5	6	8	10	11	13	14	16	18	19	21	23	24	26	27	29	31	32	
28.45	0	2	3	5	6	8	9	11	12	14	16	17	19	20	22	23	25	27	28	30	31	
28.50	0	1	3	4	6	7	9	10	12	14	15	17	18	20	21	23	24	26	27	29	30	31.50
28.55	0	1	3	4	6	7	9	10	12	13	15	16	17	19	20	22	23	25	26	28	29	31.45
28.60	0	1	3	4	6	7	8	10	11	13	14	15	17	18	20	21	23	24	25	27	28	31.40
28.65	0	1	3	4	5	7	8	9	11	12	14	15	16	18	19	20	22	23	25	26	27	31.35
28.70	0	1	3	4	5	6	8	9	10	12	13	14	16	17	18	20	21	22	24	25	26	31.30
28.75	0	1	2	4	5	6	7	9	10	11	13	14	15	16	18	19	20	21	23	24	25	31.25
28.80	0	1	2	4	5	6	7	8	10	11	12	13	14	16	17	18	19	21	22	23	24	31.20
28.85	0	1	2	3	5	6	7	8	9	10	12	13	14	15	16	17	19	20	21	22	23	31.15
28.90	0	1	2	3	4	5	7	8	9	10	11	12	13	14	16	17	18	19	20	21	22	31.10
28.95	0	1	2	3	4	5	6	7	8	9	11	12	13	14	15	16	17	18	19	20	21	31.05
29.00	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	31.00
29.05	0	1	2	3	4	5	6	7	8	9	10	11	11	12	13	14	15	16	17	18	19	30.95
29.10	0	1	2	3	4	4	5	6	7	8	9	10	11	12	13	14	15	15	16	17	18	30.90
29.15	0	1	2	3	3	4	5	6	7	8	9	9	10	11	12	13	14	15	15	16	17	30.85
29.20	0	1	2	2	3	4	5	6	6	7	8	9	10	10	11	12	13	14	15	15	16	30.80
29.25	0	1	1	2	3	4	4	5	6	7	8	8	9	10	11	11	12	13	14	14	15	30.75
29.30	0	1	1	2	3	3	4	5	6	6	7	8	8	9	10	11	11	12	13	13	14	30.70
29.35	0	1	1	2	3	3	4	5	5	6	7	7	8	8	9	10	10	11	12	13	13	30.65
29.40	0	1	1	2	2	3	4	4	5	5	6	7	7	8	8	9	10	10	11	12	12	30.60
29.45	0	1	1	2	2	3	3	4	4	5	6	6	7	7	8	8	9	9	10	11	11	30.55
29.50	0	0	1	1	2	2	3	3	4	5	5	6	6	7	7	8	8	9	9	10	10	30.50
29.55	0	0	1	1	2	2	3	3	4	4	5	5	5	6	6	7	7	8	8	9	9	30.45
29.60	0	0	1	1	2	2	2	3	3	4	4	4	5	5	6	6	6	7	7	8	8	30.40
29.65	0	0	1	1	1	2	2	2	3	3	4	4	4	5	5	5	6	6	6	7	7	30.35
29.70	0	0	1	1	1	1	2	2	2	3	3	3	4	4	4	5	5	5	5	6	6	30.30
29.75	0	0	0	1	1	1	1	2	2	2	3	3	3	3	4	4	4	4	5	5	5	30.25
29.80	0	0	0	1	1	1	1	1	2	2	2	2	3	3	3	3	3	3	4	4	4	30.20
29.85	0	0	0	0	1	1	1	1	1	1	2	2	2	2	2	2	2	3	3	3	3	30.15
29.90	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2	30.10
29.95	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	30.05
30.00	0	0	0	0	0	0	0	0	00	0	0	0	0	0	0	0	0	0	0	0	0	30.00
Subtract.	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	Add.
Barometer.	0'	1'	2'	3'	4'	5'	6'	7'	8'	9'	10'	MEAN REFRACTION.										Barometer.



# TABLE IV. B.

Correction of the Mean Refraction for the Height of the Thermometer.

Thermom.		MEAN REFRACTION.																				Thermom.				
Add.		0'		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'		Add.		
		0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0		
—10		0	4	8	12	16	20	24	28	33	37	41	46	50	55	60	65	70	75	80	85	90	—10		—10	
— 8		0	4	8	12	15	19	23	27	31	36	40	44	48	53	58	62	67	72	77	82	87	— 8		— 8	
— 6		0	4	7	11	15	19	22	26	30	34	38	42	47	51	55	60	64	69	74	79	84	— 6		— 6	
— 4		0	4	7	11	14	18	22	25	29	33	37	41	45	49	53	57	62	66	71	76	80	— 4		— 4	
— 2		0	3	7	10	14	17	21	24	28	31	35	39	43	47	51	55	59	64	68	72	77	— 2		— 2	
0		0	3	7	10	13	16	20	23	27	30	34	37	41	45	49	53	57	61	65	69	74	0		0	
2		0	3	6	9	12	16	19	22	25	29	32	36	39	43	47	50	54	58	62	66	70	2		2	
4		0	3	6	9	12	15	18	21	24	28	31	34	37	41	44	48	52	55	59	63	67	4		4	
6		0	3	6	8	11	14	17	20	23	26	29	32	36	39	42	46	49	53	56	60	64	6		6	
8		0	3	5	8	11	14	16	19	22	25	28	31	34	37	40	43	47	50	54	57	61	8		8	
10		0	3	5	8	10	13	15	18	21	24	26	29	32	35	38	41	44	48	51	54	58	10		10	
11		0	2	5	7	10	13	15	18	20	23	26	28	31	34	37	40	43	46	49	53	56	11		11	
12		0	2	5	7	10	12	15	17	20	22	25	28	30	33	36	39	42	45	48	51	54	12		12	
13		0	2	5	7	9	12	14	17	19	22	24	27	30	32	35	38	41	44	47	50	53	13		13	
14		0	2	5	7	9	11	14	16	19	21	24	26	29	31	34	37	40	42	45	48	51	14		14	
15		0	2	4	7	9	11	13	16	18	20	23	25	28	30	33	36	38	41	44	47	50	15		15	
16		0	2	4	6	9	11	13	15	18	20	22	25	27	29	32	35	37	40	43	45	48	16		16	
17		0	2	4	6	8	10	13	15	17	19	21	24	26	29	31	33	36	39	41	44	47	17		17	
18		0	2	4	6	8	10	12	14	16	19	21	23	25	28	30	32	35	37	40	43	45	18		18	
19		0	2	4	6	8	10	12	14	16	18	20	22	24	27	29	31	34	36	39	41	44	19		19	
20		0	2	4	6	8	9	11	13	15	17	19	22	24	26	28	30	33	35	37	40	42	20		20	
21		0	2	4	5	7	9	11	13	15	17	19	21	23	25	27	29	31	34	36	38	41	21		21	
22		0	2	3	5	7	9	11	12	14	16	18	20	22	24	26	28	30	32	35	37	39	22		22	
23		0	2	3	5	7	8	10	12	14	15	17	19	21	23	25	27	29	31	33	36	38	23		23	
24		0	2	3	5	6	8	10	11	13	15	17	18	20	22	24	26	28	30	32	34	36	24		24	
25		0	2	3	5	6	8	9	11	13	14	16	18	19	21	23	25	27	29	31	33	35	25		25	
26		0	1	3	4	6	7	9	11	12	14	15	17	19	20	22	24	26	28	29	31	33	26		26	
27		0	1	3	4	6	7	9	10	12	13	15	16	18	19	21	23	25	26	38	30	32	27		27	
28		0	1	3	4	5	7	8	10	11	12	14	15	17	19	20	22	23	25	27	29	30	28		28	
29		0	1	3	4	5	6	8	9	11	12	13	15	16	18	19	21	22	24	26	27	29	29		29	
30		0	1	2	4	5	6	7	9	10	11	13	14	15	17	18	20	21	23	24	26	28	30		30	
31		0	1	2	3	5	6	7	8	9	11	12	13	15	16	17	19	20	22	23	25	26	31		31	
32		0	1	2	3	4	6	7	8	9	10	11	13	14	15	16	18	19	20	22	23	25	32		32	
33		0	1	2	3	4	5	6	7	8	10	11	12	13	14	15	17	18	19	21	22	23	33		33	
34		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	16	17	18	19	21	22	34		34	
35		0	1	2	3	4	5	6	6	7	8	9	10	11	13	14	15	16	17	18	19	20	35		35	
36		0	1	2	3	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	36		36	
37		0	1	2	2	3	4	5	6	6	7	8	9	10	11	12	13	14	15	16	17	18	37		37	
38		0	1	1	2	3	4	4	5	6	7	7	8	9	10	11	12	13	13	14	15	16	38		38	
39		0	1	1	2	3	3	4	5	5	6	7	8	8	9	10	11	11	12	13	14	15	39		39	
40		0	1	1	2	2	3	4	4	5	6	6	7	8	8	9	10	10	11	12	13	13	40		40	
41		0	1	1	2	2	3	3	4	4	5	6	6	7	7	8	9	9	10	11	11	12	41		41	
42		0	0	1	1	2	2	3	3	4	4	5	5	6	7	7	8	8	9	9	10	11	42		42	
43		0	0	1	1	2	2	3	3	3	4	4	5	5	6	6	7	7	8	8	9	9	43		43	
44		0	0	1	1	1	2	2	3	3	3	4	4	4	5	5	6	6	7	7	8	8	44		44	
45		0	0	1	1	1	1	2	2	2	3	3	3	4	4	4	5	5	6	6	6	7	45		45	
46		0	0	0	1	1	1	1	2	2	2	2	2	3	3	4	4	4	4	5	5	5	46		46	
47		0	0	0	1	1	1	1	1	2	2	2	2	2	2	3	3	3	3	4	4	4	47		47	
48		0	0	0	0	0	1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	3	48		48	
49		0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	49		49	
50		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	50		50	
Add.		0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	Add.			
Thermom.		0'	1'	2'	3'	4'	5'	6'	7'	8'	9'	10'	MEAN REFRACTION.										Thermom.			

# TABLE IV. B.

Correction of the Mean Refraction for the Height of the Thermometer.

Thermom.	MEAN REFRACTION.																					Thermom.
	0'		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'	
	Subtract.																					
	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	
50°	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	50°
51	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	51
52	0	0	0	0	0	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2	3	52
53	0	0	0	1	1	1	1	1	1	2	2	2	2	2	2	3	3	3	3	4	4	53
54	0	0	0	1	1	1	1	2	2	2	2	3	3	3	3	4	4	4	5	5	5	54
55	0	0	1	1	1	1	2	2	2	3	3	3	4	4	4	5	5	5	6	6	6	55
56	0	0	1	1	1	2	2	2	3	3	4	4	4	5	5	6	6	6	7	7	8	56
57	0	0	1	1	2	2	2	3	3	4	4	5	5	6	6	6	7	8	8	9	9	57
58	0	0	1	1	2	2	3	3	4	4	5	5	6	6	7	7	8	9	9	10	10	58
59	0	1	1	2	2	3	3	4	4	5	5	6	6	7	8	8	9	10	10	11	12	59
60	0	1	1	2	2	3	3	4	5	5	6	7	7	8	9	9	10	11	11	12	13	60
61	0	1	1	2	3	3	4	4	5	6	7	7	8	9	10	10	11	12	12	13	14	61
62	0	1	1	2	3	3	4	5	6	6	7	8	9	9	10	11	12	13	14	15	15	62
63	0	1	1	2	3	4	5	5	6	7	8	8	9	10	11	12	13	14	15	16	17	63
64	0	1	2	2	3	4	5	6	7	7	8	9	10	11	12	13	14	15	16	17	18	64
65	0	1	2	3	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	65
66	0	1	2	3	4	5	6	6	7	8	9	10	11	12	14	15	16	17	18	19	20	66
67	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	16	17	18	19	20	22	67
68	0	1	2	3	4	5	6	7	8	9	11	11	13	14	15	16	18	19	20	22	23	68
69	0	1	2	3	4	5	7	8	9	10	11	12	13	15	16	17	19	20	21	23	24	69
70	0	1	2	3	5	6	7	8	9	10	12	12	14	16	17	18	20	21	22	24	25	70
71	0	1	2	4	5	6	7	8	10	11	12	13	15	16	18	19	20	22	23	25	27	71
72	0	1	2	4	5	6	8	9	10	11	13	14	16	17	18	20	21	23	25	26	28	72
73	0	1	3	4	5	7	8	9	11	12	13	14	16	18	19	21	22	24	26	27	29	73
74	0	1	3	4	5	7	8	10	11	12	14	15	17	18	20	22	23	25	27	28	30	74
75	0	1	3	4	6	7	8	10	11	13	14	16	18	19	21	22	24	26	28	29	31	75
76	0	1	3	4	6	7	9	10	12	13	15	16	18	20	22	23	25	27	29	31	32	76
77	0	1	3	5	6	8	9	11	12	14	16	17	19	21	22	24	26	28	30	32	34	77
78	0	2	3	5	6	8	9	11	13	14	16	18	20	21	23	25	27	29	31	33	35	78
79	0	2	3	5	6	8	10	11	13	15	17	18	20	22	24	26	28	30	32	34	36	79
80	0	2	3	5	7	8	10	12	14	15	17	19	21	23	25	27	29	31	33	35	37	80
81	0	2	3	5	7	9	10	12	14	16	18	20	21	24	26	28	30	32	34	36	38	81
82	0	2	4	5	7	9	11	13	14	16	18	20	22	24	26	28	31	33	35	37	40	82
83	0	2	4	5	7	9	11	13	15	17	19	21	23	25	27	29	31	34	36	38	41	83
84	0	2	4	6	8	9	11	13	15	17	19	21	23	26	28	30	32	35	37	39	42	84
85	0	2	4	6	8	10	12	14	16	18	20	22	24	26	29	31	33	36	38	40	43	85
86	0	2	4	6	8	10	12	14	16	18	20	23	25	27	29	32	34	37	39	42	44	86
87	0	2	4	6	8	10	12	14	17	19	21	23	25	28	30	32	35	38	40	43	45	87
88	0	2	4	6	8	10	13	15	17	19	21	24	26	28	31	33	36	38	41	44	46	88
89	0	2	4	6	9	11	13	15	17	20	22	24	27	29	32	34	37	39	42	45	48	89
90	0	2	4	7	9	11	13	16	18	20	23	25	27	30	32	35	38	40	43	46	49	90
91	0	2	4	7	9	11	14	16	18	21	23	25	28	31	33	36	39	41	44	47	50	91
92	0	2	5	7	9	11	14	16	19	21	24	26	29	31	34	37	39	42	45	48	51	92
93	0	2	5	7	9	12	14	17	19	22	24	27	29	32	35	37	40	43	46	49	52	93
94	0	2	5	7	10	12	14	17	19	22	25	27	30	33	35	38	41	44	47	50	53	94
95	0	2	5	7	10	12	15	17	20	22	25	28	30	33	36	39	42	45	48	51	54	95
96	0	2	5	7	10	12	15	18	20	23	26	28	31	34	37	40	43	46	49	52	55	96
97	0	3	5	8	10	13	15	18	21	23	26	29	32	35	38	41	44	47	50	53	56	97
98	0	3	5	8	10	13	16	18	21	24	27	29	32	35	38	41	44	48	51	54	58	98
99	0	3	5	8	11	13	16	19	21	24	27	30	33	36	39	42	45	49	52	55	59	99
100	0	3	5	8	11	13	16	19	22	25	28	31	34	37	40	43	46	50	53	56	60	100
Subtract.	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	Subtract.
Thermom.	0		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'	Thermom.
MEAN REFRACTION.																						

MEAN REFRACTION.



# TABLE V. LOG. A.

Logs A, B, C, and D, for Computing the *First Correction* of the Lunar Distance.

App. Alt. of $\odot$	REDUCED PARALLAX AND REFRACTION OF $\odot$ .														
	41'	42'	43'	44'	45'	46'	47'	48'	49'	50'	51'	52'	53'	54'	55'
5° 0'	.0288	.0295	.0301	.0308	.0315	.0321	.0328	.0335	.0341	.0348	.0355	.0361	.0368		
2	.0286	.0293	.0299	.0306	.0313	.0319	.0326	.0333	.0339	.0346	.0352	.0359	.0366		
4	.0284	.0291	.0297	.0304	.0311	.0317	.0324	.0330	.0337	.0344	.0350	.0357	.0363		
6	.0282	.0289	.0296	.0302	.0309	.0315	.0322	.0328	.0335	.0341	.0348	.0354	.0361		
8	.0281	.0287	.0294	.0300	.0307	.0313	.0320	.0326	.0333	.0339	.0346	.0352	.0359		
5 10	.0279	.0285	.0292	.0298	.0305	.0311	.0318	.0324	.0331	.0337	.0344	.0350	.0356		
12	.0277	.0284	.0290	.0296	.0303	.0309	.0316	.0322	.0329	.0335	.0341	.0348	.0354		
14	.0275	.0282	.0288	.0295	.0301	.0307	.0314	.0320	.0327	.0333	.0339	.0346	.0352		
16	.0274	.0280	.0286	.0293	.0299	.0306	.0312	.0318	.0325	.0331	.0337	.0344	.0350		
18	.0272	.0278	.0285	.0291	.0297	.0304	.0310	.0316	.0323	.0329	.0335	.0341	.0348		
5 20	.0270	.0277	.0283	.0289	.0296	.0302	.0308	.0314	.0321	.0327	.0333	.0339	.0346		
22	.0269	.0275	.0281	.0288	.0294	.0300	.0306	.0313	.0319	.0325	.0331	.0337	.0344		
24	.0267	.0273	.0280	.0286	.0292	.0298	.0304	.0311	.0317	.0323	.0329	.0335	.0341		
26	.0265	.0272	.0278	.0284	.0290	.0296	.0303	.0309	.0315	.0321	.0327	.0333	.0339	.0346	
28	.0264	.0270	.0276	.0282	.0289	.0295	.0301	.0307	.0313	.0319	.0325	.0331	.0337	.0344	
5 30	.0262	.0268	.0275	.0281	.0287	.0293	.0299	.0305	.0311	.0317	.0323	.0329	.0335	.0342	
32	.0261	.0267	.0273	.0279	.0285	.0291	.0297	.0303	.0309	.0315	.0321	.0327	.0334	.0340	
34	.0259	.0265	.0271	.0277	.0283	.0290	.0296	.0302	.0308	.0314	.0320	.0326	.0332	.0338	
36	.0258	.0264	.0270	.0276	.0282	.0288	.0294	.0300	.0306	.0312	.0318	.0324	.0330	.0336	
38		.0262	.0268	.0274	.0280	.0286	.0292	.0298	.0304	.0310	.0316	.0322	.0328	.0334	
5 40		.0261	.0267	.0273	.0279	.0285	.0290	.0296	.0302	.0308	.0314	.0320	.0326	.0332	
42		.0259	.0265	.0271	.0277	.0283	.0289	.0295	.0301	.0306	.0312	.0318	.0324	.0330	
44		.0258	.0264	.0270	.0275	.0281	.0287	.0293	.0299	.0305	.0311	.0316	.0322	.0328	
46		.0256	.0262	.0268	.0274	.0280	.0286	.0291	.0297	.0303	.0309	.0315	.0320	.0326	
48		.0255	.0261	.0267	.0272	.0278	.0284	.0290	.0296	.0301	.0307	.0313	.0319	.0324	
5 50		.0253	.0259	.0265	.0271	.0277	.0282	.0288	.0294	.0300	.0305	.0311	.0317	.0323	
52		.0252	.0258	.0264	.0269	.0275	.0281	.0287	.0292	.0298	.0304	.0309	.0315	.0321	
54		.0251	.0256	.0262	.0268	.0274	.0279	.0285	.0291	.0296	.0302	.0308	.0313	.0319	
56		.0249	.0255	.0261	.0266	.0272	.0278	.0283	.0289	.0295	.0300	.0306	.0312	.0317	
58		.0248	.0254	.0259	.0265	.0271	.0276	.0282	.0287	.0293	.0299	.0304	.0310	.0316	
6 0		.0247	.0252	.0258	.0263	.0269	.0275	.0280	.0286	.0291	.0297	.0303	.0308	.0314	
2		.0245	.0251	.0256	.0262	.0268	.0273	.0279	.0284	.0290	.0295	.0301	.0307	.0312	
4		.0244	.0249	.0255	.0261	.0266	.0272	.0277	.0283	.0288	.0294	.0299	.0305	.0310	
6		.0243	.0248	.0254	.0259	.0265	.0270	.0276	.0281	.0287	.0292	.0298	.0303	.0309	
8		.0241	.0247	.0252	.0258	.0263	.0269	.0274	.0280	.0285	.0291	.0296	.0302	.0307	
6 10		.0240	.0246	.0251	.0256	.0262	.0267	.0273	.0278	.0284	.0289	.0295	.0300	.0306	
12		.0239	.0244	.0250	.0255	.0261	.0266	.0271	.0277	.0282	.0288	.0293	.0299	.0304	
14		.0237	.0243	.0248	.0254	.0259	.0265	.0270	.0275	.0281	.0286	.0292	.0297	.0302	
16		.0236	.0242	.0247	.0252	.0258	.0263	.0269	.0274	.0279	.0285	.0290	.0295	.0301	
18		.0235	.0240	.0246	.0251	.0257	.0262	.0267	.0273	.0278	.0283	.0289	.0294	.0299	
6 20		.0234	.0239	.0245	.0250	.0255	.0261	.0266	.0271	.0276	.0282	.0287	.0292	.0298	
22		.0233	.0238	.0243	.0249	.0254	.0259	.0264	.0270	.0275	.0280	.0286	.0291	.0296	
24		.0231	.0237	.0242	.0247	.0253	.0258	.0263	.0268	.0274	.0279	.0284	.0289	.0295	
26			.0236	.0241	.0246	.0251	.0257	.0262	.0267	.0272	.0277	.0283	.0288	.0293	
28			.0234	.0240	.0245	.0250	.0255	.0260	.0266	.0271	.0276	.0281	.0286	.0292	.0297
6 30			.0233	.0238	.0244	.0249	.0254	.0259	.0264	.0270	.0275	.0280	.0285	.0290	.0295
32			.0232	.0237	.0242	.0248	.0253	.0258	.0263	.0268	.0273	.0278	.0284	.0289	.0294
34			.0231	.0236	.0241	.0246	.0251	.0257	.0262	.0267	.0272	.0277	.0282	.0287	.0292
36			.0230	.0235	.0240	.0245	.0250	.0255	.0260	.0266	.0271	.0276	.0281	.0286	.0291
38			.0229	.0234	.0239	.0244	.0249	.0254	.0259	.0264	.0269	.0274	.0279	.0284	.0290
6 40			.0227	.0232	.0238	.0243	.0248	.0253	.0258	.0263	.0268	.0273	.0278	.0283	.0288
42			.0226	.0231	.0236	.0241	.0246	.0252	.0257	.0262	.0267	.0272	.0277	.0282	.0287
44			.0225	.0230	.0235	.0240	.0245	.0250	.0255	.0260	.0265	.0270	.0275	.0280	.0285
46			.0224	.0229	.0234	.0239	.0244	.0249	.0254	.0259	.0264	.0269	.0274	.0279	.0284
48			.0223	.0228	.0233	.0238	.0243	.0248	.0253	.0258	.0263	.0268	.0273	.0278	.0283
6 50			.0222	.0227	.0232	.0237	.0242	.0247	.0252	.0257	.0262	.0266	.0271	.0276	.0281
52			.0221	.0226	.0231	.0236	.0241	.0246	.0250	.0255	.0260	.0265	.0270	.0275	.0280
54			.0220	.0225	.0230	.0235	.0239	.0244	.0249	.0254	.0259	.0264	.0269	.0274	.0279
56			.0219	.0224	.0229	.0233	.0238	.0243	.0248	.0253	.0258	.0263	.0267	.0272	.0277
58			.0218	.0223	.0227	.0232	.0237	.0242	.0247	.0252	.0257	.0261	.0266	.0271	.0276
7 0			.0217	.0222	.0226	.0231	.0236	.0241	.0246	.0251	.0255	.0260	.0265	.0270	.0275



# TABLE V. LOG. A.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.		REDUCED PARALLAX AND REFRACTION OF C.																
		44'	45'	46'	47'	48'	49'	50'	51'	52'	53'	54'	55'	56'	57'			
7° 0'	.0222	0226	0231	0236	0241	0246	0251	0255	0260	0265	0270	0275						
3	.0220	0225	0230	0234	0239	0244	0249	0254	0258	0263	0268	0273						
6	.0218	0223	0228	0233	0238	0242	0247	0252	0257	0261	0266	0271						
9	.0217	0222	0226	0231	0236	0241	0245	0250	0255	0260	0264	0269						
12	.0215	0220	0225	0230	0234	0239	0244	0248	0253	0258	0262	0267						
7 15	.0214	0219	0223	0228	0233	0237	0242	0247	0251	0256	0261	0265						
18	.0213	0217	0222	0226	0231	0236	0240	0245	0250	0254	0259	0263						
21	.0211	0216	0220	0225	0230	0234	0239	0243	0248	0253	0257	0262						
24	.0210	0214	0219	0223	0228	0233	0237	0242	0246	0251	0255	0260						
27	.0208	0213	0217	0222	0227	0231	0236	0240	0245	0249	0254	0258						
7 30	.0207	0211	0216	0220	0225	0230	0234	0239	0243	0248	0252	0257						
33	.0206	0210	0215	0219	0224	0228	0232	0237	0241	0246	0250	0255						
36	.0204	0209	0213	0218	0222	0227	0231	0235	0240	0244	0249	0253						
39	.0203	0207	0212	0216	0221	0225	0229	0234	0238	0243	0247	0252						
42	.0202	0206	0210	0215	0219	0224	0228	0232	0237	0241	0246	0250						
7 45	.0200	0205	0209	0213	0218	0222	0227	0231	0235	0240	0244	0248						
48	.0199	0203	0208	0212	0216	0221	0225	0229	0234	0238	0242	0247						
51	.0198	0202	0206	0211	0215	0219	0224	0228	0232	0237	0241	0245	0249					
54	.0196	0201	0205	0209	0214	0218	0222	0227	0231	0235	0239	0244	0248					
57	.0195	0200	0204	0208	0212	0217	0221	0225	0229	0234	0238	0242	0246					
8 0	.0194	0198	0203	0207	0211	0215	0219	0224	0228	0232	0236	0241	0245					
3	.0193	0197	0201	0206	0210	0214	0218	0222	0227	0231	0235	0239	0243					
6	.0192	0196	0200	0204	0208	0213	0217	0221	0225	0229	0233	0238	0242					
9		0195	0199	0203	0207	0211	0215	0220	0224	0228	0232	0236	0240					
12		0193	0198	0202	0206	0210	0214	0218	0222	0227	0231	0235	0239					
8 15		0192	0196	0201	0205	0209	0213	0217	0221	0225	0229	0233	0237					
18		0191	0195	0199	0203	0207	0212	0217	0220	0224	0228	0232	0236					
21		0190	0194	0198	0202	0206	0210	0214	0218	0222	0226	0231	0235					
24		0189	0193	0197	0201	0205	0209	0213	0217	0221	0225	0229	0233					
27		0188	0192	0196	0200	0204	0208	0212	0216	0220	0224	0228	0232					
8 30		0187	0191	0195	0199	0203	0207	0211	0215	0219	0223	0226	0230					
33		0186	0190	0193	0197	0201	0205	0209	0213	0217	0221	0225	0229					
36		0184	0188	0192	0196	0200	0204	0208	0212	0216	0220	0224	0228					
39		0183	0187	0191	0195	0199	0203	0207	0211	0215	0219	0223	0226					
42		0182	0186	0190	0194	0198	0202	0206	0210	0214	0217	0221	0225					
8 45		0181	0185	0189	0193	0197	0201	0205	0208	0212	0216	0220	0224					
48		0180	0184	0188	0192	0196	0200	0203	0207	0211	0215	0219	0223					
51		0179	0183	0187	0191	0195	0198	0202	0206	0210	0214	0218	0221					
54		0178	0182	0186	0190	0193	0197	0201	0205	0209	0212	0216	0220					
57		0177	0181	0185	0189	0192	0196	0200	0204	0208	0211	0215	0219					
9 0		0176	0180	0184	0188	0191	0195	0199	0203	0206	0210	0214	0218					
3		0175	0179	0183	0186	0190	0194	0198	0201	0205	0209	0213	0216					
6		0174	0178	0182	0185	0189	0193	0197	0200	0204	0208	0211	0215					
9		0173	0177	0181	0184	0188	0192	0196	0199	0203	0207	0210	0214					
12		0172	0176	0180	0183	0187	0191	0194	0198	0202	0206	0209	0213					
9 15		0171	0175	0179	0182	0186	0190	0193	0197	0201	0204	0208	0212					
18		0170	0174	0178	0181	0185	0189	0192	0196	0200	0203	0207	0211					
21		0170	0173	0177	0180	0184	0188	0191	0195	0199	0202	0206	0209					
24			0172	0176	0179	0183	0187	0190	0194	0198	0201	0205	0208					
27			0171	0175	0179	0182	0186	0189	0193	0196	0200	0204	0207					
9 30			0170	0174	0178	0181	0185	0188	0192	0195	0199	0203	0206					
33			0170	0173	0177	0180	0184	0187	0191	0194	0198	0201	0205					
36			0169	0172	0176	0179	0183	0186	0190	0193	0197	0200	0204					
39			0168	0171	0175	0178	0182	0185	0189	0192	0196	0199	0203					
42			0167	0170	0174	0177	0181	0184	0188	0191	0195	0198	0202					
9 45			0166	0169	0173	0176	0180	0183	0187	0190	0194	0197	0201					
48			0165	0169	0172	0176	0179	0182	0186	0189	0193	0196	0200	0203				
51			0164	0168	0171	0175	0178	0182	0185	0188	0192	0195	0199	0202				
54			0163	0167	0170	0174	0177	0181	0184	0187	0191	0194	0198	0201				
57			0163	0166	0169	0173	0176	0180	0183	0186	0190	0193	0197	0200				
10 0			0162	0165	0169	0172	0175	0179	0182	0186	0189	0192	0196	0199				

# TABLE V. LOG. A.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.	REDUCED PARALLAX AND REFRACTION OF D.																
	46'	47'	48'	49'	50'	51'	52'	53'	54'	55'	56'	57'	58'				
10° 0'	.0162	.0165	.0169	.0172	.0175	.0179	.0182	.0186	.0189	.0192	.0196	.0199					
5	.0160	.0164	.0167	.0171	.0174	.0177	.0181	.0184	.0187	.0191	.0194	.0197					
10	.0159	.0162	.0166	.0169	.0172	.0176	.0179	.0182	.0186	.0189	.0192	.0196					
15	.0158	.0161	.0164	.0168	.0171	.0174	.0178	.0181	.0184	.0187	.0191	.0194					
20	.0156	.0160	.0163	.0166	.0170	.0173	.0176	.0179	.0183	.0186	.0189	.0192					
25	.0155	.0158	.0162	.0165	.0168	.0171	.0175	.0178	.0181	.0184	.0188	.0191					
10 30	.0154	.0157	.0160	.0164	.0167	.0170	.0173	.0177	.0180	.0183	.0186	.0189					
35	.0153	.0156	.0159	.0162	.0166	.0169	.0172	.0175	.0178	.0181	.0185	.0188					
40	.0151	.0155	.0158	.0161	.0164	.0167	.0171	.0174	.0177	.0180	.0183	.0186					
45	.0150	.0153	.0157	.0160	.0163	.0166	.0169	.0172	.0175	.0179	.0182	.0185					
50	.0149	.0152	.0155	.0158	.0162	.0165	.0168	.0171	.0174	.0177	.0180	.0183					
55	.0148	.0151	.0154	.0157	.0160	.0163	.0167	.0170	.0173	.0176	.0179	.0182					
11 0	.0147	.0150	.0153	.0156	.0159	.0162	.0165	.0168	.0171	.0174	.0177	.0181					
5	.0146	.0149	.0152	.0155	.0158	.0161	.0164	.0167	.0170	.0173	.0176	.0179					
10		.0148	.0151	.0154	.0157	.0160	.0163	.0166	.0169	.0172	.0175	.0178					
15		.0146	.0149	.0152	.0155	.0158	.0161	.0164	.0167	.0170	.0173	.0176					
20		.0145	.0148	.0151	.0154	.0157	.0160	.0163	.0166	.0169	.0172	.0175					
25		.0144	.0147	.0150	.0153	.0156	.0159	.0162	.0165	.0168	.0171	.0174					
11 30		.0143	.0146	.0149	.0152	.0155	.0158	.0161	.0164	.0167	.0170	.0172					
35		.0142	.0145	.0148	.0151	.0154	.0157	.0160	.0162	.0165	.0168	.0171					
40		.0141	.0144	.0147	.0150	.0153	.0156	.0158	.0161	.0164	.0167	.0170					
45		.0140	.0143	.0146	.0149	.0151	.0154	.0157	.0160	.0163	.0166	.0169					
50		.0139	.0142	.0145	.0148	.0150	.0153	.0156	.0159	.0162	.0165	.0167					
55		.0138	.0141	.0144	.0146	.0149	.0152	.0155	.0158	.0161	.0163	.0166					
12 0		.0137	.0140	.0143	.0145	.0148	.0151	.0154	.0157	.0159	.0162	.0165					
5		.0136	.0139	.0142	.0144	.0147	.0150	.0153	.0156	.0158	.0161	.0164					
10		.0135	.0138	.0141	.0143	.0146	.0149	.0152	.0154	.0157	.0160	.0163					
15		.0134	.0137	.0140	.0142	.0145	.0148	.0151	.0153	.0156	.0159	.0162					
20		.0133	.0136	.0139	.0141	.0144	.0147	.0150	.0152	.0155	.0158	.0160					
25		.0132	.0135	.0138	.0140	.0143	.0146	.0148	.0151	.0154	.0157	.0159					
12 30		.0131	.0134	.0137	.0139	.0142	.0145	.0147	.0150	.0153	.0155	.0158					
35		.0130	.0133	.0136	.0138	.0141	.0144	.0146	.0149	.0152	.0154	.0157					
40		.0129	.0132	.0135	.0137	.0140	.0143	.0145	.0148	.0151	.0153	.0156					
45		.0129	.0131	.0134	.0136	.0139	.0142	.0144	.0147	.0150	.0152	.0155	.0158				
50		.0128	.0130	.0133	.0136	.0138	.0141	.0143	.0146	.0149	.0151	.0154	.0156				
55		.0127	.0129	.0132	.0135	.0137	.0140	.0142	.0145	.0148	.0150	.0153	.0155				
13 0		.0126	.0129	.0131	.0134	.0136	.0139	.0141	.0144	.0147	.0149	.0152	.0154				
5		.0125	.0128	.0130	.0133	.0135	.0138	.0141	.0143	.0146	.0148	.0151	.0153				
10		.0124	.0127	.0129	.0132	.0135	.0137	.0140	.0142	.0145	.0147	.0150	.0152				
15		.0123	.0126	.0129	.0131	.0134	.0136	.0139	.0141	.0144	.0146	.0149	.0151				
20		.0123	.0125	.0128	.0130	.0133	.0135	.0138	.0140	.0143	.0145	.0148	.0150				
25		.0122	.0124	.0127	.0129	.0132	.0134	.0137	.0139	.0142	.0144	.0147	.0149				
13 30		.0121	.0124	.0126	.0129	.0131	.0133	.0136	.0138	.0141	.0143	.0146	.0148				
35		.0120	.0123	.0125	.0128	.0130	.0133	.0135	.0138	.0140	.0142	.0145	.0147				
40		.0120	.0122	.0124	.0127	.0129	.0132	.0134	.0137	.0139	.0142	.0144	.0146				
45			.0121	.0124	.0126	.0128	.0131	.0133	.0136	.0138	.0141	.0143	.0145				
50			.0120	.0123	.0125	.0128	.0130	.0132	.0135	.0137	.0140	.0142	.0145				
55			.0120	.0122	.0124	.0127	.0129	.0132	.0134	.0136	.0139	.0141	.0144				
14 0			.0119	.0121	.0124	.0126	.0128	.0131	.0133	.0136	.0138	.0140	.0143				
5			.0118	.0121	.0123	.0125	.0128	.0130	.0132	.0135	.0137	.0139	.0142				
10			.0117	.0120	.0122	.0124	.0127	.0129	.0132	.0134	.0136	.0139	.0141				
15			.0117	.0119	.0121	.0124	.0126	.0128	.0131	.0133	.0135	.0138	.0140				
20			.0116	.0118	.0121	.0123	.0125	.0128	.0130	.0132	.0135	.0137	.0139				
25			.0115	.0118	.0120	.0122	.0124	.0127	.0129	.0131	.0134	.0136	.0138				
14 30			.0114	.0117	.0119	.0121	.0124	.0126	.0128	.0131	.0133	.0135	.0137				
35			.0114	.0116	.0118	.0121	.0123	.0125	.0128	.0130	.0132	.0134	.0137				
40			.0113	.0115	.0118	.0120	.0122	.0124	.0127	.0129	.0131	.0134	.0136				
45			.0112	.0115	.0117	.0119	.0121	.0124	.0126	.0128	.0130	.0133	.0135				
50			.0112	.0114	.0116	.0118	.0121	.0123	.0125	.0127	.0130	.0132	.0134				
55			.0111	.0113	.0116	.0118	.0120	.0122	.0124	.0127	.0129	.0131	.0133				
15 0			.0110	.0113	.0115	.0117	.0119	.0121	.0124	.0126	.0128	.0130	.0133				

# TABLE V. LOG. A.

Logs. A, B, C, and D, for Lunars.

App. Alt. of $\Delta$ .	REDUCED PARALLAX AND REFRACTION OF $\Delta$ .																
	48'	49'	50'	51'	52'	53'	54'	55'	56'	57'	58'	59'					
15° 0'	.0110	.0113	.0115	.0117	.0119	.0121	.0124	.0126	.0128	.0130	.0133						
10	.0109	.0111	.0113	.0116	.0118	.0120	.0122	.0124	.0127	.0129	.0131						
20	.0108	.0110	.0112	.0114	.0116	.0119	.0121	.0123	.0125	.0127	.0129						
30	.0107	.0109	.0111	.0113	.0115	.0117	.0119	.0121	.0124	.0126	.0128						
40	.0105	.0107	.0110	.0112	.0114	.0116	.0118	.0120	.0122	.0124	.0126						
50	.0104	.0106	.0108	.0110	.0112	.0115	.0117	.0119	.0121	.0123	.0125						
16 0	.0103	.0105	.0107	.0109	.0111	.0113	.0115	.0117	.0119	.0121	.0124						
10	.0102	.0104	.0106	.0108	.0110	.0112	.0114	.0116	.0118	.0120	.0122						
20	.0101	.0103	.0105	.0107	.0109	.0111	.0113	.0115	.0117	.0119	.0121						
30	.0100	.0102	.0103	.0105	.0107	.0109	.0111	.0113	.0115	.0117	.0119						
40	.0098	.0100	.0102	.0104	.0106	.0108	.0110	.0112	.0114	.0116	.0118						
50	.0097	.0099	.0101	.0103	.0105	.0107	.0109	.0111	.0113	.0115	.0117						
17 0	.0096	.0098	.0100	.0102	.0104	.0106	.0108	.0110	.0112	.0114	.0116						
10	.0095	.0097	.0099	.0101	.0103	.0105	.0107	.0109	.0110	.0112	.0114						
20	.0094	.0096	.0098	.0100	.0102	.0104	.0106	.0107	.0109	.0111	.0113						
30		.0095	.0097	.0099	.0101	.0103	.0104	.0106	.0108	.0110	.0112						
40		.0094	.0096	.0098	.0100	.0101	.0103	.0105	.0107	.0109	.0111						
50		.0093	.0095	.0097	.0099	.0100	.0102	.0104	.0106	.0108	.0109						
18 0		.0092	.0094	.0096	.0098	.0099	.0101	.0103	.0105	.0107	.0108						
10		.0091	.0093	.0095	.0097	.0098	.0100	.0102	.0104	.0105	.0107	.0109					
20		.0090	.0092	.0094	.0096	.0097	.0099	.0101	.0103	.0104	.0106	.0108					
30		.0089	.0091	.0093	.0095	.0096	.0098	.0100	.0102	.0103	.0105	.0107					
40		.0088	.0090	.0092	.0094	.0095	.0097	.0099	.0101	.0102	.0104	.0106					
50		.0088	.0089	.0091	.0093	.0094	.0096	.0098	.0099	.0101	.0103	.0105					
19 0		.0087	.0088	.0090	.0092	.0093	.0095	.0097	.0098	.0100	.0102	.0104					
10		.0086	.0087	.0089	.0091	.0092	.0094	.0096	.0098	.0099	.0101	.0103					
20		.0085	.0087	.0088	.0090	.0092	.0093	.0095	.0097	.0098	.0100	.0102					
30		.0084	.0086	.0087	.0089	.0091	.0092	.0094	.0096	.0097	.0099	.0101					
40		.0083	.0085	.0087	.0088	.0090	.0091	.0093	.0095	.0096	.0098	.0100					
50		.0082	.0084	.0086	.0087	.0089	.0090	.0092	.0094	.0095	.0097	.0099					
20 0		.0082	.0083	.0085	.0086	.0088	.0090	.0091	.0093	.0094	.0096	.0098					
10		.0081	.0082	.0084	.0086	.0087	.0089	.0090	.0092	.0093	.0095	.0097					
20		.0080	.0082	.0083	.0085	.0086	.0088	.0089	.0091	.0093	.0094	.0096					
30		.0079	.0081	.0082	.0084	.0086	.0087	.0089	.0090	.0092	.0093	.0095					
40		.0079	.0080	.0082	.0083	.0085	.0086	.0088	.0089	.0091	.0092	.0094					
50		.0078	.0079	.0081	.0082	.0084	.0085	.0087	.0088	.0090	.0091	.0093					
21 0		.0077	.0079	.0080	.0082	.0083	.0085	.0086	.0088	.0089	.0091	.0092					
10		.0076	.0078	.0079	.0081	.0082	.0084	.0085	.0087	.0088	.0090	.0091					
20		.0076	.0077	.0079	.0080	.0082	.0083	.0085	.0086	.0087	.0089	.0090					
30		.0075	.0076	.0078	.0079	.0081	.0082	.0084	.0085	.0087	.0088	.0090					
40		.0074	.0076	.0077	.0079	.0080	.0082	.0083	.0084	.0086	.0087	.0089					
50		.0074	.0075	.0076	.0078	.0079	.0081	.0082	.0084	.0085	.0086	.0088					
22 0		.0073	.0074	.0076	.0077	.0079	.0080	.0081	.0083	.0084	.0086	.0087					
10		.0072	.0074	.0075	.0076	.0078	.0079	.0081	.0082	.0083	.0085	.0086					
20		.0072	.0073	.0074	.0076	.0077	.0079	.0080	.0081	.0083	.0084	.0086					
30		.0071	.0072	.0074	.0075	.0076	.0078	.0079	.0081	.0082	.0083	.0085					
40		.0070	.0072	.0073	.0074	.0076	.0077	.0079	.0080	.0081	.0083	.0084					
50		.0070	.0071	.0072	.0074	.0075	.0076	.0078	.0079	.0081	.0082	.0083					
23 0		.0069	.0070	.0072	.0073	.0074	.0076	.0077	.0078	.0080	.0081	.0082					
10		.0068	.0070	.0071	.0072	.0074	.0075	.0076	.0078	.0079	.0080	.0082					
20		.0068	.0069	.0070	.0072	.0073	.0074	.0076	.0077	.0078	.0080	.0081					
30		.0067	.0069	.0070	.0071	.0072	.0074	.0075	.0076	.0078	.0079	.0080					
40		.0067	.0068	.0069	.0071	.0072	.0073	.0074	.0076	.0077	.0078	.0080					
50		.0066	.0067	.0069	.0070	.0071	.0073	.0074	.0075	.0076	.0078	.0079					
24 0			.0067	.0068	.0069	.0071	.0072	.0073	.0074	.0076	.0077	.0078					
10			.0066	.0067	.0069	.0070	.0071	.0073	.0074	.0075	.0076	.0078					
20			.0066	.0067	.0068	.0069	.0071	.0072	.0073	.0074	.0076	.0077					
30			.0065	.0066	.0068	.0069	.0070	.0071	.0072	.0074	.0075	.0076					
40			.0065	.0066	.0067	.0068	.0069	.0071	.0072	.0073	.0074	.0076					
50			.0064	.0065	.0066	.0068	.0069	.0070	.0071	.0072	.0074	.0075					
25 0			.0063	.0065	.0066	.0067	.0068	.0069	.0071	.0072	.0073	.0074					



# TABLE V. LOG. A.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.		REDUCED PARALLAX AND REFRACTION OF D.															
		50'	51'	52'	53'	54'	55'	56'	57'	58'	59'	60'					
25° 0'		.0063	.0065	.0066	.0067	.0068	.0069	.0071	.0072	.0073	.0074						
20		.0062	.0064	.0065	.0066	.0067	.0068	.0069	.0071	.0072	.0073						
40		.0061	.0062	.0064	.0065	.0066	.0067	.0068	.0069	.0071	.0072						
26 0		.0060	.0061	.0063	.0064	.0065	.0066	.0067	.0068	.0069	.0071						
20		.0059	.0060	.0062	.0063	.0064	.0065	.0066	.0067	.0068	.0069						
40		.0058	.0059	.0061	.0062	.0063	.0064	.0065	.0066	.0067	.0068						
27 0		.0057	.0058	.0060	.0061	.0062	.0063	.0064	.0065	.0066	.0067						
20		.0056	.0057	.0059	.0060	.0061	.0062	.0063	.0064	.0065	.0066						
40		.0055	.0057	.0058	.0059	.0060	.0061	.0062	.0063	.0064	.0065						
28 0		.0055	.0056	.0057	.0058	.0059	.0060	.0061	.0062	.0063	.0064						
20		.0054	.0055	.0056	.0057	.0058	.0059	.0060	.0061	.0062	.0063						
40		.0053	.0054	.0055	.0056	.0057	.0058	.0059	.0060	.0061	.0062						
29 0		.0052	.0053	.0054	.0055	.0056	.0057	.0058	.0059	.0060	.0061						
20		.0051	.0052	.0053	.0054	.0055	.0056	.0057	.0058	.0059	.0060						
40		.0050	.0051	.0052	.0053	.0054	.0055	.0056	.0057	.0058	.0059						
30 0		.0050	.0051	.0051	.0052	.0053	.0054	.0055	.0056	.0057	.0058						
20		.0049	.0050	.0051	.0052	.0052	.0053	.0054	.0055	.0056	.0057						
40		.0048	.0049	.0050	.0051	.0052	.0053	.0053	.0054	.0055	.0056						
31 0		.0047	.0048	.0049	.0050	.0051	.0052	.0053	.0053	.0054	.0055						
20		.0047	.0047	.0048	.0049	.0050	.0051	.0052	.0053	.0054	.0054	.0055					
40		.0046	.0047	.0048	.0048	.0049	.0050	.0051	.0052	.0053	.0054	.0054	.0055				
32 0		.0045	.0046	.0047	.0048	.0048	.0049	.0050	.0051	.0052	.0053	.0053	.0054				
20		.0044	.0045	.0046	.0047	.0048	.0049	.0049	.0050	.0051	.0052	.0052	.0053				
40		.0044	.0045	.0045	.0046	.0047	.0048	.0049	.0049	.0050	.0051	.0051	.0052				
33 0		.0043	.0044	.0045	.0045	.0046	.0047	.0048	.0049	.0049	.0050	.0050	.0051				
20		.0042	.0043	.0044	.0045	.0046	.0046	.0047	.0048	.0049	.0050	.0050	.0051				
40		.0042	.0043	.0043	.0044	.0045	.0046	.0046	.0047	.0048	.0049	.0049	.0050				
34 0		.0041	.0042	.0043	.0043	.0044	.0045	.0046	.0046	.0047	.0048	.0048	.0049				
20		.0040	.0041	.0042	.0043	.0043	.0044	.0045	.0046	.0047	.0047	.0047	.0048				
40		.0040	.0041	.0041	.0042	.0043	.0044	.0044	.0045	.0046	.0047	.0047	.0047				
35 0		.0039	.0040	.0041	.0041	.0042	.0043	.0044	.0044	.0045	.0046	.0046	.0047				
20		.0039	.0039	.0040	.0041	.0042	.0042	.0043	.0044	.0044	.0045	.0045	.0046				
40		.0038	.0039	.0039	.0040	.0041	.0042	.0042	.0043	.0044	.0044	.0044	.0045				
36 0		.0037	.0038	.0039	.0040	.0040	.0041	.0042	.0042	.0043	.0044	.0044	.0044				
20		.0037	.0038	.0038	.0039	.0040	.0040	.0041	.0042	.0042	.0043	.0043	.0044				
40		.0036	.0037	.0038	.0038	.0039	.0040	.0040	.0041	.0042	.0042	.0042	.0043				
37 0		.0036	.0036	.0037	.0038	.0038	.0039	.0040	.0040	.0041	.0042	.0042	.0042				
20		.0035	.0036	.0037	.0037	.0038	.0039	.0039	.0040	.0040	.0041	.0041	.0042				
40		.0035	.0035	.0036	.0037	.0037	.0038	.0039	.0039	.0040	.0040	.0040	.0041				
38 0		.0034	.0035	.0035	.0036	.0037	.0037	.0038	.0039	.0039	.0040	.0040	.0040				
20		.0034	.0034	.0035	.0036	.0036	.0037	.0037	.0038	.0039	.0039	.0039	.0040				
40		.0033	.0034	.0034	.0035	.0036	.0036	.0037	.0037	.0038	.0039	.0039	.0039				
39 0		.0033	.0034	.0034	.0035	.0035	.0036	.0036	.0037	.0037	.0038	.0038	.0039				
20		.0033	.0033	.0034	.0035	.0035	.0036	.0036	.0037	.0037	.0038	.0038	.0039				
40		.0032	.0033	.0033	.0034	.0035	.0035	.0036	.0036	.0037	.0037	.0037	.0037				
40 0		.0032	.0032	.0033	.0033	.0034	.0035	.0035	.0036	.0036	.0036	.0036	.0037				
20		.0031	.0032	.0032	.0033	.0034	.0034	.0035	.0035	.0035	.0036	.0036	.0036				
40		.0031	.0031	.0032	.0032	.0033	.0034	.0034	.0035	.0035	.0035	.0035	.0036				
41 0		.0030	.0031	.0031	.0032	.0033	.0033	.0034	.0034	.0035	.0035	.0035	.0035				
20		.0030	.0030	.0031	.0031	.0032	.0033	.0033	.0034	.0034	.0034	.0034	.0035				
40		.0029	.0030	.0030	.0031	.0032	.0032	.0033	.0033	.0033	.0034	.0034	.0034				
42 0		.0029	.0029	.0030	.0031	.0031	.0032	.0032	.0033	.0033	.0033	.0033	.0034				
20		.0029	.0029	.0030	.0030	.0031	.0031	.0032	.0032	.0032	.0033	.0033	.0033				
40		.0028	.0029	.0029	.0030	.0030	.0031	.0031	.0032	.0032	.0032	.0032	.0033				
43 0		.0028	.0028	.0029	.0029	.0030	.0030	.0031	.0031	.0031	.0032	.0032	.0032				
20		.0027	.0028	.0028	.0029	.0029	.0030	.0030	.0031	.0031	.0031	.0031	.0032				
40		.0027	.0027	.0028	.0028	.0029	.0029	.0030	.0030	.0030	.0031	.0031	.0031				
44 0		.0026	.0027	.0027	.0028	.0028	.0029	.0029	.0030	.0030	.0030	.0030	.0031				
20		.0026	.0026	.0027	.0027	.0028	.0028	.0029	.0029	.0029	.0030	.0030	.0030				
40		.0026	.0026	.0026	.0027	.0027	.0028	.0028	.0029	.0029	.0029	.0030	.0030				
45 0		.0025	.0026	.0026	.0027	.0027	.0027	.0027	.0028	.0028	.0029	.0029	.0029				



# TABLE V. LOG. A.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.	REDUCED PARALLAX AND REFRACTION OF D.															
	51'	52'	53'	54'	55'	56'	57'	58'	59'	60'						
45° 0'	.0025	.0026	.0026	.0027	.0027	.0027	.0028	.0028	.0029	.0029						
30	.0025	.0025	.0025	.0026	.0026	.0027	.0027	.0028	.0028	.0028						
46 0	.0024	.0024	.0025	.0025	.0026	.0026	.0027	.0027	.0027	.0028						
30	.0023	.0024	.0024	.0025	.0025	.0026	.0026	.0026	.0027	.0027						
47 0	.0023	.0023	.0024	.0024	.0025	.0025	.0025	.0026	.0026	.0026						
30	.0022	.0023	.0023	.0024	.0024	.0024	.0025	.0025	.0025	.0026						
48 0	.0022	.0022	.0023	.0023	.0023	.0024	.0024	.0024	.0025	.0025						
30	.0021	.0022	.0022	.0022	.0023	.0023	.0024	.0024	.0024	.0025						
49 0	.0021	.0021	.0022	.0022	.0022	.0023	.0023	.0023	.0024	.0024						
30	.0020	.0021	.0021	.0021	.0022	.0022	.0022	.0023	.0023	.0023						
50 0	.0020	.0020	.0020	.0021	.0021	.0022	.0022	.0022	.0023	.0023						
30	.0019	.0020	.0020	.0020	.0021	.0021	.0021	.0022	.0022	.0022						
51 0	.0019	.0019	.0020	.0020	.0020	.0020	.0021	.0021	.0021	.0022						
30	.0018	.0019	.0019	.0019	.0020	.0020	.0020	.0021	.0021	.0021						
52 0	.0018	.0018	.0019	.0019	.0019	.0019	.0020	.0020	.0020	.0021						
30	.0018	.0018	.0018	.0018	.0019	.0019	.0019	.0020	.0020	.0020						
53 0	.0017	.0017	.0018	.0018	.0018	.0018	.0019	.0019	.0019	.0020						
30	.0017	.0017	.0017	.0017	.0018	.0018	.0018	.0019	.0019	.0019						
54 0	.0016	.0016	.0017	.0017	.0017	.0018	.0018	.0018	.0018	.0019						
30	.0016	.0016	.0016	.0017	.0017	.0017	.0017	.0018	.0018	.0018						
55 0	.0015	.0016	.0016	.0016	.0016	.0017	.0017	.0017	.0017	.0018						
30	.0015	.0015	.0015	.0016	.0016	.0016	.0016	.0017	.0017	.0017						
56 0	.0015	.0015	.0015	.0015	.0016	.0016	.0016	.0016	.0016	.0017						
30	.0014	.0014	.0015	.0015	.0015	.0015	.0016	.0016	.0016	.0016						
57 0	.0014	.0014	.0014	.0015	.0015	.0015	.0015	.0015	.0016	.0016						
30	.0014	.0014	.0014	.0014	.0014	.0015	.0015	.0015	.0015	.0015						
58 0	.0013	.0013	.0014	.0014	.0014	.0014	.0014	.0015	.0015	.0015						
30	.0013	.0013	.0013	.0013	.0014	.0014	.0014	.0014	.0014	.0014						
59 0	.0012	.0013	.0013	.0013	.0013	.0013	.0014	.0014	.0014	.0014						
30	.0012	.0012	.0012	.0013	.0013	.0013	.0013	.0013	.0014	.0014						
60	.0012	.0012	.0012	.0012	.0013	.0013	.0013	.0013	.0013	.0013						
61	.0011	.0011	.0011	.0012	.0012	.0012	.0012	.0012	.0012	.0012						
62	.0011	.0011	.0011	.0011	.0011	.0011	.0011	.0012	.0012	.0012						
63	.0010	.0010	.0010	.0010	.0011	.0011	.0011	.0011	.0011	.0011						
64	.0009	.0010	.0010	.0010	.0010	.0010	.0010	.0010	.0010	.0010						
65	.0009	.0009	.0009	.0009	.0009	.0009	.0010	.0010	.0010	.0010						
66	.0008	.0008	.0009	.0009	.0009	.0009	.0009	.0009	.0009	.0009						
67	.0008	.0008	.0008	.0008	.0008	.0008	.0008	.0009	.0009	.0009						
68	.0007	.0007	.0008	.0008	.0008	.0008	.0008	.0008	.0008	.0008						
69	.0007	.0007	.0007	.0007	.0007	.0007	.0007	.0008	.0008	.0008						
70	.0007	.0007	.0007	.0007	.0007	.0007	.0007	.0007	.0007	.0007						
71	.0006	.0006	.0006	.0006	.0006	.0006	.0007	.0007	.0007	.0007						
72	.0006	.0006	.0006	.0006	.0006	.0006	.0006	.0006	.0006	.0006						
73	.0005	.0005	.0006	.0006	.0006	.0006	.0006	.0006	.0006	.0006						
74	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005						
75	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005						
76	.0004	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005	.0005						
77	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004						
78	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004						
79	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004						
80	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004	.0004						
81	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
82	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
83	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
84	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
85	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
86	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
87	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
88	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
89	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						
90	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003	.0003						

# TABLE V. LOG. B.

Logs. A, B, C, and D, for Lunars.

App. Alt. of ☉ or ✕.	REDUCED REFRACTION AND PARALLAX OF ☉ OR ✕.											
	0' 0"	0' 30"	1' 0"	1' 30"	2' 0"	2' 30"	3' 0"	3' 30"	4' 0"	4' 30"	5' 0"	5' 30"
5° 0'												
10												
20												
30												
40												
50												
6 0												
20												9.9970
40												9.9972
7 0											9.9976	9.9974
20											9.9977	9.9975
40										9.9981	9.9978	9.9976
8 0										9.9982	9.9979	9.9977
20										9.9982	9.9980	9.9978
40										9.9983	9.9981	9.9979
9 0									9.9986	9.9984	9.9982	9.9980
20									9.9986	9.9985	9.9983	9.9981
40									9.9987	9.9985	9.9983	9.9982
10 0								9.9989	9.9988	9.9986	9.9984	9.9982
11							9.9992	9.9991	9.9989	9.9987	9.9986	9.9984
12							9.9993	9.9992	9.9990	9.9989	9.9987	9.9986
13						9.9995	9.9994	9.9992	9.9991	9.9990	9.9989	9.9987
14						9.9995	9.9994	9.9993	9.9992	9.9991	9.9990	
15					9.9997	9.9996	9.9995	9.9994	9.9993	9.9992	9.9991	
16					9.9997	9.9996	9.9995	9.9994	9.9993	9.9993		
18				9.9990	9.9998	9.9997	9.9996	9.9995	9.9995			
20			0.0000	9.9999	9.9998	9.9998	9.9997	9.9996	9.9996			
25			0.0000	0.0000	9.9999	9.9999	9.9998	9.9998				
30		0.0001	0.0001	0.0000	0.0000	0.0000	9.9999					
50	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001						
90	0.0001	0.0002	0.0002	0.0002								

## LOG. C.

App. Alt. of ☉ or ✕.	REDUCED REFRACTION AND PARALLAX OF ☉ OR ✕.											
	0' 0"	0' 30"	1' 0"	1' 30"	2' 0"	2' 30"	3' 0"	3' 30"	4' 0"	4' 30"	5' 0"	5' 30"
5° 0'												
20												
40												
6 0												
20												9.9969
40												9.9970
7 0											9.9974	9.9972
8										9.9980	9.9978	9.9975
9									9.9984	9.9982	9.9980	9.9978
10								9.9988	9.9986	9.9984	9.9982	9.9981
11							9.9990	9.9989	9.9987	9.9986	9.9984	9.9982
12							9.9991	9.9990	9.9988	9.9987	9.9985	9.9984
13						9.9993	9.9992	9.9991	9.9989	9.9988	9.9987	9.9985
14						9.9994	9.9993	9.9991	9.9990	9.9989	9.9988	
15					9.9995	9.9994	9.9993	9.9992	9.9991	9.9990	9.9989	
16					9.9996	9.9995	9.9994	9.9993	9.9992	9.9990		
17					9.9996	9.9995	9.9994	9.9993	9.9992	9.9991		
18				9.9997	9.9996	9.9995	9.9994	9.9994	9.9993			
20			9.9998	9.9998	9.9997	9.9996	9.9995	9.9994	9.9993			
25			9.9999	9.9998	9.9998	9.9997	9.9996	9.9996				
30		0.0000	9.9999	9.9999	9.9998	9.9998	9.9997					
40		0.0000	9.9999	9.9999	9.9999	9.9999						
50	0.0000	0.0000	0.0000	9.9999	9.9999							
90	0.0000	0.0000	0.0000	0.0000								

# TABLE V. LOG. B.

Logs. A, B, C, and D, for Lunars.

App. Alt. of ☉ or ✕.	REDUCED REFRACTION AND PARALLAX OF ☉ OR ✕.											
	6' 0"	6' 30"	7' 0"	7' 30"	8' 0"	8' 30"	9' 0"	9' 30"	10' 0"	10' 30"	11' 0"	11' 30"
5° 0'			9.9951	9.9947	9.9944	9.9940	9.9937	9.9933	9.9929	9.9926	9.9922	9.9919
10			9.9953	9.9949	9.9946	9.9942	9.9939	9.9935	9.9932	9.9928	9.9925	9.9921
20			9.9954	9.9951	9.9948	9.9944	9.9941	9.9937	9.9934	9.9931	9.9927	9.9924
30		9.9959	9.9956	9.9952	9.9949	9.9946	9.9943	9.9939	9.9936	9.9933	9.9929	
40		9.9960	9.9957	9.9954	9.9951	9.9948	9.9944	9.9941	9.9938	9.9935	9.9932	
50	9.9965	9.9962	9.9958	9.9955	9.9952	9.9949	9.9946	9.9943	9.9940	9.9937		
6 0	9.9966	9.9963	9.9960	9.9957	9.9954	9.9951	9.9948	9.9945	9.9942	9.9939		
20	9.9968	9.9965	9.9962	9.9959	9.9956	9.9954	9.9951	9.9948	9.9945			
40	9.9969	9.9967	9.9964	9.9961	9.9959	9.9956	9.9953	9.9951	9.9948			
7 0	9.9971	9.9968	9.9966	9.9963	9.9961	9.9958	9.9956	9.9953				
20	9.9972	9.9970	9.9968	9.9965	9.9963	9.9960	9.9958					
40	9.9974	9.9971	9.9969	9.9967	9.9965	9.9962						
8 0	9.9975	9.9973	9.9971	9.9968	9.9966	9.9964						
20	9.9976	9.9974	9.9972	9.9970	9.9968							
40	9.9977	9.9975	9.9973	9.9971								
9 0	9.9978	9.9976	9.9974	9.9972								
20	9.9979	9.9977	9.9975									
40	9.9980	9.9978	9.9976									
10	9.9981	9.9979	9.9977									
11	9.9983	9.9981										
12	9.9985											
13												
14												
15												
16												
18												
20												
25												
30												
50												
90												

## LOG. C.

App. Alt. of ☉ or ✕.	REDUCED REFRACTION AND PARALLAX OF ☉ OR ✕.											
	6' 0"	6' 30"	7' 0"	7' 30"	8' 0"	8' 30"	9' 0"	9' 30"	10' 0"	10' 30"	11' 0"	11' 30"
5° 0'			9.9949	9.9946	9.9942	9.9938	9.9935	9.9931	9.9927	9.9924	9.9920	9.9916
20		9.9956	9.9953	9.9949	9.9946	9.9942	9.9939	9.9936	9.9932	9.9929	9.9925	9.9922
40	9.9962	9.9959	9.9955	9.9952	9.9949	9.9946	9.9943	9.9939	9.9936	9.9933	9.9930	
6 0	9.9964	9.9961	9.9958	9.9955	9.9952	9.9949	9.9946	9.9943	9.9940	9.9937		
20	9.9966	9.9963	9.9960	9.9957	9.9955	9.9952	9.9949	9.9946	9.9943			
40	9.9968	9.9965	9.9962	9.9960	9.9957	9.9954	9.9951	9.9949	9.9946			
7 0	9.9969	9.9967	9.9964	9.9962	9.9959	9.9956	9.9954	9.9951				
8 0	9.9973	9.9971	9.9969	9.9966	9.9964	9.9962	9.9960					
9 0	9.9976	9.9974	9.9972	9.9970	9.9968							
10	9.9979	9.9977	9.9975									
11	9.9981	9.9979										
12	9.9983											
13												
14												
15												
16												
17												
18												
20												
25												
30												
40												
50												
90												



# TABLE V. LOG. D.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.	REDUCED PARALLAX AND REFRACTION OF D.														
	41'	42'	43'	44'	45'	46'	47'	48'	49'	50'	51'	52'	53'	54'	55'
5° 0'	.0283	.0290	.0296	.0303	.0310	.0316	.0323	.0329	.0336	.0343	.0349	.0356	.0362	.0369	
3	.0280	.0287	.0293	.0300	.0307	.0313	.0320	.0326	.0333	.0339	.0346	.0352	.0359	.0365	
6	.0277	.0284	.0291	.0297	.0304	.0310	.0317	.0323	.0330	.0336	.0342	.0349	.0355	.0362	
9	.0275	.0281	.0288	.0294	.0301	.0307	.0313	.0320	.0326	.0333	.0339	.0345	.0352	.0358	
12	.0272	.0279	.0285	.0291	.0298	.0304	.0310	.0317	.0323	.0330	.0336	.0342	.0349	.0355	
5 15	.0270	.0276	.0282	.0289	.0295	.0301	.0308	.0314	.0320	.0326	.0333	.0339	.0345	.0351	
18	.0267	.0273	.0280	.0286	.0292	.0298	.0305	.0311	.0317	.0323	.0330	.0336	.0342	.0348	
21	.0264	.0271	.0277	.0283	.0289	.0296	.0302	.0308	.0314	.0320	.0327	.0333	.0339	.0345	
24	.0262	.0268	.0274	.0281	.0287	.0293	.0299	.0305	.0311	.0317	.0324	.0330	.0336	.0342	
27	.0260	.0266	.0272	.0278	.0284	.0290	.0296	.0302	.0308	.0314	.0321	.0327	.0333	.0339	
5 30	.0257	.0263	.0269	.0275	.0282	.0288	.0294	.0300	.0306	.0312	.0318	.0324	.0330	.0336	
33	.0255	.0261	.0267	.0273	.0279	.0285	.0291	.0297	.0303	.0309	.0315	.0321	.0327	.0333	
36	.0253	.0259	.0265	.0271	.0276	.0282	.0288	.0294	.0300	.0306	.0312	.0318	.0324	.0330	
39		.0256	.0262	.0268	.0274	.0280	.0286	.0292	.0298	.0303	.0309	.0315	.0321	.0327	
42		.0254	.0260	.0266	.0272	.0277	.0283	.0289	.0295	.0301	.0306	.0312	.0318	.0324	
5 45		.0252	.0258	.0263	.0269	.0275	.0281	.0287	.0292	.0298	.0304	.0310	.0315	.0321	
48		.0250	.0255	.0261	.0267	.0273	.0278	.0284	.0290	.0295	.0301	.0307	.0313	.0318	
51		.0247	.0253	.0259	.0265	.0270	.0276	.0282	.0287	.0293	.0299	.0304	.0310	.0316	
54		.0245	.0251	.0257	.0262	.0268	.0274	.0279	.0285	.0290	.0296	.0302	.0307	.0313	
57		.0243	.0249	.0254	.0260	.0266	.0271	.0277	.0282	.0288	.0294	.0299	.0305	.0310	
6 0		.0241	.0247	.0252	.0258	.0263	.0269	.0275	.0280	.0286	.0291	.0297	.0302	.0308	
3		.0239	.0245	.0250	.0256	.0261	.0267	.0272	.0278	.0283	.0289	.0294	.0300	.0305	
6		.0237	.0243	.0248	.0254	.0259	.0265	.0270	.0275	.0281	.0286	.0292	.0297	.0302	
9		.0235	.0241	.0246	.0252	.0257	.0262	.0268	.0273	.0279	.0284	.0289	.0295	.0300	
12		.0233	.0239	.0244	.0249	.0255	.0260	.0266	.0271	.0276	.0282	.0287	.0292	.0298	
6 15		.0231	.0237	.0242	.0247	.0253	.0258	.0263	.0269	.0274	.0279	.0285	.0290	.0295	
18		.0230	.0235	.0240	.0245	.0251	.0256	.0261	.0267	.0272	.0277	.0282	.0288	.0293	
21		.0228	.0233	.0238	.0243	.0249	.0254	.0259	.0264	.0270	.0275	.0280	.0285	.0290	
24		.0226	.0231	.0236	.0242	.0247	.0252	.0257	.0262	.0267	.0273	.0278	.0283	.0288	
27			.0229	.0234	.0240	.0245	.0250	.0255	.0260	.0265	.0271	.0276	.0281	.0286	.0291
6 30			.0227	.0233	.0238	.0243	.0248	.0253	.0258	.0263	.0268	.0274	.0279	.0284	.0289
33			.0226	.0231	.0236	.0241	.0246	.0251	.0256	.0261	.0266	.0271	.0276	.0281	.0287
36			.0224	.0229	.0234	.0239	.0244	.0249	.0254	.0259	.0264	.0269	.0274	.0279	.0284
39			.0222	.0227	.0232	.0237	.0242	.0247	.0252	.0257	.0262	.0267	.0272	.0277	.0282
42			.0220	.0225	.0230	.0235	.0240	.0245	.0250	.0255	.0260	.0265	.0270	.0275	.0280
6 45			.0219	.0224	.0229	.0234	.0239	.0244	.0248	.0253	.0258	.0263	.0268	.0273	.0278
48			.0217	.0222	.0227	.0232	.0237	.0242	.0247	.0251	.0256	.0261	.0266	.0271	.0276
51			.0216	.0220	.0225	.0230	.0235	.0240	.0245	.0250	.0254	.0259	.0264	.0269	.0274
54			.0214	.0219	.0224	.0228	.0233	.0238	.0243	.0248	.0253	.0257	.0262	.0267	.0272
57			.0212	.0217	.0222	.0227	.0232	.0236	.0241	.0246	.0251	.0255	.0260	.0265	.0270
7 0			.0211	.0216	.0220	.0225	.0230	.0235	.0239	.0244	.0249	.0254	.0258	.0263	.0268
3			.0209	.0214	.0219	.0223	.0228	.0233	.0238	.0242	.0247	.0252	.0256	.0261	.0266
6			.0208	.0212	.0217	.0222	.0227	.0231	.0236	.0241	.0245	.0250	.0255	.0259	.0264
9				.0211	.0216	.0220	.0225	.0230	.0234	.0239	.0243	.0248	.0253	.0257	.0262
12				.0209	.0214	.0219	.0223	.0228	.0232	.0237	.0242	.0246	.0251	.0255	.0260
7 15				.0208	.0212	.0217	.0222	.0226	.0231	.0235	.0240	.0245	.0249	.0254	.0258
18				.0206	.0211	.0216	.0220	.0225	.0229	.0234	.0238	.0243	.0247	.0252	.0256
21				.0205	.0209	.0214	.0219	.0223	.0228	.0232	.0237	.0241	.0246	.0250	.0255
24				.0204	.0208	.0213	.0217	.0222	.0226	.0230	.0235	.0239	.0244	.0248	.0253
27				.0202	.0207	.0211	.0216	.0220	.0224	.0229	.0233	.0238	.0242	.0247	.0251
7 30				.0201	.0205	.0210	.0214	.0218	.0223	.0227	.0232	.0236	.0241	.0245	.0249
33				.0199	.0204	.0208	.0213	.0217	.0221	.0226	.0230	.0234	.0239	.0243	.0248
36				.0198	.0202	.0207	.0211	.0215	.0220	.0224	.0229	.0233	.0237	.0242	.0246
39				.0197	.0201	.0205	.0210	.0214	.0218	.0223	.0227	.0231	.0236	.0240	.0244
42				.0195	.0200	.0204	.0208	.0213	.0217	.0221	.0225	.0230	.0234	.0238	.0243
7 45				.0194	.0198	.0203	.0207	.0211	.0215	.0220	.0224	.0228	.0232	.0237	.0241
48				.0193	.0197	.0201	.0205	.0210	.0214	.0218	.0222	.0227	.0231	.0235	.0239
51				.0191	.0196	.0200	.0204	.0208	.0213	.0217	.0221	.0225	.0229	.0234	.0238
54				.0190	.0194	.0198	.0203	.0207	.0211	.0215	.0219	.0224	.0228	.0232	.0236
57				.0189	.0193	.0197	.0201	.0206	.0210	.0214	.0218	.0222	.0226	.0230	.0235
8 0				.0188	.0192	.0196	.0200	.0204	.0208	.0212	.0217	.0221	.0225	.0229	.0233

# TABLE V. LOG. D.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.	REDUCED PARALLAX AND REFRACTION OF D.														
	45'	46'	47'	48'	49'	50'	51'	52'	53'	54'	55'	56'	57'	58'	
8° 0'	.0192	.0196	.0200	.0204	.0208	.0212	.0217	.0221	.0225	.0229	.0233	.0237			
5	.0190	.0194	.0198	.0202	.0206	.0210	.0214	.0218	.0222	.0227	.0231	.0235			
10	.0188	.0192	.0196	.0200	.0204	.0208	.0212	.0216	.0220	.0224	.0228	.0232			
15	.0186	.0190	.0194	.0198	.0202	.0206	.0210	.0214	.0218	.0222	.0226	.0230			
20	.0184	.0188	.0192	.0196	.0200	.0204	.0207	.0211	.0215	.0219	.0223	.0227			
25	.0182	.0186	.0190	.0194	.0197	.0201	.0205	.0209	.0213	.0217	.0221	.0225			
8 30	.0180	.0184	.0188	.0192	.0195	.0199	.0203	.0207	.0211	.0215	.0219	.0223			
35	.0178	.0182	.0186	.0190	.0193	.0197	.0201	.0205	.0209	.0213	.0216	.0220			
40	.0176	.0180	.0184	.0188	.0191	.0195	.0199	.0203	.0207	.0210	.0214	.0218			
45	.0174	.0178	.0182	.0186	.0189	.0193	.0197	.0201	.0205	.0208	.0212	.0216			
50	.0173	.0176	.0180	.0184	.0188	.0191	.0195	.0199	.0202	.0206	.0210	.0214			
55	.0171	.0175	.0178	.0182	.0186	.0189	.0193	.0197	.0200	.0204	.0208	.0212			
9 0	.0169	.0173	.0177	.0180	.0184	.0188	.0191	.0195	.0198	.0202	.0206	.0209			
5	.0167	.0171	.0175	.0178	.0182	.0186	.0189	.0193	.0197	.0200	.0204	.0207			
10	.0166	.0169	.0173	.0177	.0180	.0184	.0187	.0191	.0195	.0198	.0202	.0205			
15	.0164	.0168	.0171	.0175	.0179	.0182	.0186	.0189	.0193	.0196	.0200	.0203			
20	.0163	.0166	.0170	.0173	.0177	.0180	.0184	.0187	.0191	.0194	.0198	.0201			
25	.0161	.0165	.0168	.0172	.0175	.0179	.0182	.0186	.0189	.0193	.0196	.0199			
9 30		.0163	.0166	.0170	.0173	.0177	.0180	.0184	.0187	.0191	.0194	.0198			
35		.0161	.0165	.0168	.0172	.0175	.0179	.0182	.0185	.0189	.0192	.0196			
40		.0160	.0163	.0167	.0170	.0174	.0177	.0180	.0184	.0187	.0191	.0194			
45		.0158	.0162	.0165	.0169	.0172	.0175	.0179	.0182	.0185	.0189	.0192	.0195		
50		.0157	.0160	.0164	.0167	.0170	.0174	.0177	.0180	.0184	.0187	.0190	.0194		
55		.0156	.0159	.0162	.0165	.0169	.0172	.0175	.0179	.0182	.0185	.0189	.0192		
10 0		.0154	.0157	.0161	.0164	.0167	.0171	.0174	.0177	.0180	.0184	.0187	.0190		
5		.0153	.0156	.0159	.0162	.0166	.0169	.0172	.0175	.0179	.0182	.0185	.0188		
10		.0151	.0155	.0158	.0161	.0164	.0167	.0171	.0174	.0177	.0180	.0183	.0187		
15		.0150	.0153	.0156	.0160	.0163	.0166	.0169	.0172	.0175	.0179	.0182	.0185		
20		.0149	.0152	.0155	.0158	.0161	.0164	.0168	.0171	.0174	.0177	.0180	.0183		
25		.0147	.0150	.0154	.0157	.0160	.0163	.0166	.0169	.0172	.0175	.0179	.0182		
10 30		.0146	.0149	.0152	.0155	.0158	.0162	.0165	.0168	.0171	.0174	.0177	.0180		
35		.0145	.0148	.0151	.0154	.0157	.0160	.0163	.0166	.0169	.0172	.0175	.0179		
40		.0143	.0147	.0150	.0153	.0156	.0159	.0162	.0165	.0168	.0171	.0174	.0177		
45		.0142	.0145	.0148	.0151	.0154	.0157	.0160	.0163	.0166	.0169	.0172	.0175		
50		.0141	.0144	.0147	.0150	.0153	.0156	.0159	.0162	.0165	.0168	.0171	.0174		
55		.0140	.0143	.0146	.0149	.0152	.0155	.0158	.0161	.0164	.0167	.0170	.0172		
11 0		.0139	.0142	.0145	.0147	.0150	.0153	.0156	.0159	.0162	.0165	.0168	.0171		
5		.0137	.0140	.0143	.0146	.0149	.0152	.0155	.0158	.0161	.0164	.0167	.0170		
10			.0139	.0142	.0145	.0148	.0151	.0154	.0157	.0159	.0162	.0165	.0168		
15			.0138	.0141	.0144	.0147	.0150	.0152	.0155	.0158	.0161	.0164	.0167		
20			.0137	.0140	.0143	.0145	.0148	.0151	.0154	.0157	.0160	.0163	.0165		
25			.0136	.0139	.0141	.0144	.0147	.0150	.0153	.0156	.0158	.0161	.0164		
11 30			.0135	.0137	.0140	.0143	.0146	.0149	.0151	.0154	.0157	.0160	.0163		
35			.0133	.0136	.0139	.0142	.0145	.0147	.0150	.0153	.0156	.0159	.0161		
40			.0132	.0135	.0138	.0141	.0143	.0146	.0149	.0152	.0154	.0157	.0160		
45			.0131	.0134	.0137	.0140	.0142	.0145	.0148	.0150	.0153	.0156	.0159		
50			.0130	.0133	.0136	.0138	.0141	.0144	.0147	.0149	.0152	.0155	.0157		
55			.0129	.0132	.0135	.0137	.0140	.0143	.0145	.0148	.0151	.0153	.0156		
12 0			.0128	.0131	.0134	.0136	.0139	.0142	.0144	.0147	.0150	.0152	.0155		
5			.0127	.0130	.0132	.0135	.0138	.0140	.0143	.0146	.0148	.0151	.0154		
10			.0126	.0129	.0131	.0134	.0137	.0139	.0142	.0145	.0147	.0150	.0152		
15			.0125	.0128	.0130	.0133	.0136	.0138	.0141	.0143	.0146	.0149	.0151		
20			.0124	.0127	.0129	.0132	.0135	.0137	.0140	.0142	.0145	.0147	.0150		
25			.0123	.0126	.0128	.0131	.0133	.0136	.0139	.0141	.0144	.0146	.0149		
12 30			.0122	.0125	.0127	.0130	.0132	.0135	.0138	.0140	.0143	.0145	.0148		
35			.0121	.0124	.0126	.0129	.0131	.0134	.0136	.0139	.0141	.0144	.0147		
40			.0120	.0123	.0125	.0128	.0130	.0133	.0135	.0138	.0140	.0143	.0145		
45			.0119	.0122	.0124	.0127	.0129	.0132	.0134	.0137	.0139	.0142	.0144	.0147	
50			.0118	.0121	.0123	.0126	.0128	.0131	.0133	.0136	.0138	.0141	.0143	.0146	
55			.0118	.0120	.0123	.0125	.0127	.0130	.0132	.0135	.0137	.0140	.0142	.0145	
13 0			.0117	.0119	.0122	.0124	.0126	.0129	.0131	.0134	.0136	.0139	.0141	.0143	



# TABLE V. LOG. D.

Logs. A, B, C, and D, for Lunars.

App. Alt. of D.	REDUCED PARALLAX AND REFRACTION OF D.															
	47'	48'	49'	50'	51'	52'	53'	54'	55'	56'	57'	58'	59'			
13° 0'	.0117	0119	0122	0124	0126	0129	0131	0134	0136	0139	0141	0143				
10	.0115	0117	0120	0122	0125	0127	0129	0132	0134	0137	0139	0141				
20	.0113	0116	0118	0120	0123	0125	0127	0130	0132	0134	0137	0139				
30	.0112	0114	0116	0119	0121	0123	0125	0128	0130	0132	0135	0137				
40		0112	0114	0117	0119	0121	0124	0126	0128	0131	0133	0135				
50		0111	0113	0115	0117	0120	0122	0124	0126	0129	0131	0133				
14 0		0109	0111	0113	0116	0118	0120	0122	0125	0127	0129	0131				
10		0107	0110	0112	0114	0116	0118	0121	0123	0125	0127	0129				
20		0106	0108	0110	0112	0114	0117	0119	0121	0123	0125	0127				
30		0104	0106	0109	0111	0113	0115	0117	0119	0121	0123	0126				
40		0103	0105	0107	0109	0111	0113	0115	0118	0120	0122	0124				
50		0101	0103	0106	0108	0110	0112	0114	0116	0118	0120	0122				
15 0		0100	0102	0104	0106	0108	0110	0112	0114	0116	0118	0120				
10		0099	0101	0103	0105	0107	0109	0111	0113	0115	0117	0119				
20		0097	0099	0101	0103	0105	0107	0109	0111	0113	0115	0117				
30		0096	0098	0100	0102	0104	0106	0108	0110	0112	0113	0115				
40		0094	0096	0098	0100	0102	0104	0106	0108	0110	0112	0114				
50		0093	0095	0097	0099	0101	0103	0105	0107	0108	0110	0112				
16 0		0092	0094	0096	0098	0099	0101	0103	0105	0107	0109	0111				
10		0091	0093	0094	0096	0098	0100	0102	0104	0106	0107	0109				
20		0089	0091	0093	0095	0097	0099	0100	0102	0104	0106	0108				
30		0088	0090	0092	0094	0096	0097	0099	0101	0103	0105	0106				
40		0087	0089	0091	0092	0094	0096	0098	0100	0101	0103	0105				
50		0086	0088	0089	0091	0093	0095	0096	0098	0100	0102	0104				
17 0		0085	0087	0088	0090	0092	0093	0095	0097	0099	0100	0102				
10		0084	0085	0087	0089	0091	0092	0094	0096	0097	0099	0101				
20		0083	0084	0086	0088	0089	0091	0093	0094	0096	0098	0099				
30			0083	0085	0086	0088	0090	0091	0093	0095	0096	0098				
40			0082	0084	0085	0087	0089	0090	0092	0094	0095	0097				
50			0081	0083	0084	0086	0087	0089	0091	0092	0094	0096				
18 0			0080	0082	0083	0085	0086	0088	0090	0091	0093	0094				
20			0078	0079	0081	0083	0084	0086	0087	0089	0090	0092	0093			
40			0076	0077	0079	0080	0082	0083	0085	0087	0088	0090	0091			
19 0			0074	0075	0077	0078	0080	0081	0083	0084	0086	0087	0089			
20			0072	0073	0075	0076	0078	0079	0081	0082	0084	0085	0086			
40			0070	0072	0073	0074	0076	0077	0079	0080	0081	0083	0084			
20 0			0068	0070	0071	0073	0074	0075	0077	0078	0079	0081	0082			
20			0067	0068	0069	0071	0072	0073	0075	0076	0077	0079	0080			
40			0065	0066	0068	0069	0070	0072	0073	0074	0075	0077	0078			
21 0			0063	0065	0066	0067	0068	0070	0071	0072	0074	0075	0076			
20			0062	0063	0064	0065	0067	0068	0069	0070	0072	0073	0074			
40			0060	0061	0063	0064	0065	0066	0067	0069	0070	0071	0072			
22 0			0059	0060	0061	0062	0063	0065	0066	0067	0068	0069	0070			
20			0057	0058	0059	0061	0062	0063	0064	0065	0066	0068	0069			
40			0056	0057	0058	0059	0060	0061	0062	0064	0065	0066	0067			
23 0			0054	0055	0057	0058	0059	0060	0061	0062	0063	0064	0065			
20			0053	0054	0055	0056	0057	0058	0059	0060	0061	0063	0064			
40			0052	0053	0054	0055	0056	0057	0058	0059	0060	0061	0062			
24 0			0050	0051	0052	0053	0054	0055	0056	0057	0058	0059	0060			
20				0050	0051	0052	0053	0054	0055	0056	0057	0058	0059			
40				0049	0050	0051	0052	0053	0053	0054	0055	0056	0057			
25 0				0047	0048	0049	0050	0051	0052	0053	0054	0055	0056			
20				0046	0047	0048	0049	0050	0051	0052	0053	0053	0054			
40				0045	0046	0047	0048	0049	0049	0050	0051	0052	0053			
26 0				0044	0045	0046	0046	0047	0048	0049	0050	0051	0052			
20				0043	0043	0044	0045	0046	0047	0048	0048	0049	0050			
40				0041	0042	0043	0044	0045	0046	0046	0047	0048	0049			
27 0				0040	0041	0042	0043	0044	0044	0045	0046	0047	0047			
20				0039	0040	0041	0042	0042	0043	0044	0045	0045	0046			
40				0038	0039	0040	0040	0041	0042	0043	0043	0044	0045			
28 0				0037	0038	0039	0039	0040	0041	0042	0042	0043	0044			



# TABLE V. LOG. D.

Logs. A, B, C, and D, for Lunars.

Apparent Altitude of D.	REDUCED PARALLAX AND REFRACTION OF D.										
	50'	51'	52'	53'	54'	55'	56'	57'	58'	59'	60'
28° 0'	0.0037	0.0038	0.0039	0.0039	0.0040	0.0041	0.0042	0.0042	0.0043	0.0044	
30	0.0036	0.0036	0.0037	0.0038	0.0038	0.0039	0.0040	0.0040	0.0041	0.0042	
29 0	0.0034	0.0035	0.0035	0.0036	0.0037	0.0037	0.0038	0.0039	0.0039	0.0040	
30	0.0033	0.0033	0.0034	0.0035	0.0035	0.0036	0.0036	0.0037	0.0038	0.0038	
30 0	0.0031	0.0032	0.0032	0.0033	0.0034	0.0034	0.0035	0.0035	0.0036	0.0037	
30	0.0030	0.0030	0.0031	0.0031	0.0032	0.0033	0.0033	0.0034	0.0034	0.0035	
31 0	0.0028	0.0029	0.0029	0.0030	0.0031	0.0031	0.0032	0.0032	0.0033	0.0033	
30	0.0027	0.0028	0.0028	0.0029	0.0029	0.0030	0.0030	0.0031	0.0031	0.0032	0.0032
32 0	0.0026	0.0026	0.0027	0.0027	0.0028	0.0028	0.0029	0.0029	0.0030	0.0030	0.0031
30	0.0024	0.0025	0.0025	0.0026	0.0026	0.0027	0.0027	0.0028	0.0028	0.0029	0.0029
33 0	0.0023	0.0024	0.0024	0.0025	0.0025	0.0025	0.0026	0.0026	0.0027	0.0027	0.0028
30	0.0022	0.0022	0.0023	0.0023	0.0024	0.0024	0.0025	0.0025	0.0025	0.0026	0.0026
34 0	0.0021	0.0021	0.0022	0.0022	0.0022	0.0023	0.0023	0.0024	0.0024	0.0024	0.0025
30	0.0020	0.0020	0.0020	0.0021	0.0021	0.0022	0.0022	0.0022	0.0023	0.0023	0.0023
35 0	0.0018	0.0019	0.0019	0.0020	0.0020	0.0020	0.0021	0.0021	0.0021	0.0022	0.0022
30	0.0017	0.0018	0.0018	0.0018	0.0019	0.0019	0.0019	0.0020	0.0020	0.0020	0.0021
36 0	0.0016	0.0017	0.0017	0.0017	0.0018	0.0018	0.0018	0.0019	0.0019	0.0019	0.0019
30	0.0015	0.0016	0.0016	0.0016	0.0016	0.0017	0.0017	0.0017	0.0018	0.0018	0.0018
37 0	0.0014	0.0014	0.0015	0.0015	0.0015	0.0016	0.0016	0.0016	0.0016	0.0017	0.0017
30	0.0013	0.0013	0.0014	0.0014	0.0014	0.0014	0.0015	0.0015	0.0015	0.0015	0.0016
38 0	0.0012	0.0012	0.0013	0.0013	0.0013	0.0013	0.0014	0.0014	0.0014	0.0014	0.0014
30	0.0011	0.0011	0.0012	0.0012	0.0012	0.0012	0.0012	0.0013	0.0013	0.0013	0.0013
39 0	0.0010	0.0010	0.0011	0.0011	0.0011	0.0011	0.0011	0.0012	0.0012	0.0012	0.0012
30	0.0009	0.0010	0.0010	0.0010	0.0010	0.0010	0.0010	0.0010	0.0011	0.0011	0.0011
40	0.0008	0.0009	0.0009	0.0009	0.0009	0.0009	0.0009	0.0009	0.0010	0.0010	0.0010
41	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007	0.0008	0.0008
42	0.0005	0.0005	0.0005	0.0005	0.0005	0.0005	0.0005	0.0005	0.0005	0.0005	0.0006
43	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0004
44	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0002	0.0002	0.0002	0.0002
45	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
46	9.9998	9.9998	9.9998	9.9998	9.9998	9.9998	9.9998	9.9998	9.9998	9.9998	9.9998
47	9.9997	9.9997	9.9997	9.9997	9.9997	9.9996	9.9996	9.9996	9.9996	9.9996	9.9996
48	9.9995	9.9995	9.9995	9.9995	9.9995	9.9995	9.9995	9.9995	9.9995	9.9994	9.9994
49	9.9994	9.9994	9.9994	9.9994	9.9993	9.9993	9.9993	9.9993	9.9993	9.9993	9.9993
50	9.9992	9.9992	9.9992	9.9992	9.9992	9.9992	9.9992	9.9992	9.9991	9.9991	9.9991
51	9.9991	9.9991	9.9991	9.9991	9.9990	9.9990	9.9990	9.9990	9.9990	9.9990	9.9990
52	9.9990	9.9990	9.9990	9.9989	9.9989	9.9989	9.9989	9.9989	9.9989	9.9988	9.9988
53	9.9989	9.9988	9.9988	9.9988	9.9988	9.9988	9.9988	9.9987	9.9987	9.9987	9.9987
54	9.9988	9.9987	9.9987	9.9987	9.9987	9.9987	9.9986	9.9986	9.9986	9.9986	9.9985
55	9.9986	9.9986	9.9986	9.9986	9.9986	9.9985	9.9985	9.9985	9.9984	9.9984	9.9984
56	9.9985	9.9985	9.9985	9.9984	9.9984	9.9984	9.9984	9.9984	9.9983	9.9983	9.9983
57	9.9984	9.9984	9.9984	9.9983	9.9983	9.9983	9.9983	9.9982	9.9982	9.9982	9.9981
58	9.9983	9.9983	9.9983	9.9982	9.9982	9.9982	9.9982	9.9981	9.9981	9.9981	9.9980
59	9.9982	9.9982	9.9981	9.9981	9.9981	9.9981	9.9980	9.9980	9.9980	9.9979	9.9979
60	9.9981	9.9981	9.9980	9.9980	9.9980	9.9980	9.9979	9.9979	9.9979	9.9978	9.9978
61	9.9980	9.9980	9.9980	9.9979	9.9979	9.9979	9.9978	9.9978	9.9978	9.9977	9.9977
62	9.9979	9.9979	9.9979	9.9978	9.9978	9.9978	9.9977	9.9977	9.9977	9.9976	9.9976
63	9.9979	9.9978	9.9978	9.9977	9.9977	9.9977	9.9976	9.9976	9.9976	9.9975	9.9975
64	9.9978	9.9977	9.9977	9.9976	9.9976	9.9976	9.9976	9.9975	9.9975	9.9974	9.9974
65	9.9977	9.9977	9.9976	9.9976	9.9976	9.9975	9.9975	9.9974	9.9974	9.9973	9.9972
66	9.9976	9.9976	9.9975	9.9975	9.9975	9.9974	9.9974	9.9973	9.9973	9.9973	9.9972
67	9.9976	9.9975	9.9975	9.9974	9.9974	9.9974	9.9973	9.9973	9.9972	9.9972	9.9971
68	9.9975	9.9974	9.9974	9.9973	9.9973	9.9973	9.9972	9.9972	9.9971	9.9971	9.9970
69	9.9974	9.9974	9.9973	9.9973	9.9973	9.9972	9.9972	9.9971	9.9971	9.9970	9.9970
70	9.9974	9.9973	9.9973	9.9972	9.9972	9.9972	9.9971	9.9970	9.9970	9.9969	9.9969
72	9.9972	9.9972	9.9971	9.9971	9.9970	9.9970	9.9970	9.9969	9.9969	9.9968	9.9968
74	9.9971	9.9971	9.9970	9.9970	9.9969	9.9969	9.9969	9.9968	9.9968	9.9967	9.9966
76	9.9971	9.9970	9.9969	9.9969	9.9968	9.9968	9.9968	9.9967	9.9966	9.9966	9.9965
78	9.9970	9.9969	9.9969	9.9968	9.9968	9.9967	9.9967	9.9966	9.9966	9.9965	9.9964
80	9.9969	9.9969	9.9968	9.9968	9.9967	9.9967	9.9966	9.9965	9.9965	9.9964	9.9964
90	9.9968	9.9967	9.9966	9.9966	9.9966	9.9965	9.9964	9.9964	9.9963	9.9963	9.9962

# TABLE VI.

Second Correction of the Lunar Distance.

## FIRST CORRECTION OF DISTANCE.

Appar- ent Dis- tance.	3	7	10	12	14	16	18	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	Appar- ent Dis- tance.
Subtr.	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	Add.
15° 0'	0	2	3	5	6	8	11	13	14	16	17	19	20	22	24	26	27	29	33	33	35	38	40	42	
30	0	2	3	5	6	8	10	13	14	15	17	18	20	21	23	25	26	28	32	32	34	36	39	41	
16 0	0	1	3	4	6	8	10	12	13	15	16	18	19	21	22	24	26	27	31	31	33	35	37	39	
30	0	1	3	4	6	8	10	12	13	14	16	17	18	20	21	23	25	27	30	30	32	34	36	38	
17 0	0	1	3	4	6	7	9	11	13	14	15	16	18	19	21	22	24	26	29	29	31	33	35	37	
30	0	1	3	4	5	7	9	11	12	13	15	16	17	19	20	22	23	25	28	28	30	32	34	36	
18 0	0	1	3	4	5	7	9	11	12	13	14	15	17	18	20	21	23	24	28	28	29	31	33	35	
30	0	1	3	4	5	7	8	10	12	13	14	15	16	18	19	20	22	23	27	27	28	30	32	34	
19 0	0	1	3	4	5	6	8	10	11	12	13	15	16	17	18	20	21	23	26	26	28	29	31	33	
30	0	1	2	4	5	6	8	10	11	12	13	14	15	17	18	19	21	22	25	25	27	28	30	32	
20 0	0	1	2	3	5	6	8	10	11	12	13	14	15	16	17	19	20	22	25	25	26	28	29	31	
21	0	1	2	3	4	6	7	9	10	11	12	13	14	15	17	18	19	20	23	23	25	26	28	29	
22	0	1	2	3	4	6	7	9	10	10	11	12	14	15	16	17	18	19	22	22	24	25	26	28	
23	0	1	2	3	4	5	7	8	9	10	11	12	13	14	15	16	17	19	21	21	22	24	25	27	
24	0	1	2	3	4	5	6	8	9	9	10	11	12	13	14	15	16	18	20	20	21	23	24	25	
25	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	19	19	20	22	23	24	
26	0	1	2	3	4	5	6	7	8	9	9	10	11	12	13	14	15	16	18	18	19	21	22	23	
27	0	1	2	2	3	4	6	7	8	8	9	10	11	12	13	14	15	18	18	19	20	21	22	23	
28	0	1	2	2	3	4	5	7	7	8	9	9	10	11	12	13	14	15	17	17	18	19	20	21	
29	0	1	2	2	3	4	5	6	7	8	8	9	10	11	11	12	13	14	16	16	17	18	19	20	
30	0	1	2	2	3	4	5	6	7	7	8	9	9	10	11	12	13	14	15	15	16	17	19	20	
31	0	1	1	2	3	4	5	6	6	7	8	8	9	10	11	11	12	13	15	15	16	17	18	19	
32	0	1	1	2	3	4	5	6	6	7	7	8	9	9	10	11	12	13	14	14	15	16	17	18	
33	0	1	1	2	3	3	4	5	6	7	7	8	8	9	10	11	11	12	14	14	15	16	16	17	
34	0	1	1	2	3	3	4	5	6	6	7	7	8	9	9	10	11	12	13	13	14	15	16	17	
35	0	1	1	2	2	3	4	5	5	6	7	7	8	8	9	10	10	11	13	13	14	14	15	16	
36	0	1	1	2	2	3	4	5	5	6	6	7	8	8	9	9	10	11	12	12	13	14	15	16	
37	0	1	1	2	2	3	4	5	5	6	6	7	7	8	8	9	10	10	12	12	13	13	14	15	
38	0	1	1	2	2	3	4	4	5	5	6	6	7	8	8	9	9	10	11	11	12	13	14	14	
39	0	1	1	2	2	3	3	4	5	5	6	6	7	7	8	8	9	10	11	11	12	12	13	14	
40	0	1	1	2	2	3	3	4	5	5	6	6	7	7	8	8	9	9	11	11	11	12	13	13	140°
42	0	0	1	1	2	2	3	4	4	5	5	6	6	7	7	8	8	9	10	10	11	11	12	13	138
44	0	0	1	1	2	2	3	4	4	4	5	5	6	6	7	7	8	8	9	9	10	10	11	12	136
46	0	0	1	1	2	2	3	3	4	4	4	5	5	6	6	7	7	8	9	9	9	10	10	11	134
48	0	0	1	1	2	2	3	3	3	4	4	5	5	5	6	6	7	7	8	8	9	9	10	10	132
50	0	0	1	1	1	2	2	3	3	4	4	4	5	5	5	6	6	7	8	8	8	8	9	9	130
52	0	0	1	1	1	2	2	3	3	3	4	4	4	5	5	5	6	6	7	7	7	8	8	9	128
54	0	0	1	1	1	2	2	3	3	3	3	4	4	4	5	5	5	6	6	6	7	7	8	8	126
56	0	0	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	6	6	6	7	7	8	124
58	0	0	1	1	1	1	2	2	2	2	3	3	3	4	4	4	5	5	6	6	6	6	7	7	122
60	0	0	0	1	1	1	2	2	2	2	3	3	3	3	4	4	4	5	5	5	5	6	6	7	120
62	0	0	0	1	1	1	2	2	2	2	2	3	3	3	3	4	4	4	5	5	5	5	6	6	118
64	0	0	0	1	1	1	1	2	2	2	2	2	3	3	3	3	4	4	4	4	5	5	5	6	116
66	0	0	0	1	1	1	1	2	2	2	2	2	2	3	3	3	3	4	4	4	4	4	5	5	114
68	0	0	0	1	1	1	1	1	2	2	2	2	2	2	3	3	3	3	3	4	4	4	4	5	112
70	0	0	0	0	1	1	1	1	1	2	2	2	2	2	2	2	3	3	3	3	3	4	4	4	110
74	0	0	0	0	0	1	1	1	1	1	1	1	1	2	2	2	2	2	3	3	3	3	3	3	106
78	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2	102
82	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	98
86	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	94
90°	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	90°
Appar- ent Dis- tance.	3	7	10	12	14	16	18	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	Appar- ent Dis- tance.
FIRST CORRECTION OF DISTANCE.																									

### Second Correction of the Lunar Distance.

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# TABLE VII.

For finding the Correction of the Lunar Distance for the Contraction of the Moon's Semidiameter.

TABLE VII. A. GIVING THE ARGUMENT FOR TABLE VII. B.

Reduced P. and R. of $\odot$ .	APPARENT ALTITUDE OF $\odot$ .																								
	$5^{\circ}$	$5\frac{1}{2}^{\circ}$	$6^{\circ}$	$6\frac{1}{2}^{\circ}$	$7^{\circ}$	$7\frac{1}{2}^{\circ}$	$8^{\circ}$	$8\frac{1}{2}^{\circ}$	$9^{\circ}$	$9\frac{1}{2}^{\circ}$	$10^{\circ}$	$11^{\circ}$	$12^{\circ}$	$13^{\circ}$	$14^{\circ}$	$15^{\circ}$	$16^{\circ}$	$17^{\circ}$	$18^{\circ}$	$20^{\circ}$	$25^{\circ}$	$30^{\circ}$	$40^{\circ}$	$50^{\circ}$	
41'	65	56																							
42	63	54	47	41																					
43	62	53	46	40	35																				
44	60	51	45	39	34	30	27																		
45	58	50	43	38	33	30	26	24	21	20															
46	57	49	42	37	33	29	26	23	21	19	17	15													
47	56	48	41	36	32	28	25	23	20	19	17	14	12	10											
48	54	46	40	35	31	28	25	22	20	18	17	14	12	10	9	8		7	6						
49	53	45	39	35	30	27	24	22	19	18	16	14	12	10	9	8	7	7	6	6	5	3			
50	52	44	38	34	30	26	24	21	19	17	16	13	11	10	9	8	7	6	5	5	3	3	2		
51	50	43	38	33	29	26	23	21	19	17	15	13	11	10	8	7	7	6	5	5	3	2	2	2	
52	49	42	37	32	28	25	23	20	18	17	15	13	11	9	8	7	7	6	5	4	3	2	2	2	
53	48	41	36	32	28	25	22	20	18	16	15	12	11	9	8	7	6	6	5	4	3	2	2	2	
54	47	41	35	31	27	24	22	19	18	16	15	12	10	9	8	7	6	6	5	4	3	2	2	2	
55			35	30	27	24	21	19	17	16	14	12	10	9	8	7	6	6	5	4	3	2	2	2	
56					26	23	21	19	17	15	14	12	10	9	8	7	6	5	5	4	3	2	2	2	
57								18	17	15	14	12	10	9	7	7	6	5	5	4	3	2	2	2	
58											13	11	10	8	7	7	6	5	5	4	3	2	2	2	
59														8	7	6	6	5	5	4	3	2	2	2	
60																				4	3	2	2	2	

TABLE VII. B. CONTRACTION OF  $\odot$ 'S SEMIDIAMETER.

Whole Correction of $\odot$ .	ARGUMENT = NUMBER FROM TABLE VII. A																										
	2	4	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	44	48	52	56	60	64	
0	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"
5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
15	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	3	3	3	3
20	0	0	0	0	1	1	1	1	1	1	2	2	2	2	2	2	3	3	3	3	3	4	4	4	5	5	5
22	0	0	1	1	1	1	1	2	2	2	2	2	3	3	3	3	3	3	4	4	4	5	5	5	6	6	6
24	0	0	1	1	1	1	2	2	2	2	2	3	3	3	3	4	4	4	4	5	5	6	6	6	7	7	7
26	0	1	1	1	1	2	2	2	3	3	3	3	4	4	4	4	5	5	5	5	6	6	7	8	8	9	9
28	0	1	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	6	6	6	7	8	9	9	10	10
30	0	1	1	1	1	2	2	3	3	3	4	4	4	5	5	5	6	6	6	7	7	8	9	10	11	12	12
32	0	1	1	2	2	2	3	3	4	4	5	5	5	6	6	7	7	7	8	8	9	10	11	11	12	13	13
34	0	1	1	2	2	3	3	4	4	5	5	6	6	6	7	7	8	8	9	9	10	11	12	13	14	15	15
36	1	1	2	2	3	3	4	4	5	5	6	6	7	7	8	8	9	9	10	10	11	12	13	15	16	17	17
38	1	1	2	2	3	3	4	5	5	6	6	7	8	8	9	9	10	10	11	12	13	14	15	16	17	18	18
40	1	1	2	3	3	4	4	5	6	6	7	8	8	9	9	10	11	12	12	13	14	15	17	18	19	20	20
42	1	1	2	3	4	4	5	6	6	7	8	8	9	10	11	11	12	13	13	14	16	17	18	20	21	23	23
44	1	2	2	3	4	5	5	6	7	8	9	9	10	11	12	12	13	14	15	15	17	19	20	22	23		
45	1	2	2	3	4	5	6	6	7	8	9	10	11	11	12	13	14	15	15	16	18	19	21	23	24		
46	1	2	3	3	4	5	6	7	7	8	9	10	11	12	13	14	14	15	16	17	19	20	22	24			
47	1	2	3	4	4	5	6	7	8	9	10	11	11	12	13	14	15	16	17	18	19	21	23	25			
48	1	2	3	4	5	6	6	7	8	9	10	11	12	13	14	15	16	17	18	18	20	22	24	26			
49	1	2	3	4	5	6	7	8	9	10	11	12	12	13	14	15	16	17	18	19	21	23	25				
50	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	22	24	26				
51	1	2	3	4	5	6	7	8	9	10	11	12	14	15	16	17	18	19	20	21	23	25	27				
52	1	2	3	4	5	6	8	9	10	11	12	13	14	15	16	17	18	19	21	22	24	26					
53	1	2	3	4	6	7	8	9	10	11	12	13	15	16	17	18	19	20	21	22	25	27					
54		2	3	5	6	7	8	9	10	12	13	14	15	16	17	19	20	21	22	23	26						
55		2	4	5	6	7	8	10	11	12	13	15	16	17	18	19	21	22									
56		3	4	5	6	8	9	10	11	13	14	15	16														
57		4	5	7																							

When the nearest limb is observed subtract this correction; when the farthest, add.

# TABLE VIII.

For finding the Correction of the Lunar Distance for the Contraction of the Sun's Semidiameter.

TABLE VIII. A. GIVING THE ARGUMENT FOR TABLE VIII. B.

APPARENT ALTITUDE OF ☉.

Reduced P. and R. of ☉.	APPARENT ALTITUDE OF ☉.																									
	5	5½	6	6½	7	7½	8	8½	9	9½	10	11	12	13	14	15	16	17	18	20	25	30	40	50		
1' 0"																								22	18	
30																									24	29
2 0																				35	37	30	34	46		
30																	40	42	44	47	53	42	46			
3 0															44	46	49	51	53	57		59				
30													45	48	51	54	57	60	62	67						
4 0											45	49	52	55	59	62	65	68								
30									47	49	51	55	59	63	66	70										
5 0							47	50	52	54	57	61	66	70	74											
30					47	50	52	55	57	60	62	67	72													
6 0				49	52	55	57	60	63	66	68	74														
30			50	53	56	59	62	65	68	71	74															
7 0		51	54	58	61	64	67	70	74																	
30		55	58	62	65	69	72	75																		
8 0	55	59	62	66	70	73	77																			
30	59	63	66	70	74	78																				
9 0	62	66	70	74	79																					
30	66	70	74	79																						
10 0	69	74	78																							
30	73	77																								
11 0	76	81																								
30	80																									

TABLE VIII. B. CONTRACTION OF ☉'S SEMIDIAMETER.

ARGUMENT = NUMBER FROM TABLE VIII. A.

Whole Correction of ☉.	20	24	28	32	36	40	44	46	48	50	52	54	56	58	60	62	64	66	68	70	72	74	76	78
0 0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
1 0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
2 0			2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
30					3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
3 0						4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4
30							5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
4 0							6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
20							7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
40								9	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8
5 0								10	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
20									11	10	10	9	9	9	9	9	9	9	9	9	9	9	9	9
40									12	12	11	11	10	10	10	9	9	9	9	9	9	9	9	9
6 0										13	12	12	12	12	11	11	10	10	10	10	10	10	10	10
20										14	14	13	13	12	12	12	11	11	11	11	10	10	10	9
40										16	15	15	14	14	13	13	13	12	12	11	11	11	11	10
7 0										18	17	16	16	15	15	14	14	13	13	13	12	12	12	11
20											19	18	17	17	16	16	15	15	14	14	13	13	13	12
40											20	19	18	18	17	17	16	16	15	15	14	14	14	13
8 0											21	21	20	20	19	19	18	18	17	17	17	16	16	15
20													22	21	20	20	20	19	18	18	17	17	16	16
40														23	23	22	21	20	20	19	18	18	17	16
9 0																24	23	22	21	20	20	20	19	19
20																	25	24	23	22	21	21	21	20
40																		25	25	24	23	23	22	22
10 0																				26	26	25	24	23
20																					28	27	26	25
40																						28	28	27
11 0																							29	28
20																								30

Subtract this correction from the distance.

# TABLE IX.

LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	<sup>0</sup>	<sup>1</sup>	<sup>2</sup>	<sup>3</sup>	<sup>4</sup>	<sup>5</sup>	<sup>6</sup>	<sup>7</sup>	<sup>8</sup>	<sup>9</sup>
0 <sup>h</sup> . 0 <sup>m</sup> . 0 <sup>s</sup> .	0.0000	0.3010	0.4771	0.6021	0.6990	0.7782	0.8451	0.9031	0.9542	
0 10	1.0000	1.0414	1.0792	1.1139	1.1461	1.1761	1.2041	1.2304	1.2553	1.2788
0 20	1.3010	1.3222	1.3424	1.3617	1.3802	1.3979	1.4150	1.4314	1.4472	1.4624
0 30	1.4771	1.4914	1.5051	1.5185	1.5315	1.5441	1.5563	1.5682	1.5798	1.5911
0 40	1.6021	1.6128	1.6232	1.6335	1.6435	1.6532	1.6628	1.6721	1.6812	1.6902
0 50	1.6990	1.7076	1.7160	1.7243	1.7324	1.7404	1.7482	1.7559	1.7634	1.7709
0 1 0	1.7782	1.7853	1.7924	1.7993	1.8062	1.8129	1.8195	1.8261	1.8325	1.8388
0 1 10	1.8451	1.8513	1.8573	1.8633	1.8692	1.8751	1.8808	1.8865	1.8921	1.8976
0 1 20	1.9031	1.9085	1.9138	1.9191	1.9243	1.9294	1.9345	1.9395	1.9445	1.9494
0 1 30	1.9542	1.9589	1.9638	1.9685	1.9731	1.9777	1.9823	1.9868	1.9912	1.9956
0 1 40	2.0000	2.0043	2.0086	2.0128	2.0170	2.0212	2.0253	2.0294	2.0334	2.0374
0 1 50	2.0414	2.0453	2.0492	2.0531	2.0569	2.0607	2.0645	2.0682	2.0719	2.0755
0 2 0	2.0792	2.0828	2.0864	2.0899	2.0934	2.0969	2.1004	2.1038	2.1072	2.1106
0 2 10	2.1139	2.1173	2.1206	2.1239	2.1271	2.1303	2.1335	2.1367	2.1399	2.1430
0 2 20	2.1461	2.1492	2.1523	2.1553	2.1584	2.1614	2.1644	2.1673	2.1703	2.1732
0 2 30	2.1761	2.1790	2.1818	2.1847	2.1875	2.1903	2.1931	2.1959	2.1987	2.2014
0 2 40	2.2041	2.2068	2.2095	2.2122	2.2148	2.2175	2.2201	2.2227	2.2253	2.2279
0 2 50	2.2304	2.2330	2.2355	2.2380	2.2405	2.2430	2.2455	2.2480	2.2504	2.2529
0 3 0	2.2553	2.2577	2.2601	2.2625	2.2648	2.2672	2.2695	2.2718	2.2742	2.2765
0 3 10	2.2788	2.2810	2.2833	2.2856	2.2878	2.2900	2.2923	2.2945	2.2967	2.2989
0 3 20	2.3010	2.3032	2.3054	2.3075	2.3096	2.3118	2.3139	2.3160	2.3181	2.3201
0 3 30	2.3222	2.3243	2.3263	2.3284	2.3304	2.3324	2.3345	2.3365	2.3385	2.3404
0 3 40	2.3424	2.3444	2.3464	2.3483	2.3502	2.3522	2.3541	2.3560	2.3579	2.3598
0 3 50	2.3617	2.3636	2.3655	2.3674	2.3692	2.3711	2.3729	2.3747	2.3766	2.3784
0 4 0	2.3802	2.3820	2.3838	2.3856	2.3874	2.3892	2.3909	2.3927	2.3945	2.3962
0 4 10	2.3979	2.3997	2.4014	2.4031	2.4048	2.4065	2.4082	2.4099	2.4116	2.4133
0 4 20	2.4150	2.4166	2.4183	2.4200	2.4216	2.4232	2.4249	2.4265	2.4281	2.4298
0 4 30	2.4314	2.4330	2.4346	2.4362	2.4378	2.4393	2.4409	2.4425	2.4440	2.4456
0 4 40	2.4472	2.4487	2.4502	2.4518	2.4533	2.4548	2.4564	2.4579	2.4594	2.4609
0 4 50	2.4624	2.4639	2.4654	2.4669	2.4683	2.4698	2.4713	2.4728	2.4742	2.4757
0 5 0	2.4771	2.4786	2.4800	2.4814	2.4829	2.4843	2.4857	2.4871	2.4886	2.4900
0 5 10	2.4914	2.4928	2.4942	2.4955	2.4969	2.4983	2.4997	2.5011	2.5024	2.5038
0 5 20	2.5051	2.5065	2.5079	2.5092	2.5105	2.5119	2.5132	2.5145	2.5159	2.5172
0 5 30	2.5185	2.5198	2.5211	2.5224	2.5237	2.5250	2.5263	2.5276	2.5289	2.5302
0 5 40	2.5315	2.5328	2.5340	2.5353	2.5366	2.5378	2.5391	2.5403	2.5416	2.5428
0 5 50	2.5441	2.5453	2.5465	2.5478	2.5490	2.5502	2.5514	2.5527	2.5539	2.5551
0 6 0	2.5563	2.5575	2.5587	2.5599	2.5611	2.5623	2.5635	2.5647	2.5658	2.5670
0 6 10	2.5682	2.5694	2.5705	2.5717	2.5729	2.5740	2.5752	2.5763	2.5775	2.5786
0 6 20	2.5798	2.5809	2.5821	2.5832	2.5843	2.5855	2.5866	2.5877	2.5888	2.5899
0 6 30	2.5911	2.5922	2.5933	2.5944	2.5955	2.5966	2.5977	2.5988	2.5999	2.6010
0 6 40	2.6021	2.6031	2.6042	2.6053	2.6064	2.6075	2.6085	2.6096	2.6107	2.6117
0 6 50	2.6128	2.6138	2.6149	2.6160	2.6170	2.6180	2.6191	2.6201	2.6212	2.6222
0 7 0	2.6232	2.6243	2.6253	2.6263	2.6274	2.6284	2.6294	2.6304	2.6314	2.6325
0 7 10	2.6335	2.6345	2.6355	2.6365	2.6375	2.6385	2.6395	2.6405	2.6415	2.6425
0 7 20	2.6435	2.6444	2.6454	2.6464	2.6474	2.6484	2.6493	2.6503	2.6513	2.6522
0 7 30	2.6532	2.6542	2.6551	2.6561	2.6571	2.6580	2.6590	2.6599	2.6609	2.6618
0 7 40	2.6628	2.6637	2.6646	2.6656	2.6665	2.6675	2.6684	2.6693	2.6702	2.6712
0 7 50	2.6721	2.6730	2.6739	2.6749	2.6758	2.6767	2.6776	2.6785	2.6794	2.6803
0 8 0	2.6812	2.6821	2.6830	2.6839	2.6848	2.6857	2.6866	2.6875	2.6884	2.6893
0 8 10	2.6902	2.6911	2.6920	2.6928	2.6937	2.6946	2.6955	2.6964	2.6972	2.6981
0 8 20	2.6990	2.6998	2.7007	2.7016	2.7024	2.7033	2.7042	2.7050	2.7059	2.7067
0 8 30	2.7076	2.7084	2.7093	2.7101	2.7110	2.7118	2.7126	2.7135	2.7143	2.7152
0 8 40	2.7160	2.7168	2.7177	2.7185	2.7193	2.7202	2.7210	2.7218	2.7226	2.7235
0 8 50	2.7243	2.7251	2.7259	2.7267	2.7275	2.7284	2.7292	2.7300	2.7308	2.7316
0 9 0	2.7324	2.7332	2.7340	2.7348	2.7356	2.7364	2.7372	2.7380	2.7388	2.7396
0 9 10	2.7404	2.7412	2.7419	2.7427	2.7435	2.7443	2.7451	2.7459	2.7466	2.7474
0 9 20	2.7482	2.7490	2.7497	2.7505	2.7513	2.7520	2.7528	2.7536	2.7543	2.7551
0 9 30	2.7559	2.7566	2.7574	2.7582	2.7589	2.7597	2.7604	2.7612	2.7619	2.7627
0 9 40	2.7634	2.7642	2.7649	2.7657	2.7664	2.7672	2.7679	2.7686	2.7694	2.7701
0 9 50	2.7709	2.7716	2.7723	2.7731	2.7738	2.7745	2.7752	2.7760	2.7767	2.7774



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0 <sup>h</sup> . 10 <sup>m</sup> . 0 <sup>s</sup> .	2.7782	2.7789	2.7796	2.7803	2.7810	2.7818	2.7825	2.7832	2.7839	2.7846
10 10	2.7853	2.7860	2.7868	2.7875	2.7882	2.7889	2.7896	2.7903	2.7910	2.7917
10 20	2.7924	2.7931	2.7938	2.7945	2.7952	2.7959	2.7966	2.7973	2.7980	2.7987
10 30	2.7993	2.8000	2.8007	2.8014	2.8021	2.8028	2.8035	2.8041	2.8048	2.8055
10 40	2.8062	2.8069	2.8075	2.8082	2.8089	2.8096	2.8102	2.8109	2.8116	2.8122
10 50	2.8129	2.8136	2.8142	2.8149	2.8156	2.8162	2.8169	2.8176	2.8182	2.8189
0 11 0	2.8195	2.8202	2.8209	2.8215	2.8222	2.8228	2.8235	2.8241	2.8248	2.8254
11 10	2.8261	2.8267	2.8274	2.8280	2.8287	2.8293	2.8299	2.8306	2.8312	2.8319
11 20	2.8325	2.8331	2.8338	2.8344	2.8351	2.8357	2.8363	2.8370	2.8376	2.8382
11 30	2.8388	2.8395	2.8401	2.8407	2.8414	2.8420	2.8426	2.8432	2.8439	2.8445
11 40	2.8451	2.8457	2.8463	2.8470	2.8476	2.8482	2.8488	2.8494	2.8500	2.8506
11 50	2.8513	2.8519	2.8525	2.8531	2.8537	2.8543	2.8549	2.8555	2.8561	2.8567
0 12 0	2.8573	2.8579	2.8585	2.8591	2.8597	2.8603	2.8609	2.8615	2.8621	2.8627
12 10	2.8633	2.8639	2.8645	2.8651	2.8657	2.8663	2.8669	2.8675	2.8681	2.8686
12 20	2.8692	2.8698	2.8704	2.8710	2.8716	2.8722	2.8727	2.8733	2.8739	2.8745
12 30	2.8751	2.8756	2.8762	2.8768	2.8774	2.8779	2.8785	2.8791	2.8797	2.8802
12 40	2.8808	2.8814	2.8820	2.8825	2.8831	2.8837	2.8842	2.8848	2.8854	2.8859
12 50	2.8865	2.8871	2.8876	2.8882	2.8887	2.8893	2.8899	2.8904	2.8910	2.8915
0 13 0	2.8921	2.8927	2.8932	2.8938	2.8943	2.8949	2.8954	2.8960	2.8965	2.8971
13 10	2.8976	2.8982	2.8987	2.8993	2.8998	2.9004	2.9009	2.9015	2.9020	2.9025
13 20	2.9031	2.9036	2.9042	2.9047	2.9053	2.9058	2.9063	2.9069	2.9074	2.9079
13 30	2.9085	2.9090	2.9096	2.9101	2.9106	2.9112	2.9117	2.9122	2.9128	2.9133
13 40	2.9138	2.9143	2.9149	2.9154	2.9159	2.9165	2.9170	2.9175	2.9180	2.9186
13 50	2.9191	2.9196	2.9201	2.9206	2.9212	2.9217	2.9222	2.9227	2.9232	2.9238
0 14 0	2.9243	2.9248	2.9253	2.9258	2.9263	2.9269	2.9274	2.9279	2.9284	2.9289
14 10	2.9294	2.9299	2.9304	2.9309	2.9315	2.9320	2.9325	2.9330	2.9335	2.9340
14 20	2.9345	2.9350	2.9355	2.9360	2.9365	2.9370	2.9375	2.9380	2.9385	2.9390
14 30	2.9395	2.9400	2.9405	2.9410	2.9415	2.9420	2.9425	2.9430	2.9435	2.9440
14 40	2.9445	2.9450	2.9455	2.9460	2.9465	2.9469	2.9474	2.9479	2.9484	2.9489
14 50	2.9494	2.9499	2.9504	2.9509	2.9513	2.9518	2.9523	2.9528	2.9533	2.9538
0 15 0	2.9542	2.9547	2.9552	2.9557	2.9562	2.9566	2.9571	2.9576	2.9581	2.9586
15 10	2.9590	2.9595	2.9600	2.9605	2.9609	2.9614	2.9619	2.9624	2.9628	2.9633
15 20	2.9638	2.9643	2.9647	2.9652	2.9657	2.9661	2.9666	2.9671	2.9675	2.9680
15 30	2.9685	2.9689	2.9694	2.9699	2.9703	2.9708	2.9713	2.9717	2.9722	2.9727
15 40	2.9731	2.9736	2.9741	2.9745	2.9750	2.9754	2.9759	2.9763	2.9768	2.9773
15 50	2.9777	2.9782	2.9786	2.9791	2.9795	2.9800	2.9805	2.9809	2.9814	2.9818
0 16 0	2.9823	2.9827	2.9832	2.9836	2.9841	2.9845	2.9850	2.9854	2.9859	2.9863
16 10	2.9868	2.9872	2.9877	2.9881	2.9886	2.9890	2.9894	2.9899	2.9903	2.9908
16 20	2.9912	2.9917	2.9921	2.9926	2.9930	2.9934	2.9939	2.9943	2.9948	2.9952
16 30	2.9956	2.9961	2.9965	2.9969	2.9974	2.9978	2.9983	2.9987	2.9991	2.9996
16 40	3.0000	3.0004	3.0009	3.0013	3.0017	3.0022	3.0026	3.0030	3.0035	3.0039
16 50	3.0043	3.0048	3.0052	3.0056	3.0060	3.0065	3.0069	3.0073	3.0077	3.0082
0 17 0	3.0086	3.0090	3.0095	3.0099	3.0103	3.0107	3.0111	3.0116	3.0120	3.0124
17 10	3.0128	3.0133	3.0137	3.0141	3.0145	3.0149	3.0154	3.0158	3.0162	3.0166
17 20	3.0170	3.0175	3.0179	3.0183	3.0187	3.0191	3.0195	3.0199	3.0204	3.0208
17 30	3.0212	3.0216	3.0220	3.0224	3.0228	3.0233	3.0237	3.0241	3.0245	3.0249
17 40	3.0253	3.0257	3.0261	3.0265	3.0269	3.0273	3.0278	3.0282	3.0286	3.0290
17 50	3.0294	3.0298	3.0302	3.0306	3.0310	3.0314	3.0318	3.0322	3.0326	3.0330
0 18 0	3.0334	3.0338	3.0342	3.0346	3.0350	3.0354	3.0358	3.0362	3.0366	3.0370
18 10	3.0374	3.0378	3.0382	3.0386	3.0390	3.0394	3.0398	3.0402	3.0406	3.0410
18 20	3.0414	3.0418	3.0422	3.0426	3.0430	3.0434	3.0438	3.0441	3.0445	3.0449
18 30	3.0453	3.0457	3.0461	3.0465	3.0469	3.0473	3.0477	3.0481	3.0484	3.0488
18 40	3.0492	3.0496	3.0500	3.0504	3.0508	3.0512	3.0515	3.0519	3.0523	3.0527
18 50	3.0531	3.0535	3.0538	3.0542	3.0546	3.0550	3.0554	3.0558	3.0561	3.0565
0 19 0	3.0569	3.0573	3.0577	3.0580	3.0584	3.0588	3.0592	3.0596	3.0599	3.0603
19 10	3.0607	3.0611	3.0615	3.0618	3.0622	3.0626	3.0630	3.0633	3.0637	3.0641
19 20	3.0645	3.0648	3.0652	3.0656	3.0660	3.0663	3.0667	3.0671	3.0674	3.0678
19 30	3.0682	3.0686	3.0689	3.0693	3.0697	3.0700	3.0704	3.0708	3.0711	3.0715
19 40	3.0719	3.0722	3.0726	3.0730	3.0734	3.0737	3.0741	3.0745	3.0748	3.0752
19 50	3.0755	3.0759	3.0763	3.0766	3.0770	3.0774	3.0777	3.0781	3.0785	3.0788

# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0 <sup>h</sup> .20 <sup>m</sup> .0 <sup>s</sup>	3.0792	3.0795	3.0799	3.0803	3.0806	3.0810	3.0813	3.0817	3.0821	3.0824
20 10	3.0828	3.0831	3.0835	3.0839	3.0842	3.0846	3.0849	3.0853	3.0856	3.0860
20 20	3.0864	3.0867	3.0871	3.0874	3.0878	3.0881	3.0885	3.0888	3.0892	3.0896
20 30	3.0899	3.0903	3.0906	3.0910	3.0913	3.0917	3.0920	3.0924	3.0927	3.0931
20 40	3.0934	3.0938	3.0941	3.0945	3.0948	3.0952	3.0955	3.0959	3.0962	3.0966
20 50	3.0969	3.0973	3.0976	3.0980	3.0983	3.0986	3.0990	3.0993	3.0997	3.1000
0 21 0	3.1004	3.1007	3.1011	3.1014	3.1017	3.1021	3.1024	3.1028	3.1031	3.1035
21 10	3.1038	3.1041	3.1045	3.1048	3.1052	3.1055	3.1059	3.1062	3.1065	3.1069
21 20	3.1072	3.1075	3.1079	3.1082	3.1086	3.1089	3.1092	3.1096	3.1099	3.1103
21 30	3.1106	3.1109	3.1113	3.1116	3.1119	3.1123	3.1126	3.1129	3.1133	3.1136
21 40	3.1139	3.1143	3.1146	3.1149	3.1153	3.1156	3.1159	3.1163	3.1166	3.1169
21 50	3.1173	3.1176	3.1179	3.1183	3.1186	3.1189	3.1193	3.1196	3.1199	3.1202
0 22 0	3.1206	3.1209	3.1212	3.1216	3.1219	3.1222	3.1225	3.1229	3.1232	3.1235
22 10	3.1239	3.1242	3.1245	3.1248	3.1252	3.1255	3.1258	3.1261	3.1265	3.1268
22 20	3.1271	3.1274	3.1278	3.1281	3.1284	3.1287	3.1290	3.1294	3.1297	3.1300
22 30	3.1303	3.1307	3.1310	3.1313	3.1316	3.1319	3.1323	3.1326	3.1329	3.1332
22 40	3.1335	3.1339	3.1342	3.1345	3.1348	3.1351	3.1355	3.1358	3.1361	3.1364
22 50	3.1367	3.1370	3.1374	3.1377	3.1380	3.1383	3.1386	3.1389	3.1392	3.1396
0 23 0	3.1399	3.1402	3.1405	3.1408	3.1411	3.1414	3.1418	3.1421	3.1424	3.1427
23 10	3.1430	3.1433	3.1436	3.1440	3.1443	3.1446	3.1449	3.1452	3.1455	3.1458
23 20	3.1461	3.1464	3.1467	3.1471	3.1474	3.1477	3.1480	3.1483	3.1486	3.1489
23 30	3.1492	3.1495	3.1498	3.1501	3.1504	3.1508	3.1511	3.1514	3.1517	3.1520
23 40	3.1523	3.1526	3.1529	3.1532	3.1535	3.1538	3.1541	3.1544	3.1547	3.1550
23 50	3.1553	3.1556	3.1559	3.1562	3.1565	3.1569	3.1572	3.1575	3.1578	3.1581
0 24 0	3.1584	3.1587	3.1590	3.1593	3.1596	3.1599	3.1602	3.1605	3.1608	3.1611
24 10	3.1614	3.1617	3.1620	3.1623	3.1626	3.1629	3.1632	3.1635	3.1638	3.1641
24 20	3.1644	3.1647	3.1649	3.1652	3.1655	3.1658	3.1661	3.1664	3.1667	3.1670
24 30	3.1673	3.1676	3.1679	3.1682	3.1685	3.1688	3.1691	3.1694	3.1697	3.1700
24 40	3.1703	3.1706	3.1708	3.1711	3.1714	3.1717	3.1720	3.1723	3.1726	3.1729
24 50	3.1732	3.1735	3.1738	3.1741	3.1744	3.1746	3.1749	3.1752	3.1755	3.1758
0 25 0	3.1761	3.1764	3.1767	3.1770	3.1772	3.1775	3.1778	3.1781	3.1784	3.1787
25 10	3.1790	3.1793	3.1796	3.1798	3.1801	3.1804	3.1807	3.1810	3.1813	3.1816
25 20	3.1818	3.1821	3.1824	3.1827	3.1830	3.1833	3.1836	3.1838	3.1841	3.1844
25 30	3.1847	3.1850	3.1853	3.1855	3.1858	3.1861	3.1864	3.1867	3.1870	3.1872
25 40	3.1875	3.1878	3.1881	3.1884	3.1886	3.1889	3.1892	3.1895	3.1898	3.1901
25 50	3.1903	3.1906	3.1909	3.1912	3.1915	3.1917	3.1920	3.1923	3.1926	3.1928
0 26 0	3.1931	3.1934	3.1937	3.1940	3.1942	3.1945	3.1948	3.1951	3.1953	3.1956
26 10	3.1959	3.1962	3.1965	3.1967	3.1970	3.1973	3.1976	3.1978	3.1981	3.1984
26 20	3.1987	3.1989	3.1992	3.1995	3.1998	3.2000	3.2003	3.2006	3.2009	3.2011
26 30	3.2014	3.2017	3.2019	3.2022	3.2025	3.2028	3.2030	3.2033	3.2036	3.2038
26 40	3.2041	3.2044	3.2047	3.2049	3.2052	3.2055	3.2057	3.2060	3.2063	3.2066
26 50	3.2068	3.2071	3.2074	3.2076	3.2079	3.2082	3.2084	3.2087	3.2090	3.2092
0 27 0	3.2095	3.2098	3.2101	3.2103	3.2106	3.2109	3.2111	3.2114	3.2117	3.2119
27 10	3.2122	3.2125	3.2127	3.2130	3.2133	3.2135	3.2138	3.2140	3.2143	3.2146
27 20	3.2148	3.2151	3.2154	3.2156	3.2159	3.2162	3.2164	3.2167	3.2170	3.2172
27 30	3.2175	3.2177	3.2180	3.2183	3.2185	3.2188	3.2191	3.2193	3.2196	3.2198
27 40	3.2201	3.2204	3.2206	3.2209	3.2212	3.2214	3.2217	3.2219	3.2222	3.2225
27 50	3.2227	3.2230	3.2232	3.2235	3.2238	3.2240	3.2243	3.2245	3.2248	3.2250
0 28 0	3.2253	3.2256	3.2258	3.2261	3.2263	3.2266	3.2269	3.2271	3.2274	3.2276
28 10	3.2279	3.2281	3.2284	3.2287	3.2289	3.2292	3.2294	3.2297	3.2299	3.2302
28 20	3.2304	3.2307	3.2310	3.2312	3.2315	3.2317	3.2320	3.2322	3.2325	3.2327
28 30	3.2330	3.2333	3.2335	3.2338	3.2340	3.2343	3.2345	3.2348	3.2350	3.2353
28 40	3.2355	3.2358	3.2360	3.2363	3.2365	3.2368	3.2370	3.2373	3.2375	3.2378
28 50	3.2380	3.2383	3.2385	3.2388	3.2390	3.2393	3.2395	3.2398	3.2400	3.2403
0 29 0	3.2405	3.2408	3.2410	3.2413	3.2415	3.2418	3.2420	3.2423	3.2425	3.2428
29 10	3.2430	3.2433	3.2435	3.2438	3.2440	3.2443	3.2445	3.2448	3.2450	3.2453
29 20	3.2455	3.2458	3.2460	3.2463	3.2465	3.2467	3.2470	3.2472	3.2475	3.2477
29 30	3.2480	3.2482	3.2485	3.2487	3.2490	3.2492	3.2494	3.2497	3.2499	3.2502
29 40	3.2504	3.2507	3.2509	3.2512	3.2514	3.2516	3.2519	3.2521	3.2524	3.2526
29 50	3.2529	3.2531	3.2533	3.2536	3.2538	3.2541	3.2543	3.2545	3.2548	3.2550



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0° 30' 0"	3.2553	3.2555	3.2558	3.2560	3.2562	3.2565	3.2567	3.2570	3.2572	3.2574
30 10	3.2577	3.2579	3.2582	3.2584	3.2586	3.2589	3.2591	3.2594	3.2596	3.2598
30 20	3.2601	3.2603	3.2605	3.2608	3.2610	3.2613	3.2615	3.2617	3.2620	3.2622
30 30	3.2625	3.2627	3.2629	3.2632	3.2634	3.2636	3.2639	3.2641	3.2643	3.2646
30 40	3.2648	3.2651	3.2653	3.2655	3.2658	3.2660	3.2662	3.2665	3.2667	3.2669
30 50	3.2672	3.2674	3.2676	3.2679	3.2681	3.2683	3.2686	3.2688	3.2690	3.2693
0 31 0	3.2695	3.2697	3.2700	3.2702	3.2704	3.2707	3.2709	3.2711	3.2714	3.2716
31 10	3.2718	3.2721	3.2723	3.2725	3.2728	3.2730	3.2732	3.2735	3.2737	3.2739
31 20	3.2742	3.2744	3.2746	3.2749	3.2751	3.2753	3.2755	3.2758	3.2760	3.2762
31 30	3.2765	3.2767	3.2769	3.2772	3.2774	3.2776	3.2778	3.2781	3.2783	3.2785
31 40	3.2788	3.2790	3.2792	3.2794	3.2797	3.2799	3.2801	3.2804	3.2806	3.2808
31 50	3.2810	3.2813	3.2815	3.2817	3.2819	3.2822	3.2824	3.2826	3.2828	3.2831
0 32 0	3.2833	3.2835	3.2838	3.2840	3.2842	3.2844	3.2847	3.2849	3.2851	3.2853
32 10	3.2856	3.2858	3.2860	3.2862	3.2865	3.2867	3.2869	3.2871	3.2874	3.2876
32 20	3.2878	3.2880	3.2882	3.2885	3.2887	3.2889	3.2891	3.2894	3.2896	3.2898
32 30	3.2900	3.2903	3.2905	3.2907	3.2909	3.2911	3.2914	3.2916	3.2918	3.2920
32 40	3.2923	3.2925	3.2927	3.2929	3.2931	3.2934	3.2936	3.2938	3.2940	3.2942
32 50	3.2945	3.2947	3.2949	3.2951	3.2953	3.2956	3.2958	3.2960	3.2962	3.2964
0 33 0	3.2967	3.2969	3.2971	3.2973	3.2975	3.2978	3.2980	3.2982	3.2984	3.2986
33 10	3.2989	3.2991	3.2993	3.2995	3.2997	3.2999	3.3002	3.3004	3.3006	3.3008
33 20	3.3010	3.3012	3.3015	3.3017	3.3019	3.3021	3.3023	3.3025	3.3028	3.3030
33 30	3.3032	3.3034	3.3036	3.3038	3.3041	3.3043	3.3045	3.3047	3.3049	3.3051
33 40	3.3054	3.3056	3.3058	3.3060	3.3062	3.3064	3.3066	3.3069	3.3071	3.3073
33 50	3.3075	3.3077	3.3079	3.3081	3.3084	3.3086	3.3088	3.3090	3.3092	3.3094
0 34 0	3.3096	3.3098	3.3101	3.3103	3.3105	3.3107	3.3109	3.3111	3.3113	3.3115
34 10	3.3118	3.3120	3.3122	3.3124	3.3126	3.3128	3.3130	3.3132	3.3134	3.3137
34 20	3.3139	3.3141	3.3143	3.3145	3.3147	3.3149	3.3151	3.3153	3.3156	3.3158
34 30	3.3160	3.3162	3.3164	3.3166	3.3168	3.3170	3.3172	3.3174	3.3176	3.3179
34 40	3.3181	3.3183	3.3185	3.3187	3.3189	3.3191	3.3193	3.3195	3.3197	3.3199
34 50	3.3201	3.3204	3.3206	3.3208	3.3210	3.3212	3.3214	3.3216	3.3218	3.3220
0 35 0	3.3222	3.3224	3.3226	3.3228	3.3230	3.3233	3.3235	3.3237	3.3239	3.3241
35 10	3.3243	3.3245	3.3247	3.3249	3.3251	3.3253	3.3255	3.3257	3.3259	3.3261
35 20	3.3263	3.3265	3.3267	3.3269	3.3272	3.3274	3.3276	3.3278	3.3280	3.3282
35 30	3.3284	3.3286	3.3288	3.3290	3.3292	3.3294	3.3296	3.3298	3.3300	3.3302
35 40	3.3304	3.3306	3.3308	3.3310	3.3312	3.3314	3.3316	3.3318	3.3320	3.3322
35 50	3.3324	3.3326	3.3328	3.3330	3.3332	3.3334	3.3336	3.3339	3.3341	3.3343
0 36 0	3.3345	3.3347	3.3349	3.3351	3.3353	3.3355	3.3357	3.3359	3.3361	3.3363
36 10	3.3365	3.3367	3.3369	3.3371	3.3373	3.3375	3.3377	3.3379	3.3381	3.3383
36 20	3.3385	3.3387	3.3389	3.3391	3.3393	3.3395	3.3397	3.3398	3.3400	3.3402
36 30	3.3404	3.3406	3.3408	3.3410	3.3412	3.3414	3.3416	3.3418	3.3420	3.3422
36 40	3.3424	3.3426	3.3428	3.3430	3.3432	3.3434	3.3436	3.3438	3.3440	3.3442
36 50	3.3444	3.3446	3.3448	3.3450	3.3452	3.3454	3.3456	3.3458	3.3460	3.3462
0 37 0	3.3464	3.3465	3.3467	3.3469	3.3471	3.3473	3.3475	3.3477	3.3479	3.3481
37 10	3.3483	3.3485	3.3487	3.3489	3.3491	3.3493	3.3495	3.3497	3.3499	3.3501
37 20	3.3502	3.3504	3.3506	3.3508	3.3510	3.3512	3.3514	3.3516	3.3518	3.3520
37 30	3.3522	3.3524	3.3526	3.3528	3.3530	3.3531	3.3533	3.3535	3.3537	3.3539
37 40	3.3541	3.3543	3.3545	3.3547	3.3549	3.3551	3.3553	3.3555	3.3556	3.3558
37 50	3.3560	3.3562	3.3564	3.3566	3.3568	3.3570	3.3572	3.3574	3.3576	3.3577
0 38 0	3.3579	3.3581	3.3583	3.3585	3.3587	3.3589	3.3591	3.3593	3.3595	3.3596
38 10	3.3598	3.3600	3.3602	3.3604	3.3606	3.3608	3.3610	3.3612	3.3614	3.3615
38 20	3.3617	3.3619	3.3621	3.3623	3.3625	3.3627	3.3629	3.3630	3.3632	3.3634
38 30	3.3636	3.3638	3.3640	3.3642	3.3644	3.3646	3.3647	3.3649	3.3651	3.3653
38 40	3.3655	3.3657	3.3659	3.3660	3.3662	3.3664	3.3666	3.3668	3.3670	3.3672
38 50	3.3674	3.3675	3.3677	3.3679	3.3681	3.3683	3.3685	3.3687	3.3688	3.3690
0 39 0	3.3692	3.3694	3.3696	3.3698	3.3700	3.3701	3.3703	3.3705	3.3707	3.3709
39 10	3.3711	3.3713	3.3714	3.3716	3.3718	3.3720	3.3722	3.3724	3.3725	3.3727
39 20	3.3729	3.3731	3.3733	3.3735	3.3736	3.3738	3.3740	3.3742	3.3744	3.3746
39 30	3.3747	3.3749	3.3751	3.3753	3.3755	3.3757	3.3758	3.3760	3.3762	3.3764
39 40	3.3766	3.3768	3.3769	3.3771	3.3773	3.3775	3.3777	3.3779	3.3780	3.3782
39 50	3.3784	3.3786	3.3788	3.3789	3.3791	3.3793	3.3795	3.3797	3.3798	3.3800



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0° 40' 0"	3.3802	3.3804	3.3806	3.3808	3.3809	3.3811	3.3813	3.3815	3.3817	3.3818
40 10	3.3820	3.3822	3.3824	3.3826	3.3827	3.3829	3.3831	3.3833	3.3835	3.3836
40 20	3.3838	3.3840	3.3842	3.3844	3.3845	3.3847	3.3849	3.3851	3.3852	3.3854
40 30	3.3856	3.3858	3.3860	3.3861	3.3863	3.3865	3.3867	3.3869	3.3870	3.3872
40 40	3.3874	3.3876	3.3877	3.3879	3.3881	3.3883	3.3885	3.3886	3.3888	3.3890
40 50	3.3892	3.3893	3.3895	3.3897	3.3899	3.3901	3.3902	3.3904	3.3906	3.3908
0 41 0	3.3909	3.3911	3.3913	3.3915	3.3916	3.3918	3.3920	3.3922	3.3923	3.3925
41 10	3.3927	3.3929	3.3930	3.3932	3.3934	3.3936	3.3938	3.3939	3.3941	3.3943
41 20	3.3945	3.3946	3.3948	3.3950	3.3952	3.3953	3.3955	3.3957	3.3959	3.3960
41 30	3.3962	3.3964	3.3965	3.3967	3.3969	3.3971	3.3972	3.3974	3.3976	3.3978
41 40	3.3979	3.3981	3.3983	3.3985	3.3986	3.3988	3.3990	3.3992	3.3993	3.3995
41 50	3.3997	3.3998	3.4000	3.4002	3.4004	3.4005	3.4007	3.4009	3.4011	3.4012
0 42 0	3.4014	3.4016	3.4017	3.4019	3.4021	3.4023	3.4024	3.4026	3.4028	3.4029
42 10	3.4031	3.4033	3.4035	3.4036	3.4038	3.4040	3.4041	3.4043	3.4045	3.4047
42 20	3.4048	3.4050	3.4052	3.4053	3.4055	3.4057	3.4059	3.4060	3.4062	3.4064
42 30	3.4065	3.4067	3.4069	3.4071	3.4072	3.4074	3.4076	3.4077	3.4079	3.4081
42 40	3.4082	3.4084	3.4086	3.4087	3.4089	3.4091	3.4093	3.4094	3.4096	3.4098
42 50	3.4099	3.4101	3.4103	3.4104	3.4106	3.4108	3.4109	3.4111	3.4113	3.4115
0 43 0	3.4116	3.4118	3.4120	3.4121	3.4123	3.4125	3.4126	3.4128	3.4130	3.4131
43 10	3.4133	3.4135	3.4136	3.4138	3.4140	3.4141	3.4143	3.4145	3.4146	3.4148
43 20	3.4150	3.4151	3.4153	3.4155	3.4156	3.4158	3.4160	3.4161	3.4163	3.4165
43 30	3.4166	3.4168	3.4170	3.4171	3.4173	3.4175	3.4176	3.4178	3.4180	3.4181
43 40	3.4183	3.4185	3.4186	3.4188	3.4190	3.4191	3.4193	3.4195	3.4196	3.4198
43 50	3.4200	3.4201	3.4203	3.4205	3.4206	3.4208	3.4209	3.4211	3.4213	3.4214
0 44 0	3.4216	3.4218	3.4219	3.4221	3.4223	3.4224	3.4226	3.4228	3.4229	3.4231
44 10	3.4232	3.4234	3.4236	3.4237	3.4239	3.4241	3.4242	3.4244	3.4246	3.4247
44 20	3.4249	3.4250	3.4252	3.4254	3.4255	3.4257	3.4259	3.4260	3.4262	3.4263
44 30	3.4265	3.4267	3.4268	3.4270	3.4272	3.4273	3.4275	3.4276	3.4278	3.4280
44 40	3.4281	3.4283	3.4285	3.4286	3.4288	3.4289	3.4291	3.4293	3.4294	3.4296
44 50	3.4298	3.4299	3.4301	3.4302	3.4304	3.4306	3.4307	3.4309	3.4310	3.4312
0 45 0	3.4314	3.4315	3.4317	3.4318	3.4320	3.4322	3.4323	3.4325	3.4326	3.4328
45 10	3.4330	3.4331	3.4333	3.4334	3.4336	3.4338	3.4339	3.4341	3.4342	3.4344
45 20	3.4346	3.4347	3.4349	3.4350	3.4352	3.4354	3.4355	3.4357	3.4358	3.4360
45 30	3.4362	3.4363	3.4365	3.4366	3.4368	3.4370	3.4371	3.4373	3.4374	3.4376
45 40	3.4378	3.4379	3.4381	3.4382	3.4384	3.4385	3.4387	3.4389	3.4390	3.4392
45 50	3.4393	3.4395	3.4396	3.4398	3.4400	3.4401	3.4403	3.4404	3.4406	3.4408
0 46 0	3.4409	3.4411	3.4412	3.4414	3.4415	3.4417	3.4419	3.4420	3.4422	3.4423
46 10	3.4425	3.4426	3.4428	3.4429	3.4431	3.4433	3.4434	3.4436	3.4437	3.4439
46 20	3.4440	3.4442	3.4444	3.4445	3.4447	3.4448	3.4450	3.4451	3.4453	3.4454
46 30	3.4456	3.4458	3.4459	3.4461	3.4462	3.4464	3.4465	3.4467	3.4468	3.4470
46 40	3.4472	3.4473	3.4475	3.4476	3.4478	3.4479	3.4481	3.4482	3.4484	3.4486
46 50	3.4487	3.4489	3.4490	3.4492	3.4493	3.4495	3.4496	3.4498	3.4499	3.4501
0 47 0	3.4502	3.4504	3.4506	3.4507	3.4509	3.4510	3.4512	3.4513	3.4515	3.4516
47 10	3.4518	3.4519	3.4521	3.4522	3.4524	3.4526	3.4527	3.4529	3.4530	3.4532
47 20	3.4533	3.4535	3.4536	3.4538	3.4539	3.4541	3.4542	3.4544	3.4545	3.4547
47 30	3.4548	3.4550	3.4551	3.4553	3.4555	3.4556	3.4558	3.4559	3.4561	3.4562
47 40	3.4564	3.4565	3.4567	3.4568	3.4570	3.4571	3.4573	3.4574	3.4576	3.4577
47 50	3.4579	3.4580	3.4582	3.4583	3.4585	3.4586	3.4588	3.4589	3.4591	3.4592
0 48 0	3.4594	3.4595	3.4597	3.4598	3.4600	3.4601	3.4603	3.4604	3.4606	3.4607
48 10	3.4609	3.4610	3.4612	3.4613	3.4615	3.4616	3.4618	3.4619	3.4621	3.4622
48 20	3.4624	3.4625	3.4627	3.4628	3.4630	3.4631	3.4633	3.4634	3.4636	3.4637
48 30	3.4639	3.4640	3.4642	3.4643	3.4645	3.4646	3.4648	3.4649	3.4651	3.4652
48 40	3.4654	3.4655	3.4657	3.4658	3.4660	3.4661	3.4663	3.4664	3.4666	3.4667
48 50	3.4669	3.4670	3.4672	3.4673	3.4675	3.4676	3.4678	3.4679	3.4681	3.4682
0 49 0	3.4683	3.4685	3.4686	3.4688	3.4689	3.4691	3.4692	3.4694	3.4695	3.4697
49 10	3.4698	3.4700	3.4701	3.4703	3.4704	3.4706	3.4707	3.4709	3.4710	3.4711
49 20	3.4713	3.4714	3.4716	3.4717	3.4719	3.4720	3.4722	3.4723	3.4725	3.4726
49 30	3.4728	3.4729	3.4730	3.4732	3.4733	3.4735	3.4736	3.4738	3.4739	3.4741
49 40	3.4742	3.4744	3.4745	3.4747	3.4748	3.4749	3.4751	3.4752	3.4754	3.4755
49 50	3.4757	3.4758	3.4760	3.4761	3.4763	3.4764	3.4765	3.4767	3.4768	3.4770

# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0 <sup>h</sup> .50 <sup>m</sup> .0 <sup>s</sup> .	3.4771	3.4773	3.4774	3.4776	3.4777	3.4778	3.4780	3.4781	3.4783	3.4784
50 10	3.4786	3.4787	3.4789	3.4790	3.4791	3.4793	3.4794	3.4796	3.4797	3.4799
50 20	3.4800	3.4802	3.4803	3.4804	3.4806	3.4807	3.4809	3.4810	3.4812	3.4813
50 30	3.4814	3.4816	3.4817	3.4819	3.4820	3.4822	3.4823	3.4824	3.4826	3.4827
50 40	3.4829	3.4830	3.4832	3.4833	3.4834	3.4836	3.4837	3.4839	3.4840	3.4842
50 50	3.4843	3.4844	3.4846	3.4847	3.4849	3.4850	3.4852	3.4853	3.4854	3.4856
0 51 0	3.4857	3.4859	3.4860	3.4861	3.4863	3.4864	3.4866	3.4867	3.4869	3.4870
51 10	3.4871	3.4873	3.4874	3.4876	3.4877	3.4878	3.4880	3.4881	3.4883	3.4884
51 20	3.4886	3.4887	3.4888	3.4890	3.4891	3.4893	3.4894	3.4895	3.4897	3.4898
51 30	3.4900	3.4901	3.4902	3.4904	3.4905	3.4907	3.4908	3.4909	3.4911	3.4912
51 40	3.4914	3.4915	3.4916	3.4918	3.4919	3.4921	3.4922	3.4923	3.4925	3.4926
51 50	3.4928	3.4929	3.4930	3.4932	3.4933	3.4935	3.4936	3.4937	3.4939	3.4940
0 52 0	3.4942	3.4943	3.4944	3.4946	3.4947	3.4949	3.4950	3.4951	3.4953	3.4954
52 10	3.4955	3.4957	3.4958	3.4960	3.4961	3.4962	3.4964	3.4965	3.4967	3.4968
52 20	3.4969	3.4971	3.4972	3.4973	3.4975	3.4976	3.4978	3.4979	3.4980	3.4982
52 30	3.4983	3.4984	3.4986	3.4987	3.4989	3.4990	3.4991	3.4993	3.4994	3.4995
52 40	3.4997	3.4998	3.5000	3.5001	3.5002	3.5004	3.5005	3.5006	3.5008	3.5009
52 50	3.5011	3.5012	3.5013	3.5015	3.5016	3.5017	3.5019	3.5020	3.5022	3.5023
0 53 0	3.5024	3.5026	3.5027	3.5028	3.5030	3.5031	3.5032	3.5034	3.5035	3.5037
53 10	3.5038	3.5039	3.5041	3.5042	3.5043	3.5045	3.5046	3.5047	3.5049	3.5050
53 20	3.5051	3.5053	3.5054	3.5056	3.5057	3.5058	3.5060	3.5061	3.5062	3.5064
53 30	3.5065	3.5066	3.5068	3.5069	3.5070	3.5072	3.5073	3.5075	3.5076	3.5077
53 40	3.5079	3.5080	3.5081	3.5083	3.5084	3.5085	3.5087	3.5088	3.5089	3.5091
53 50	3.5092	3.5093	3.5095	3.5096	3.5097	3.5099	3.5100	3.5101	3.5103	3.5104
0 54 0	3.5105	3.5107	3.5108	3.5109	3.5111	3.5112	3.5113	3.5115	3.5116	3.5117
54 10	3.5119	3.5120	3.5122	3.5123	3.5124	3.5126	3.5127	3.5128	3.5130	3.5131
54 20	3.5132	3.5134	3.5135	3.5136	3.5138	3.5139	3.5140	3.5141	3.5143	3.5144
54 30	3.5145	3.5147	3.5148	3.5149	3.5151	3.5152	3.5153	3.5155	3.5156	3.5157
54 40	3.5159	3.5160	3.5161	3.5163	3.5164	3.5165	3.5167	3.5168	3.5169	3.5171
54 50	3.5172	3.5173	3.5175	3.5176	3.5177	3.5179	3.5180	3.5181	3.5183	3.5184
0 55 0	3.5185	3.5186	3.5188	3.5189	3.5190	3.5192	3.5193	3.5194	3.5196	3.5197
55 10	3.5198	3.5200	3.5201	3.5202	3.5204	3.5205	3.5206	3.5207	3.5209	3.5210
55 20	3.5211	3.5213	3.5214	3.5215	3.5217	3.5218	3.5219	3.5221	3.5222	3.5223
55 30	3.5224	3.5226	3.5227	3.5228	3.5230	3.5231	3.5232	3.5234	3.5235	3.5236
55 40	3.5237	3.5239	3.5240	3.5241	3.5243	3.5244	3.5245	3.5247	3.5248	3.5249
55 50	3.5250	3.5252	3.5253	3.5254	3.5256	3.5257	3.5258	3.5260	3.5261	3.5262
0 56 0	3.5263	3.5265	3.5266	3.5267	3.5269	3.5270	3.5271	3.5272	3.5274	3.5275
56 10	3.5276	3.5278	3.5279	3.5280	3.5281	3.5283	3.5284	3.5285	3.5287	3.5288
56 20	3.5289	3.5290	3.5292	3.5293	3.5294	3.5296	3.5297	3.5298	3.5299	3.5301
56 30	3.5302	3.5303	3.5305	3.5306	3.5307	3.5308	3.5310	3.5311	3.5312	3.5314
56 40	3.5315	3.5316	3.5317	3.5319	3.5320	3.5321	3.5322	3.5324	3.5325	3.5326
56 50	3.5328	3.5329	3.5330	3.5331	3.5333	3.5334	3.5335	3.5336	3.5338	3.5339
0 57 0	3.5340	3.5342	3.5343	3.5344	3.5345	3.5347	3.5348	3.5349	3.5350	3.5352
57 10	3.5353	3.5354	3.5355	3.5357	3.5358	3.5359	3.5361	3.5362	3.5363	3.5364
57 20	3.5366	3.5367	3.5368	3.5369	3.5371	3.5372	3.5373	3.5374	3.5376	3.5377
57 30	3.5378	3.5379	3.5381	3.5382	3.5383	3.5384	3.5386	3.5387	3.5388	3.5390
57 40	3.5391	3.5392	3.5393	3.5395	3.5396	3.5397	3.5398	3.5400	3.5401	3.5402
57 50	3.5403	3.5405	3.5406	3.5407	3.5408	3.5410	3.5411	3.5412	3.5413	3.5415
0 58 0	3.5416	3.5417	3.5418	3.5420	3.5421	3.5422	3.5423	3.5425	3.5426	3.5427
58 10	3.5428	3.5429	3.5431	3.5432	3.5433	3.5434	3.5436	3.5437	3.5438	3.5439
58 20	3.5441	3.5442	3.5443	3.5444	3.5446	3.5447	3.5448	3.5449	3.5451	3.5452
58 30	3.5453	3.5454	3.5456	3.5457	3.5458	3.5459	3.5460	3.5462	3.5463	3.5464
58 40	3.5465	3.5467	3.5468	3.5469	3.5470	3.5472	3.5473	3.5474	3.5475	3.5477
58 50	3.5478	3.5479	3.5480	3.5481	3.5483	3.5484	3.5485	3.5486	3.5488	3.5489
0 59 0	3.5490	3.5491	3.5492	3.5494	3.5495	3.5496	3.5497	3.5499	3.5500	3.5501
59 10	3.5502	3.5504	3.5505	3.5506	3.5507	3.5508	3.5510	3.5511	3.5512	3.5513
59 20	3.5514	3.5516	3.5517	3.5518	3.5519	3.5521	3.5522	3.5523	3.5524	3.5525
59 30	3.5527	3.5528	3.5529	3.5530	3.5532	3.5533	3.5534	3.5535	3.5536	3.5538
59 40	3.5539	3.5540	3.5541	3.5542	3.5544	3.5545	3.5546	3.5547	3.5549	3.5550
59 50	3.5551	3.5552	3.5553	3.5555	3.5556	3.5557	3.5558	3.5559	3.5561	3.5562



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
1 <sup>h</sup> . 0 <sup>m</sup> . 0 <sup>s</sup> .	3.5563	3.5564	3.5565	3.5567	3.5568	3.5569	3.5570	3.5571	3.5573	3.5574
0 10	3.5575	3.5576	3.5577	3.5579	3.5580	3.5581	3.5582	3.5583	3.5585	3.5586
0 20	3.5587	3.5588	3.5589	3.5591	3.5592	3.5593	3.5594	3.5595	3.5597	3.5598
0 30	3.5599	3.5600	3.5601	3.5603	3.5604	3.5605	3.5606	3.5607	3.5609	3.5610
0 40	3.5611	3.5612	3.5613	3.5615	3.5616	3.5617	3.5618	3.5619	3.5621	3.5622
0 50	3.5623	3.5624	3.5625	3.5626	3.5628	3.5629	3.5630	3.5631	3.5632	3.5634
1 1 0	3.5635	3.5636	3.5637	3.5638	3.5640	3.5641	3.5642	3.5643	3.5644	3.5645
1 1 10	3.5647	3.5648	3.5649	3.5650	3.5651	3.5653	3.5654	3.5655	3.5656	3.5657
1 1 20	3.5658	3.5660	3.5661	3.5662	3.5663	3.5664	3.5666	3.5667	3.5668	3.5669
1 1 30	3.5670	3.5671	3.5673	3.5674	3.5675	3.5676	3.5677	3.5678	3.5680	3.5681
1 1 40	3.5682	3.5683	3.5684	3.5686	3.5687	3.5688	3.5689	3.5690	3.5691	3.5693
1 1 50	3.5694	3.5695	3.5696	3.5697	3.5698	3.5700	3.5701	3.5702	3.5703	3.5704
1 2 0	3.5705	3.5707	3.5708	3.5709	3.5710	3.5711	3.5712	3.5714	3.5715	3.5716
1 2 10	3.5717	3.5718	3.5719	3.5721	3.5722	3.5723	3.5724	3.5725	3.5726	3.5728
1 2 20	3.5729	3.5730	3.5731	3.5732	3.5733	3.5735	3.5736	3.5737	3.5738	3.5739
1 2 30	3.5740	3.5741	3.5742	3.5744	3.5745	3.5746	3.5747	3.5748	3.5750	3.5751
1 2 40	3.5752	3.5753	3.5754	3.5755	3.5756	3.5758	3.5759	3.5760	3.5761	3.5762
1 2 50	3.5763	3.5765	3.5766	3.5767	3.5768	3.5769	3.5770	3.5771	3.5773	3.5774
1 3 0	3.5775	3.5776	3.5777	3.5778	3.5780	3.5781	3.5782	3.5783	3.5784	3.5785
1 3 10	3.5786	3.5788	3.5789	3.5790	3.5791	3.5792	3.5793	3.5794	3.5796	3.5797
1 3 20	3.5798	3.5799	3.5800	3.5801	3.5802	3.5804	3.5805	3.5806	3.5807	3.5808
1 3 30	3.5809	3.5810	3.5812	3.5813	3.5814	3.5815	3.5816	3.5817	3.5818	3.5819
1 3 40	3.5821	3.5822	3.5823	3.5824	3.5825	3.5826	3.5827	3.5829	3.5830	3.5831
1 3 50	3.5832	3.5833	3.5834	3.5835	3.5837	3.5838	3.5839	3.5840	3.5841	3.5842
1 4 0	3.5843	3.5844	3.5846	3.5847	3.5848	3.5849	3.5850	3.5851	3.5852	3.5853
1 4 10	3.5855	3.5856	3.5857	3.5858	3.5859	3.5860	3.5861	3.5862	3.5864	3.5865
1 4 20	3.5866	3.5867	3.5868	3.5869	3.5870	3.5871	3.5873	3.5874	3.5875	3.5876
1 4 30	3.5877	3.5878	3.5879	3.5880	3.5882	3.5883	3.5884	3.5885	3.5886	3.5887
1 4 40	3.5888	3.5889	3.5891	3.5892	3.5893	3.5894	3.5895	3.5896	3.5897	3.5898
1 4 50	3.5899	3.5901	3.5902	3.5903	3.5904	3.5905	3.5906	3.5907	3.5908	3.5910
1 5 0	3.5911	3.5912	3.5913	3.5914	3.5915	3.5916	3.5917	3.5918	3.5920	3.5921
1 5 10	3.5922	3.5923	3.5924	3.5925	3.5926	3.5927	3.5928	3.5930	3.5931	3.5932
1 5 20	3.5933	3.5934	3.5935	3.5936	3.5937	3.5938	3.5940	3.5941	3.5942	3.5943
1 5 30	3.5944	3.5945	3.5946	3.5947	3.5948	3.5949	3.5951	3.5952	3.5953	3.5954
1 5 40	3.5955	3.5956	3.5957	3.5958	3.5959	3.5960	3.5962	3.5963	3.5964	3.5965
1 5 50	3.5966	3.5967	3.5968	3.5969	3.5970	3.5971	3.5973	3.5974	3.5975	3.5976
1 6 0	3.5977	3.5978	3.5979	3.5980	3.5981	3.5982	3.5984	3.5985	3.5986	3.5987
1 6 10	3.5988	3.5989	3.5990	3.5991	3.5992	3.5993	3.5994	3.5996	3.5997	3.5998
1 6 20	3.5999	3.6000	3.6001	3.6002	3.6003	3.6004	3.6005	3.6006	3.6008	3.6009
1 6 30	3.6010	3.6011	3.6012	3.6013	3.6014	3.6015	3.6016	3.6017	3.6018	3.6020
1 6 40	3.6021	3.6022	3.6023	3.6024	3.6025	3.6026	3.6027	3.6028	3.6029	3.6030
1 6 50	3.6031	3.6033	3.6034	3.6035	3.6036	3.6037	3.6038	3.6039	3.6040	3.6041
1 7 0	3.6042	3.6043	3.6044	3.6046	3.6047	3.6048	3.6049	3.6050	3.6051	3.6052
1 7 10	3.6053	3.6054	3.6055	3.6056	3.6057	3.6058	3.6060	3.6061	3.6062	3.6063
1 7 20	3.6064	3.6065	3.6066	3.6067	3.6068	3.6069	3.6070	3.6071	3.6072	3.6073
1 7 30	3.6075	3.6076	3.6077	3.6078	3.6079	3.6080	3.6081	3.6082	3.6083	3.6084
1 7 40	3.6085	3.6086	3.6087	3.6088	3.6090	3.6091	3.6092	3.6093	3.6094	3.6095
1 7 50	3.6096	3.6097	3.6098	3.6099	3.6100	3.6101	3.6102	3.6103	3.6104	3.6106
1 8 0	3.6107	3.6108	3.6109	3.6110	3.6111	3.6112	3.6113	3.6114	3.6115	3.6116
1 8 10	3.6117	3.6118	3.6119	3.6120	3.6121	3.6123	3.6124	3.6125	3.6126	3.6127
1 8 20	3.6128	3.6129	3.6130	3.6131	3.6132	3.6133	3.6134	3.6135	3.6136	3.6137
1 8 30	3.6138	3.6139	3.6141	3.6142	3.6143	3.6144	3.6145	3.6146	3.6147	3.6148
1 8 40	3.6149	3.6150	3.6151	3.6152	3.6153	3.6154	3.6155	3.6156	3.6157	3.6158
1 8 50	3.6160	3.6161	3.6162	3.6163	3.6164	3.6165	3.6166	3.6167	3.6168	3.6169
1 9 0	3.6170	3.6171	3.6172	3.6173	3.6174	3.6175	3.6176	3.6177	3.6178	3.6179
1 9 10	3.6180	3.6182	3.6183	3.6184	3.6185	3.6186	3.6187	3.6188	3.6189	3.6190
1 9 20	3.6191	3.6192	3.6193	3.6194	3.6195	3.6196	3.6197	3.6198	3.6199	3.6200
1 9 30	3.6201	3.6202	3.6203	3.6204	3.6206	3.6207	3.6208	3.6209	3.6210	3.6211
1 9 40	3.6212	3.6213	3.6214	3.6215	3.6216	3.6217	3.6218	3.6219	3.6220	3.6221
1 9 50	3.6222	3.6223	3.6224	3.6225	3.6226	3.6227	3.6228	3.6229	3.6230	3.6231



# TABLE IX.

LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
$1^{\circ} 10' 0''$	3.6232	3.6234	3.6235	3.6236	3.6237	3.6238	3.6239	3.6240	3.6241	3.6242
10 10	3.6243	3.6244	3.6245	3.6246	3.6247	3.6248	3.6249	3.6250	3.6251	3.6252
10 20	3.6253	3.6254	3.6255	3.6256	3.6257	3.6258	3.6259	3.6260	3.6261	3.6262
10 30	3.6263	3.6264	3.6265	3.6266	3.6268	3.6269	3.6270	3.6271	3.6272	3.6273
10 40	3.6274	3.6275	3.6276	3.6277	3.6278	3.6279	3.6280	3.6281	3.6282	3.6283
10 50	3.6284	3.6285	3.6286	3.6287	3.6288	3.6289	3.6290	3.6291	3.6292	3.6293
1 11 0	3.6294	3.6295	3.6296	3.6297	3.6298	3.6299	3.6300	3.6301	3.6302	3.6303
11 10	3.6304	3.6305	3.6306	3.6307	3.6308	3.6309	3.6310	3.6311	3.6312	3.6313
11 20	3.6314	3.6315	3.6316	3.6317	3.6318	3.6320	3.6321	3.6322	3.6323	3.6324
11 30	3.6325	3.6326	3.6327	3.6328	3.6329	3.6330	3.6331	3.6332	3.6333	3.6334
11 40	3.6335	3.6336	3.6337	3.6338	3.6339	3.6340	3.6341	3.6342	3.6343	3.6344
11 50	3.6345	3.6346	3.6347	3.6348	3.6349	3.6350	3.6351	3.6352	3.6353	3.6354
1 12 0	3.6355	3.6356	3.6357	3.6358	3.6359	3.6360	3.6361	3.6362	3.6363	3.6364
12 10	3.6365	3.6366	3.6367	3.6368	3.6369	3.6370	3.6371	3.6372	3.6373	3.6374
12 20	3.6375	3.6376	3.6377	3.6378	3.6379	3.6380	3.6381	3.6382	3.6383	3.6384
12 30	3.6385	3.6386	3.6387	3.6388	3.6389	3.6390	3.6391	3.6392	3.6393	3.6394
12 40	3.6395	3.6396	3.6397	3.6398	3.6399	3.6400	3.6401	3.6402	3.6403	3.6404
12 50	3.6405	3.6406	3.6407	3.6408	3.6409	3.6410	3.6411	3.6412	3.6413	3.6414
1 13 0	3.6415	3.6416	3.6417	3.6418	3.6419	3.6420	3.6421	3.6422	3.6423	3.6424
13 10	3.6425	3.6426	3.6427	3.6428	3.6429	3.6430	3.6431	3.6432	3.6433	3.6434
13 20	3.6435	3.6436	3.6437	3.6438	3.6439	3.6440	3.6441	3.6442	3.6443	3.6444
13 30	3.6445	3.6446	3.6447	3.6448	3.6449	3.6450	3.6451	3.6452	3.6453	3.6454
13 40	3.6455	3.6456	3.6457	3.6458	3.6459	3.6460	3.6461	3.6462	3.6463	3.6464
13 50	3.6465	3.6466	3.6467	3.6468	3.6469	3.6470	3.6471	3.6472	3.6473	3.6474
1 14 0	3.6475	3.6476	3.6477	3.6478	3.6479	3.6480	3.6481	3.6482	3.6483	3.6484
14 10	3.6485	3.6486	3.6487	3.6488	3.6489	3.6490	3.6491	3.6492	3.6493	3.6494
14 20	3.6495	3.6496	3.6497	3.6498	3.6499	3.6500	3.6501	3.6502	3.6503	3.6504
14 30	3.6505	3.6506	3.6507	3.6508	3.6509	3.6510	3.6511	3.6512	3.6513	3.6514
14 40	3.6515	3.6516	3.6517	3.6518	3.6519	3.6520	3.6521	3.6522	3.6523	3.6524
14 50	3.6525	3.6526	3.6527	3.6528	3.6529	3.6530	3.6531	3.6532	3.6533	3.6534
1 15 0	3.6535	3.6536	3.6537	3.6538	3.6539	3.6540	3.6541	3.6542	3.6543	3.6544
15 10	3.6545	3.6546	3.6547	3.6548	3.6549	3.6550	3.6551	3.6552	3.6553	3.6554
15 20	3.6555	3.6556	3.6557	3.6558	3.6559	3.6560	3.6561	3.6562	3.6563	3.6564
15 30	3.6565	3.6566	3.6567	3.6568	3.6569	3.6570	3.6571	3.6572	3.6573	3.6574
15 40	3.6575	3.6576	3.6577	3.6578	3.6579	3.6580	3.6581	3.6582	3.6583	3.6584
15 50	3.6585	3.6586	3.6587	3.6588	3.6589	3.6590	3.6591	3.6592	3.6593	3.6594
1 16 0	3.6595	3.6596	3.6597	3.6598	3.6599	3.6600	3.6601	3.6602	3.6603	3.6604
16 10	3.6605	3.6606	3.6607	3.6608	3.6609	3.6610	3.6611	3.6612	3.6613	3.6614
16 20	3.6615	3.6616	3.6617	3.6618	3.6619	3.6620	3.6621	3.6622	3.6623	3.6624
16 30	3.6625	3.6626	3.6627	3.6628	3.6629	3.6630	3.6631	3.6632	3.6633	3.6634
16 40	3.6635	3.6636	3.6637	3.6638	3.6639	3.6640	3.6641	3.6642	3.6643	3.6644
16 50	3.6645	3.6646	3.6647	3.6648	3.6649	3.6650	3.6651	3.6652	3.6653	3.6654
1 17 0	3.6655	3.6656	3.6657	3.6658	3.6659	3.6660	3.6661	3.6662	3.6663	3.6664
17 10	3.6665	3.6666	3.6667	3.6668	3.6669	3.6670	3.6671	3.6672	3.6673	3.6674
17 20	3.6675	3.6676	3.6677	3.6678	3.6679	3.6680	3.6681	3.6682	3.6683	3.6684
17 30	3.6685	3.6686	3.6687	3.6688	3.6689	3.6690	3.6691	3.6692	3.6693	3.6694
17 40	3.6695	3.6696	3.6697	3.6698	3.6699	3.6700	3.6701	3.6702	3.6703	3.6704
17 50	3.6705	3.6706	3.6707	3.6708	3.6709	3.6710	3.6711	3.6712	3.6713	3.6714
1 18 0	3.6715	3.6716	3.6717	3.6718	3.6719	3.6720	3.6721	3.6722	3.6723	3.6724
18 10	3.6725	3.6726	3.6727	3.6728	3.6729	3.6730	3.6731	3.6732	3.6733	3.6734
18 20	3.6735	3.6736	3.6737	3.6738	3.6739	3.6740	3.6741	3.6742	3.6743	3.6744
18 30	3.6745	3.6746	3.6747	3.6748	3.6749	3.6750	3.6751	3.6752	3.6753	3.6754
18 40	3.6755	3.6756	3.6757	3.6758	3.6759	3.6760	3.6761	3.6762	3.6763	3.6764
18 50	3.6765	3.6766	3.6767	3.6768	3.6769	3.6770	3.6771	3.6772	3.6773	3.6774
1 19 0	3.6775	3.6776	3.6777	3.6778	3.6779	3.6780	3.6781	3.6782	3.6783	3.6784
19 10	3.6785	3.6786	3.6787	3.6788	3.6789	3.6790	3.6791	3.6792	3.6793	3.6794
19 20	3.6795	3.6796	3.6797	3.6798	3.6799	3.6800	3.6801	3.6802	3.6803	3.6804
19 30	3.6805	3.6806	3.6807	3.6808	3.6809	3.6810	3.6811	3.6812	3.6813	3.6814
19 40	3.6815	3.6816	3.6817	3.6818	3.6819	3.6820	3.6821	3.6822	3.6823	3.6824
19 50	3.6825	3.6826	3.6827	3.6828	3.6829	3.6830	3.6831	3.6832	3.6833	3.6834

# TABLE IX.

LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
1 <sup>h</sup> . 20 <sup>m</sup> . 0 <sup>s</sup>	3.6812	3.6813	3.6814	3.6815	3.6816	3.6817	3.6818	3.6819	3.6820	3.6821
20 10	3.6821	3.6822	3.6823	3.6824	3.6825	3.6826	3.6827	3.6828	3.6829	3.6830
20 20	3.6830	3.6831	3.6832	3.6833	3.6834	3.6835	3.6836	3.6837	3.6838	3.6839
20 30	3.6839	3.6840	3.6841	3.6842	3.6843	3.6844	3.6845	3.6846	3.6847	3.6848
20 40	3.6848	3.6849	3.6850	3.6851	3.6852	3.6853	3.6854	3.6855	3.6856	3.6857
20 50	3.6857	3.6858	3.6859	3.6860	3.6861	3.6862	3.6863	3.6864	3.6865	3.6866
1 21 0	3.6866	3.6867	3.6868	3.6869	3.6870	3.6871	3.6872	3.6873	3.6874	3.6875
21 10	3.6875	3.6876	3.6877	3.6878	3.6879	3.6880	3.6881	3.6882	3.6883	3.6884
21 20	3.6884	3.6885	3.6886	3.6887	3.6888	3.6889	3.6890	3.6891	3.6892	3.6893
21 30	3.6893	3.6894	3.6895	3.6896	3.6897	3.6898	3.6899	3.6900	3.6901	3.6902
21 40	3.6902	3.6903	3.6904	3.6905	3.6906	3.6907	3.6908	3.6909	3.6910	3.6911
21 50	3.6911	3.6912	3.6913	3.6914	3.6915	3.6916	3.6917	3.6918	3.6919	3.6920
1 22 0	3.6920	3.6921	3.6922	3.6923	3.6924	3.6925	3.6926	3.6927	3.6928	3.6929
22 10	3.6928	3.6929	3.6930	3.6931	3.6932	3.6933	3.6934	3.6935	3.6936	3.6937
22 20	3.6937	3.6938	3.6939	3.6940	3.6941	3.6942	3.6943	3.6944	3.6945	3.6946
22 30	3.6946	3.6947	3.6948	3.6949	3.6950	3.6951	3.6952	3.6953	3.6954	3.6955
22 40	3.6955	3.6956	3.6957	3.6958	3.6959	3.6960	3.6961	3.6962	3.6963	3.6964
22 50	3.6964	3.6965	3.6966	3.6967	3.6968	3.6969	3.6970	3.6971	3.6972	3.6973
1 23 0	3.6972	3.6973	3.6974	3.6975	3.6976	3.6977	3.6978	3.6979	3.6980	3.6981
23 10	3.6981	3.6982	3.6983	3.6984	3.6985	3.6986	3.6987	3.6988	3.6989	3.6990
23 20	3.6990	3.6991	3.6992	3.6993	3.6994	3.6995	3.6996	3.6997	3.6998	3.6999
23 30	3.6999	3.7000	3.7001	3.7002	3.7003	3.7004	3.7005	3.7006	3.7007	3.7008
23 40	3.7007	3.7008	3.7009	3.7010	3.7011	3.7012	3.7013	3.7014	3.7015	3.7016
23 50	3.7016	3.7017	3.7018	3.7019	3.7020	3.7021	3.7022	3.7023	3.7024	3.7025
1 24 0	3.7024	3.7025	3.7026	3.7027	3.7028	3.7029	3.7030	3.7031	3.7032	3.7033
24 10	3.7033	3.7034	3.7035	3.7036	3.7037	3.7038	3.7039	3.7040	3.7041	3.7042
24 20	3.7042	3.7043	3.7044	3.7045	3.7046	3.7047	3.7048	3.7049	3.7050	3.7051
24 30	3.7050	3.7051	3.7052	3.7053	3.7054	3.7055	3.7056	3.7057	3.7058	3.7059
24 40	3.7059	3.7060	3.7061	3.7062	3.7063	3.7064	3.7065	3.7066	3.7067	3.7068
24 50	3.7067	3.7068	3.7069	3.7070	3.7071	3.7072	3.7073	3.7074	3.7075	3.7076
1 25 0	3.7076	3.7077	3.7078	3.7079	3.7080	3.7081	3.7082	3.7083	3.7084	3.7085
25 10	3.7084	3.7085	3.7086	3.7087	3.7088	3.7089	3.7090	3.7091	3.7092	3.7093
25 20	3.7093	3.7094	3.7095	3.7096	3.7097	3.7098	3.7099	3.7100	3.7101	3.7102
25 30	3.7101	3.7102	3.7103	3.7104	3.7105	3.7106	3.7107	3.7108	3.7109	3.7110
25 40	3.7110	3.7111	3.7112	3.7113	3.7114	3.7115	3.7116	3.7117	3.7118	3.7119
25 50	3.7118	3.7119	3.7120	3.7121	3.7122	3.7123	3.7124	3.7125	3.7126	3.7127
1 26 0	3.7126	3.7127	3.7128	3.7129	3.7130	3.7131	3.7132	3.7133	3.7134	3.7135
26 10	3.7135	3.7136	3.7137	3.7138	3.7139	3.7140	3.7141	3.7142	3.7143	3.7144
26 20	3.7143	3.7144	3.7145	3.7146	3.7147	3.7148	3.7149	3.7150	3.7151	3.7152
26 30	3.7152	3.7153	3.7154	3.7155	3.7156	3.7157	3.7158	3.7159	3.7160	3.7161
26 40	3.7160	3.7161	3.7162	3.7163	3.7164	3.7165	3.7166	3.7167	3.7168	3.7169
26 50	3.7168	3.7169	3.7170	3.7171	3.7172	3.7173	3.7174	3.7175	3.7176	3.7177
1 27 0	3.7177	3.7178	3.7179	3.7180	3.7181	3.7182	3.7183	3.7184	3.7185	3.7186
27 10	3.7185	3.7186	3.7187	3.7188	3.7189	3.7190	3.7191	3.7192	3.7193	3.7194
27 20	3.7193	3.7194	3.7195	3.7196	3.7197	3.7198	3.7199	3.7200	3.7201	3.7202
27 30	3.7202	3.7203	3.7204	3.7205	3.7206	3.7207	3.7208	3.7209	3.7210	3.7211
27 40	3.7210	3.7211	3.7212	3.7213	3.7214	3.7215	3.7216	3.7217	3.7218	3.7219
27 50	3.7218	3.7219	3.7220	3.7221	3.7222	3.7223	3.7224	3.7225	3.7226	3.7227
1 28 0	3.7226	3.7227	3.7228	3.7229	3.7230	3.7231	3.7232	3.7233	3.7234	3.7235
28 10	3.7235	3.7236	3.7237	3.7238	3.7239	3.7240	3.7241	3.7242	3.7243	3.7244
28 20	3.7243	3.7244	3.7245	3.7246	3.7247	3.7248	3.7249	3.7250	3.7251	3.7252
28 30	3.7251	3.7252	3.7253	3.7254	3.7255	3.7256	3.7257	3.7258	3.7259	3.7260
28 40	3.7259	3.7260	3.7261	3.7262	3.7263	3.7264	3.7265	3.7266	3.7267	3.7268
28 50	3.7267	3.7268	3.7269	3.7270	3.7271	3.7272	3.7273	3.7274	3.7275	3.7276
1 29 0	3.7275	3.7276	3.7277	3.7278	3.7279	3.7280	3.7281	3.7282	3.7283	3.7284
29 10	3.7284	3.7285	3.7286	3.7287	3.7288	3.7289	3.7290	3.7291	3.7292	3.7293
29 20	3.7292	3.7293	3.7294	3.7295	3.7296	3.7297	3.7298	3.7299	3.7300	3.7301
29 30	3.7300	3.7301	3.7302	3.7303	3.7304	3.7305	3.7306	3.7307	3.7308	3.7309
29 40	3.7308	3.7309	3.7310	3.7311	3.7312	3.7313	3.7314	3.7315	3.7316	3.7317
29 50	3.7316	3.7317	3.7318	3.7319	3.7320	3.7321	3.7322	3.7323	3.7324	3.7325



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
1 <sup>h</sup> 30 <sup>m</sup> 0 <sup>s</sup>	3.7324	3.7325	3.7326	3.7326	3.7327	3.7328	3.7329	3.7330	3.7330	3.7331
30 10	3.7332	3.7333	3.7334	3.7334	3.7335	3.7336	3.7337	3.7338	3.7338	3.7339
30 20	3.7340	3.7341	3.7342	3.7342	3.7343	3.7344	3.7345	3.7346	3.7346	3.7347
30 30	3.7348	3.7349	3.7350	3.7350	3.7351	3.7352	3.7353	3.7354	3.7354	3.7355
30 40	3.7356	3.7357	3.7358	3.7358	3.7359	3.7360	3.7361	3.7362	3.7362	3.7363
30 50	3.7364	3.7365	3.7366	3.7366	3.7367	3.7368	3.7369	3.7370	3.7370	3.7371
1 31 0	3.7372	3.7373	3.7374	3.7374	3.7375	3.7376	3.7377	3.7377	3.7378	3.7379
31 10	3.7380	3.7381	3.7381	3.7382	3.7383	3.7384	3.7385	3.7385	3.7386	3.7387
31 20	3.7388	3.7389	3.7389	3.7390	3.7391	3.7392	3.7393	3.7393	3.7394	3.7395
31 30	3.7396	3.7397	3.7397	3.7398	3.7399	3.7400	3.7400	3.7401	3.7402	3.7403
31 40	3.7404	3.7404	3.7405	3.7406	3.7407	3.7408	3.7408	3.7409	3.7410	3.7411
31 50	3.7412	3.7412	3.7413	3.7414	3.7415	3.7415	3.7416	3.7417	3.7418	3.7419
1 32 0	3.7419	3.7420	3.7421	3.7422	3.7423	3.7423	3.7424	3.7425	3.7426	3.7426
32 10	3.7427	3.7428	3.7429	3.7430	3.7430	3.7431	3.7432	3.7433	3.7434	3.7434
32 20	3.7435	3.7436	3.7437	3.7437	3.7438	3.7439	3.7440	3.7441	3.7441	3.7442
32 30	3.7443	3.7444	3.7444	3.7445	3.7446	3.7447	3.7448	3.7448	3.7449	3.7450
32 40	3.7451	3.7452	3.7452	3.7453	3.7454	3.7455	3.7455	3.7456	3.7457	3.7458
32 50	3.7459	3.7459	3.7460	3.7461	3.7462	3.7462	3.7463	3.7464	3.7465	3.7466
1 33 0	3.7466	3.7467	3.7468	3.7469	3.7469	3.7470	3.7471	3.7472	3.7473	3.7473
33 10	3.7474	3.7475	3.7476	3.7476	3.7477	3.7478	3.7479	3.7480	3.7480	3.7481
33 20	3.7482	3.7483	3.7483	3.7484	3.7485	3.7486	3.7487	3.7487	3.7488	3.7489
33 30	3.7490	3.7490	3.7491	3.7492	3.7493	3.7493	3.7494	3.7495	3.7496	3.7497
33 40	3.7497	3.7498	3.7499	3.7500	3.7500	3.7501	3.7502	3.7503	3.7504	3.7504
33 50	3.7505	3.7506	3.7507	3.7507	3.7508	3.7509	3.7510	3.7510	3.7511	3.7512
1 34 0	3.7513	3.7514	3.7514	3.7515	3.7516	3.7517	3.7517	3.7518	3.7519	3.7520
34 10	3.7520	3.7521	3.7522	3.7523	3.7524	3.7524	3.7525	3.7526	3.7527	3.7527
34 20	3.7528	3.7529	3.7530	3.7530	3.7531	3.7532	3.7533	3.7534	3.7534	3.7535
34 30	3.7536	3.7537	3.7537	3.7538	3.7539	3.7540	3.7540	3.7541	3.7542	3.7543
34 40	3.7543	3.7544	3.7545	3.7546	3.7547	3.7547	3.7548	3.7549	3.7550	3.7550
34 50	3.7551	3.7552	3.7553	3.7553	3.7554	3.7555	3.7556	3.7556	3.7557	3.7558
1 35 0	3.7559	3.7560	3.7560	3.7561	3.7562	3.7563	3.7563	3.7564	3.7565	3.7566
35 10	3.7566	3.7567	3.7568	3.7569	3.7569	3.7570	3.7571	3.7572	3.7572	3.7573
35 20	3.7574	3.7575	3.7575	3.7576	3.7577	3.7578	3.7579	3.7579	3.7580	3.7581
35 30	3.7582	3.7582	3.7583	3.7584	3.7585	3.7585	3.7586	3.7587	3.7588	3.7588
35 40	3.7589	3.7590	3.7591	3.7591	3.7592	3.7593	3.7594	3.7594	3.7595	3.7596
35 50	3.7597	3.7597	3.7598	3.7599	3.7600	3.7600	3.7601	3.7602	3.7603	3.7603
1 36 0	3.7604	3.7605	3.7606	3.7606	3.7607	3.7608	3.7609	3.7609	3.7610	3.7611
36 10	3.7612	3.7613	3.7613	3.7614	3.7615	3.7616	3.7616	3.7617	3.7618	3.7619
36 20	3.7619	3.7620	3.7621	3.7622	3.7622	3.7623	3.7624	3.7625	3.7625	3.7626
36 30	3.7627	3.7628	3.7628	3.7629	3.7630	3.7631	3.7631	3.7632	3.7633	3.7634
36 40	3.7634	3.7635	3.7636	3.7637	3.7637	3.7638	3.7639	3.7640	3.7640	3.7641
36 50	3.7642	3.7643	3.7643	3.7644	3.7645	3.7645	3.7646	3.7647	3.7648	3.7648
1 37 0	3.7649	3.7650	3.7651	3.7651	3.7652	3.7653	3.7654	3.7654	3.7655	3.7656
37 10	3.7657	3.7657	3.7658	3.7659	3.7660	3.7660	3.7661	3.7662	3.7663	3.7663
37 20	3.7664	3.7665	3.7666	3.7666	3.7667	3.7668	3.7669	3.7669	3.7670	3.7671
37 30	3.7672	3.7672	3.7673	3.7674	3.7675	3.7675	3.7676	3.7677	3.7677	3.7678
37 40	3.7679	3.7680	3.7681	3.7681	3.7682	3.7683	3.7683	3.7684	3.7685	3.7686
37 50	3.7686	3.7687	3.7688	3.7689	3.7689	3.7690	3.7691	3.7692	3.7692	3.7693
1 38 0	3.7694	3.7695	3.7695	3.7696	3.7697	3.7697	3.7698	3.7699	3.7700	3.7700
38 10	3.7701	3.7702	3.7703	3.7703	3.7704	3.7705	3.7706	3.7706	3.7707	3.7708
38 20	3.7709	3.7709	3.7710	3.7711	3.7711	3.7712	3.7713	3.7714	3.7714	3.7715
38 30	3.7716	3.7717	3.7717	3.7718	3.7719	3.7720	3.7720	3.7721	3.7722	3.7722
38 40	3.7723	3.7724	3.7725	3.7725	3.7726	3.7727	3.7728	3.7728	3.7729	3.7730
38 50	3.7731	3.7731	3.7732	3.7733	3.7733	3.7734	3.7735	3.7736	3.7736	3.7737
1 39 0	3.7738	3.7739	3.7739	3.7740	3.7741	3.7742	3.7742	3.7743	3.7744	3.7744
39 10	3.7745	3.7746	3.7747	3.7747	3.7748	3.7749	3.7750	3.7750	3.7751	3.7752
39 20	3.7752	3.7753	3.7754	3.7755	3.7755	3.7756	3.7757	3.7758	3.7758	3.7759
39 30	3.7760	3.7760	3.7761	3.7762	3.7763	3.7763	3.7764	3.7765	3.7766	3.7766
39 40	3.7767	3.7768	3.7768	3.7769	3.7770	3.7771	3.7771	3.7772	3.7773	3.7774
39 50	3.7774	3.7775	3.7776	3.7776	3.7777	3.7778	3.7779	3.7779	3.7780	3.7781



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
<sup>o</sup> 1 <sup>b</sup> 0 <sup>m</sup> 0 <sup>s</sup>	3.7782	3.7782	3.7783	3.7784	3.7784	3.7785	3.7786	3.7787	3.7787	3.7788
40 10	3.7789	3.7789	3.7790	3.7791	3.7792	3.7792	3.7793	3.7794	3.7795	3.7795
40 20	3.7796	3.7797	3.7797	3.7798	3.7799	3.7800	3.7800	3.7801	3.7802	3.7802
40 30	3.7803	3.7804	3.7805	3.7805	3.7806	3.7807	3.7807	3.7808	3.7809	3.7810
40 40	3.7810	3.7811	3.7812	3.7813	3.7813	3.7814	3.7815	3.7815	3.7816	3.7817
40 50	3.7818	3.7818	3.7819	3.7820	3.7820	3.7821	3.7822	3.7823	3.7823	3.7824
1 41 0	3.7825	3.7825	3.7826	3.7827	3.7828	3.7828	3.7829	3.7830	3.7830	3.7831
41 10	3.7832	3.7833	3.7833	3.7834	3.7835	3.7835	3.7836	3.7837	3.7838	3.7838
41 20	3.7839	3.7840	3.7840	3.7841	3.7842	3.7843	3.7843	3.7844	3.7845	3.7845
41 30	3.7846	3.7847	3.7848	3.7848	3.7849	3.7850	3.7850	3.7851	3.7852	3.7853
41 40	3.7853	3.7854	3.7855	3.7855	3.7856	3.7857	3.7858	3.7858	3.7859	3.7860
41 50	3.7860	3.7861	3.7862	3.7863	3.7863	3.7864	3.7865	3.7865	3.7866	3.7867
1 42 0	3.7868	3.7868	3.7869	3.7870	3.7870	3.7871	3.7872	3.7872	3.7873	3.7874
42 10	3.7875	3.7875	3.7876	3.7877	3.7877	3.7878	3.7879	3.7880	3.7880	3.7881
42 20	3.7882	3.7882	3.7883	3.7884	3.7885	3.7885	3.7886	3.7887	3.7887	3.7888
42 30	3.7889	3.7889	3.7890	3.7891	3.7892	3.7892	3.7893	3.7894	3.7894	3.7895
42 40	3.7896	3.7897	3.7897	3.7898	3.7899	3.7899	3.7900	3.7901	3.7901	3.7902
42 50	3.7903	3.7904	3.7904	3.7905	3.7906	3.7906	3.7907	3.7908	3.7908	3.7909
1 43 0	3.7910	3.7911	3.7911	3.7912	3.7913	3.7913	3.7914	3.7915	3.7916	3.7916
43 10	3.7917	3.7918	3.7918	3.7919	3.7920	3.7920	3.7921	3.7922	3.7923	3.7923
43 20	3.7924	3.7925	3.7925	3.7926	3.7927	3.7927	3.7928	3.7929	3.7930	3.7930
43 30	3.7931	3.7932	3.7932	3.7933	3.7934	3.7934	3.7935	3.7936	3.7937	3.7937
43 40	3.7938	3.7939	3.7939	3.7940	3.7941	3.7941	3.7942	3.7943	3.7943	3.7944
43 50	3.7945	3.7946	3.7946	3.7947	3.7948	3.7948	3.7949	3.7950	3.7950	3.7951
1 44 0	3.7952	3.7953	3.7953	3.7954	3.7955	3.7955	3.7956	3.7957	3.7957	3.7958
44 10	3.7959	3.7959	3.7960	3.7961	3.7962	3.7962	3.7963	3.7964	3.7964	3.7965
44 20	3.7966	3.7966	3.7967	3.7968	3.7969	3.7969	3.7970	3.7971	3.7971	3.7972
44 30	3.7973	3.7973	3.7974	3.7975	3.7975	3.7976	3.7977	3.7978	3.7978	3.7979
44 40	3.7980	3.7980	3.7981	3.7982	3.7982	3.7983	3.7984	3.7984	3.7985	3.7986
44 50	3.7987	3.7987	3.7988	3.7989	3.7989	3.7990	3.7991	3.7991	3.7992	3.7993
1 45 0	3.7993	3.7994	3.7995	3.7995	3.7996	3.7997	3.7998	3.7998	3.7999	3.8000
45 10	3.8000	3.8001	3.8002	3.8002	3.8003	3.8004	3.8004	3.8005	3.8006	3.8006
45 20	3.8007	3.8008	3.8009	3.8009	3.8010	3.8011	3.8011	3.8012	3.8013	3.8013
45 30	3.8014	3.8015	3.8015	3.8016	3.8017	3.8017	3.8018	3.8019	3.8020	3.8020
45 40	3.8021	3.8022	3.8022	3.8023	3.8024	3.8024	3.8025	3.8026	3.8026	3.8027
45 50	3.8028	3.8028	3.8029	3.8030	3.8030	3.8031	3.8032	3.8033	3.8033	3.8034
1 46 0	3.8035	3.8035	3.8036	3.8036	3.8037	3.8038	3.8039	3.8039	3.8040	3.8041
46 10	3.8041	3.8042	3.8043	3.8043	3.8044	3.8045	3.8045	3.8046	3.8047	3.8048
46 20	3.8048	3.8049	3.8050	3.8050	3.8051	3.8052	3.8052	3.8053	3.8054	3.8054
46 30	3.8055	3.8056	3.8056	3.8057	3.8058	3.8058	3.8059	3.8060	3.8060	3.8061
46 40	3.8062	3.8062	3.8063	3.8064	3.8065	3.8065	3.8066	3.8067	3.8067	3.8068
46 50	3.8069	3.8069	3.8070	3.8071	3.8071	3.8072	3.8073	3.8073	3.8074	3.8075
1 47 0	3.8075	3.8076	3.8077	3.8077	3.8078	3.8079	3.8079	3.8080	3.8081	3.8081
47 10	3.8082	3.8083	3.8083	3.8084	3.8085	3.8085	3.8086	3.8087	3.8088	3.8088
47 20	3.8089	3.8090	3.8090	3.8091	3.8092	3.8092	3.8093	3.8094	3.8094	3.8095
47 30	3.8096	3.8096	3.8097	3.8098	3.8098	3.8099	3.8099	3.8100	3.8101	3.8102
47 40	3.8102	3.8103	3.8104	3.8104	3.8105	3.8106	3.8106	3.8107	3.8108	3.8108
47 50	3.8109	3.8110	3.8110	3.8111	3.8112	3.8112	3.8113	3.8114	3.8114	3.8115
1 48 0	3.8116	3.8116	3.8117	3.8118	3.8118	3.8119	3.8120	3.8120	3.8121	3.8122
48 10	3.8122	3.8123	3.8124	3.8124	3.8125	3.8126	3.8126	3.8127	3.8128	3.8128
48 20	3.8129	3.8130	3.8130	3.8131	3.8132	3.8132	3.8133	3.8134	3.8134	3.8135
48 30	3.8136	3.8136	3.8137	3.8138	3.8138	3.8139	3.8140	3.8140	3.8141	3.8142
48 40	3.8142	3.8143	3.8144	3.8144	3.8145	3.8146	3.8146	3.8147	3.8148	3.8148
48 50	3.8149	3.8150	3.8150	3.8151	3.8152	3.8152	3.8153	3.8154	3.8154	3.8155
1 49 0	3.8156	3.8156	3.8157	3.8158	3.8158	3.8159	3.8160	3.8160	3.8161	3.8162
49 10	3.8162	3.8163	3.8164	3.8164	3.8165	3.8166	3.8166	3.8167	3.8168	3.8168
49 20	3.8169	3.8170	3.8170	3.8171	3.8172	3.8172	3.8173	3.8174	3.8174	3.8175
49 30	3.8176	3.8176	3.8177	3.8178	3.8178	3.8179	3.8180	3.8180	3.8181	3.8182
49 40	3.8182	3.8183	3.8184	3.8184	3.8185	3.8185	3.8186	3.8187	3.8188	3.8188
49 50	3.8189	3.8190	3.8190	3.8191	3.8191	3.8192	3.8193	3.8193	3.8194	3.8195

# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
1 <sup>h</sup> 50 <sup>m</sup> 0 <sup>s</sup>	3.8195	3.8196	3.8197	3.8197	3.8198	3.8199	3.8199	3.8200	3.8201	3.8201
50 10	3.8202	3.8203	3.8203	3.8204	3.8205	3.8205	3.8206	3.8207	3.8207	3.8208
50 20	3.8209	3.8209	3.8210	3.8211	3.8211	3.8212	3.8213	3.8213	3.8214	3.8214
50 30	3.8215	3.8216	3.8216	3.8217	3.8218	3.8218	3.8219	3.8220	3.8220	3.8221
50 40	3.8222	3.8222	3.8223	3.8224	3.8224	3.8225	3.8226	3.8226	3.8227	3.8228
50 50	3.8228	3.8229	3.8230	3.8230	3.8231	3.8231	3.8232	3.8233	3.8233	3.8234
1 51 0	3.8235	3.8235	3.8236	3.8237	3.8237	3.8238	3.8239	3.8239	3.8240	3.8241
51 10	3.8241	3.8242	3.8243	3.8243	3.8244	3.8245	3.8245	3.8246	3.8246	3.8247
51 20	3.8248	3.8248	3.8249	3.8250	3.8250	3.8251	3.8252	3.8252	3.8253	3.8254
51 30	3.8254	3.8255	3.8256	3.8256	3.8257	3.8258	3.8258	3.8259	3.8259	3.8260
51 40	3.8261	3.8261	3.8262	3.8263	3.8263	3.8264	3.8265	3.8265	3.8266	3.8267
51 50	3.8267	3.8268	3.8269	3.8269	3.8270	3.8270	3.8271	3.8272	3.8272	3.8273
1 52 0	3.8274	3.8274	3.8275	3.8276	3.8276	3.8277	3.8278	3.8278	3.8279	3.8280
52 10	3.8280	3.8281	3.8281	3.8282	3.8283	3.8283	3.8284	3.8285	3.8285	3.8286
52 20	3.8287	3.8287	3.8288	3.8289	3.8289	3.8290	3.8290	3.8291	3.8292	3.8292
52 30	3.8293	3.8294	3.8294	3.8295	3.8296	3.8296	3.8297	3.8298	3.8298	3.8299
52 40	3.8299	3.8300	3.8301	3.8301	3.8302	3.8303	3.8303	3.8304	3.8305	3.8305
52 50	3.8306	3.8307	3.8307	3.8308	3.8308	3.8309	3.8310	3.8310	3.8311	3.8312
1 53 0	3.8312	3.8313	3.8314	3.8314	3.8315	3.8315	3.8316	3.8317	3.8317	3.8318
53 10	3.8319	3.8319	3.8320	3.8321	3.8321	3.8322	3.8323	3.8323	3.8324	3.8324
53 20	3.8325	3.8326	3.8326	3.8327	3.8328	3.8328	3.8329	3.8330	3.8330	3.8331
53 30	3.8331	3.8332	3.8333	3.8333	3.8334	3.8335	3.8335	3.8336	3.8337	3.8337
53 40	3.8338	3.8338	3.8339	3.8340	3.8340	3.8341	3.8342	3.8342	3.8343	3.8344
53 50	3.8344	3.8345	3.8345	3.8346	3.8347	3.8347	3.8348	3.8349	3.8349	3.8350
1 54 0	3.8351	3.8351	3.8352	3.8352	3.8353	3.8354	3.8354	3.8355	3.8356	3.8356
54 10	3.8357	3.8358	3.8358	3.8359	3.8359	3.8360	3.8361	3.8361	3.8362	3.8363
54 20	3.8363	3.8364	3.8365	3.8365	3.8366	3.8366	3.8367	3.8368	3.8368	3.8369
54 30	3.8370	3.8370	3.8371	3.8371	3.8372	3.8373	3.8373	3.8374	3.8375	3.8375
54 40	3.8376	3.8377	3.8377	3.8378	3.8378	3.8379	3.8380	3.8380	3.8381	3.8382
54 50	3.8382	3.8383	3.8383	3.8384	3.8385	3.8385	3.8386	3.8387	3.8387	3.8388
1 55 0	3.8388	3.8389	3.8390	3.8390	3.8391	3.8392	3.8392	3.8393	3.8394	3.8394
55 10	3.8395	3.8395	3.8396	3.8397	3.8397	3.8398	3.8399	3.8399	3.8400	3.8400
55 20	3.8401	3.8402	3.8402	3.8403	3.8404	3.8404	3.8405	3.8405	3.8406	3.8407
55 30	3.8407	3.8408	3.8409	3.8409	3.8410	3.8410	3.8411	3.8412	3.8412	3.8413
55 40	3.8414	3.8414	3.8415	3.8415	3.8416	3.8417	3.8417	3.8418	3.8419	3.8419
55 50	3.8420	3.8420	3.8421	3.8422	3.8422	3.8423	3.8424	3.8424	3.8425	3.8425
1 56 0	3.8426	3.8427	3.8427	3.8428	3.8429	3.8429	3.8430	3.8430	3.8431	3.8432
56 10	3.8432	3.8433	3.8434	3.8434	3.8435	3.8435	3.8436	3.8437	3.8437	3.8438
56 20	3.8439	3.8439	3.8440	3.8440	3.8441	3.8442	3.8442	3.8443	3.8444	3.8444
56 30	3.8445	3.8445	3.8446	3.8447	3.8447	3.8448	3.8448	3.8449	3.8450	3.8450
56 40	3.8451	3.8452	3.8452	3.8453	3.8453	3.8454	3.8455	3.8455	3.8456	3.8457
56 50	3.8457	3.8458	3.8458	3.8459	3.8460	3.8460	3.8461	3.8462	3.8462	3.8463
1 57 0	3.8463	3.8464	3.8465	3.8465	3.8466	3.8466	3.8467	3.8468	3.8468	3.8469
57 10	3.8470	3.8470	3.8471	3.8471	3.8472	3.8473	3.8473	3.8474	3.8474	3.8475
57 20	3.8476	3.8476	3.8477	3.8478	3.8478	3.8479	3.8479	3.8480	3.8481	3.8481
57 30	3.8482	3.8483	3.8483	3.8484	3.8484	3.8485	3.8486	3.8486	3.8487	3.8487
57 40	3.8488	3.8489	3.8489	3.8490	3.8491	3.8491	3.8492	3.8492	3.8493	3.8494
57 50	3.8494	3.8495	3.8495	3.8496	3.8497	3.8497	3.8498	3.8499	3.8499	3.8500
1 58 0	3.8500	3.8501	3.8502	3.8502	3.8503	3.8503	3.8504	3.8505	3.8505	3.8506
58 10	3.8506	3.8507	3.8508	3.8508	3.8509	3.8510	3.8510	3.8511	3.8511	3.8512
58 20	3.8513	3.8513	3.8514	3.8514	3.8515	3.8516	3.8516	3.8517	3.8517	3.8518
58 30	3.8519	3.8519	3.8520	3.8521	3.8521	3.8522	3.8522	3.8523	3.8524	3.8524
58 40	3.8525	3.8525	3.8526	3.8527	3.8527	3.8528	3.8528	3.8529	3.8530	3.8530
58 50	3.8531	3.8532	3.8532	3.8533	3.8533	3.8534	3.8535	3.8535	3.8536	3.8536
1 59 0	3.8537	3.8538	3.8538	3.8539	3.8539	3.8540	3.8541	3.8541	3.8542	3.8542
59 10	3.8543	3.8544	3.8544	3.8545	3.8545	3.8546	3.8547	3.8547	3.8548	3.8549
59 20	3.8549	3.8550	3.8550	3.8551	3.8552	3.8552	3.8553	3.8553	3.8554	3.8555
59 30	3.8555	3.8556	3.8556	3.8557	3.8558	3.8558	3.8559	3.8559	3.8560	3.8561
59 40	3.8561	3.8562	3.8562	3.8563	3.8564	3.8564	3.8565	3.8565	3.8566	3.8567
59 50	3.8567	3.8568	3.8568	3.8569	3.8570	3.8570	3.8571	3.8572	3.8572	3.8573



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0 <sup>h</sup> . 0 <sup>m</sup> . 0 <sup>s</sup> .	3.8573	3.8574	3.8575	3.8575	3.8576	3.8576	3.8577	3.8578	3.8578	3.8579
0 10	3.8579	3.8580	3.8581	3.8581	3.8582	3.8582	3.8583	3.8584	3.8584	3.8585
0 20	3.8585	3.8586	3.8587	3.8587	3.8588	3.8588	3.8589	3.8590	3.8590	3.8591
0 30	3.8591	3.8592	3.8593	3.8593	3.8594	3.8594	3.8595	3.8596	3.8596	3.8597
0 40	3.8597	3.8598	3.8599	3.8599	3.8600	3.8600	3.8601	3.8602	3.8602	3.8603
0 50	3.8603	3.8604	3.8605	3.8605	3.8606	3.8606	3.8607	3.8608	3.8608	3.8609
2 1 0	3.8609	3.8610	3.8611	3.8611	3.8612	3.8612	3.8613	3.8614	3.8614	3.8615
1 10	3.8615	3.8616	3.8617	3.8617	3.8618	3.8618	3.8619	3.8620	3.8620	3.8621
1 20	3.8621	3.8622	3.8623	3.8623	3.8624	3.8624	3.8625	3.8625	3.8626	3.8627
1 30	3.8627	3.8628	3.8628	3.8629	3.8630	3.8630	3.8631	3.8631	3.8632	3.8633
1 40	3.8633	3.8634	3.8634	3.8635	3.8636	3.8636	3.8637	3.8637	3.8638	3.8639
1 50	3.8639	3.8640	3.8640	3.8641	3.8642	3.8642	3.8643	3.8643	3.8644	3.8645
2 2 0	3.8645	3.8646	3.8646	3.8647	3.8647	3.8648	3.8649	3.8649	3.8650	3.8650
2 10	3.8651	3.8652	3.8652	3.8653	3.8653	3.8654	3.8655	3.8655	3.8656	3.8656
2 20	3.8657	3.8658	3.8658	3.8659	3.8659	3.8660	3.8661	3.8661	3.8662	3.8662
2 30	3.8663	3.8663	3.8664	3.8665	3.8665	3.8666	3.8666	3.8667	3.8668	3.8668
2 40	3.8669	3.8669	3.8670	3.8671	3.8671	3.8672	3.8672	3.8673	3.8673	3.8674
2 50	3.8675	3.8675	3.8676	3.8676	3.8677	3.8678	3.8678	3.8679	3.8679	3.8680
2 3 0	3.8681	3.8681	3.8682	3.8682	3.8683	3.8684	3.8684	3.8685	3.8685	3.8686
3 10	3.8686	3.8687	3.8688	3.8688	3.8689	3.8689	3.8690	3.8691	3.8691	3.8692
3 20	3.8692	3.8693	3.8693	3.8694	3.8695	3.8695	3.8696	3.8696	3.8697	3.8698
3 30	3.8698	3.8699	3.8699	3.8700	3.8701	3.8701	3.8702	3.8702	3.8703	3.8703
3 40	3.8704	3.8705	3.8705	3.8706	3.8706	3.8707	3.8708	3.8708	3.8709	3.8709
3 50	3.8710	3.8710	3.8711	3.8712	3.8712	3.8713	3.8713	3.8714	3.8715	3.8715
2 4 0	3.8716	3.8716	3.8717	3.8717	3.8718	3.8719	3.8719	3.8720	3.8720	3.8721
4 10	3.8722	3.8722	3.8723	3.8723	3.8724	3.8724	3.8725	3.8726	3.8726	3.8727
4 20	3.8727	3.8728	3.8729	3.8729	3.8730	3.8730	3.8731	3.8731	3.8732	3.8733
4 30	3.8733	3.8734	3.8734	3.8735	3.8736	3.8736	3.8737	3.8737	3.8738	3.8738
4 40	3.8739	3.8740	3.8740	3.8741	3.8741	3.8742	3.8742	3.8743	3.8744	3.8744
4 50	3.8745	3.8745	3.8746	3.8747	3.8747	3.8748	3.8748	3.8749	3.8749	3.8750
2 5 0	3.8751	3.8751	3.8752	3.8752	3.8753	3.8754	3.8754	3.8755	3.8755	3.8756
5 10	3.8756	3.8757	3.8758	3.8758	3.8759	3.8759	3.8760	3.8760	3.8761	3.8762
5 20	3.8762	3.8763	3.8763	3.8764	3.8764	3.8765	3.8766	3.8766	3.8767	3.8767
5 30	3.8768	3.8769	3.8769	3.8770	3.8770	3.8771	3.8771	3.8772	3.8773	3.8773
5 40	3.8774	3.8774	3.8775	3.8775	3.8776	3.8777	3.8777	3.8778	3.8778	3.8779
5 50	3.8779	3.8780	3.8781	3.8781	3.8782	3.8782	3.8783	3.8783	3.8784	3.8785
2 6 0	3.8785	3.8786	3.8786	3.8787	3.8788	3.8788	3.8789	3.8789	3.8790	3.8790
6 10	3.8791	3.8792	3.8792	3.8793	3.8793	3.8794	3.8794	3.8795	3.8796	3.8796
6 20	3.8797	3.8797	3.8798	3.8798	3.8799	3.8800	3.8800	3.8801	3.8801	3.8802
6 30	3.8802	3.8803	3.8804	3.8804	3.8805	3.8805	3.8806	3.8806	3.8807	3.8808
6 40	3.8808	3.8809	3.8809	3.8810	3.8810	3.8811	3.8812	3.8812	3.8813	3.8813
6 50	3.8814	3.8814	3.8815	3.8816	3.8816	3.8817	3.8817	3.8818	3.8818	3.8819
2 7 0	3.8820	3.8820	3.8821	3.8821	3.8822	3.8822	3.8823	3.8824	3.8824	3.8825
7 10	3.8825	3.8826	3.8826	3.8827	3.8828	3.8828	3.8829	3.8829	3.8830	3.8830
7 20	3.8831	3.8832	3.8832	3.8833	3.8833	3.8834	3.8834	3.8835	3.8835	3.8836
7 30	3.8837	3.8837	3.8838	3.8838	3.8839	3.8839	3.8840	3.8841	3.8841	3.8842
7 40	3.8842	3.8843	3.8843	3.8844	3.8845	3.8845	3.8846	3.8846	3.8847	3.8847
7 50	3.8848	3.8849	3.8849	3.8850	3.8850	3.8851	3.8851	3.8852	3.8852	3.8853
2 8 0	3.8854	3.8854	3.8855	3.8855	3.8856	3.8856	3.8857	3.8858	3.8858	3.8859
8 10	3.8859	3.8860	3.8860	3.8861	3.8862	3.8862	3.8863	3.8863	3.8864	3.8864
8 20	3.8865	3.8865	3.8866	3.8867	3.8867	3.8868	3.8868	3.8869	3.8869	3.8870
8 30	3.8871	3.8871	3.8872	3.8872	3.8873	3.8873	3.8874	3.8874	3.8875	3.8876
8 40	3.8876	3.8877	3.8877	3.8878	3.8878	3.8879	3.8880	3.8880	3.8881	3.8881
8 50	3.8882	3.8882	3.8883	3.8883	3.8884	3.8885	3.8885	3.8886	3.8886	3.8887
2 9 0	3.8887	3.8888	3.8889	3.8889	3.8890	3.8890	3.8891	3.8891	3.8892	3.8892
9 10	3.8893	3.8894	3.8894	3.8895	3.8895	3.8896	3.8896	3.8897	3.8897	3.8898
9 20	3.8899	3.8899	3.8900	3.8900	3.8901	3.8901	3.8902	3.8903	3.8903	3.8904
9 30	3.8904	3.8905	3.8905	3.8906	3.8906	3.8907	3.8908	3.8908	3.8909	3.8909
9 40	3.8910	3.8910	3.8911	3.8911	3.8912	3.8912	3.8913	3.8914	3.8914	3.8915
9 50	3.8915	3.8916	3.8916	3.8917	3.8918	3.8918	3.8919	3.8919	3.8920	3.8920



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
$2^h 10^m 0^s$	3.8921	3.8922	3.8922	3.8923	3.8923	3.8924	3.8924	3.8925	3.8925	3.8926
10 10	3.8927	3.8927	3.8928	3.8928	3.8929	3.8929	3.8930	3.8930	3.8931	3.8932
10 20	3.8932	3.8933	3.8933	3.8934	3.8934	3.8935	3.8935	3.8936	3.8937	3.8937
10 30	3.8938	3.8938	3.8939	3.8939	3.8940	3.8940	3.8941	3.8941	3.8942	3.8943
10 40	3.8943	3.8944	3.8944	3.8945	3.8945	3.8946	3.8946	3.8947	3.8948	3.8948
10 50	3.8949	3.8949	3.8950	3.8950	3.8951	3.8951	3.8952	3.8953	3.8953	3.8954
2 11 0	3.8954	3.8955	3.8955	3.8956	3.8956	3.8957	3.8958	3.8958	3.8959	3.8959
11 10	3.8960	3.8960	3.8961	3.8961	3.8962	3.8963	3.8963	3.8964	3.8964	3.8965
11 20	3.8965	3.8966	3.8966	3.8967	3.8967	3.8968	3.8969	3.8969	3.8970	3.8970
11 30	3.8971	3.8971	3.8972	3.8972	3.8973	3.8974	3.8974	3.8975	3.8975	3.8976
11 40	3.8976	3.8977	3.8977	3.8978	3.8978	3.8979	3.8980	3.8980	3.8981	3.8981
11 50	3.8982	3.8982	3.8983	3.8983	3.8984	3.8985	3.8985	3.8986	3.8986	3.8987
2 12 0	3.8987	3.8988	3.8988	3.8989	3.8989	3.8990	3.8991	3.8991	3.8992	3.8992
12 10	3.8993	3.8993	3.8994	3.8994	3.8995	3.8995	3.8996	3.8997	3.8997	3.8998
12 20	3.8998	3.8999	3.8999	3.9000	3.9000	3.9001	3.9001	3.9002	3.9003	3.9003
12 30	3.9004	3.9004	3.9005	3.9005	3.9006	3.9006	3.9007	3.9007	3.9008	3.9009
12 40	3.9009	3.9010	3.9010	3.9011	3.9011	3.9012	3.9012	3.9013	3.9013	3.9014
12 50	3.9015	3.9015	3.9016	3.9016	3.9017	3.9017	3.9018	3.9018	3.9019	3.9019
2 13 0	3.9020	3.9021	3.9021	3.9022	3.9022	3.9023	3.9023	3.9024	3.9024	3.9025
13 10	3.9025	3.9026	3.9027	3.9027	3.9028	3.9028	3.9029	3.9029	3.9030	3.9030
13 20	3.9031	3.9031	3.9032	3.9033	3.9033	3.9034	3.9034	3.9035	3.9035	3.9036
13 30	3.9036	3.9037	3.9037	3.9038	3.9038	3.9039	3.9040	3.9040	3.9041	3.9041
13 40	3.9042	3.9042	3.9043	3.9043	3.9044	3.9044	3.9045	3.9046	3.9046	3.9047
13 50	3.9047	3.9048	3.9048	3.9049	3.9049	3.9050	3.9050	3.9051	3.9051	3.9052
2 14 0	3.9053	3.9053	3.9054	3.9054	3.9055	3.9055	3.9056	3.9056	3.9057	3.9057
14 10	3.9058	3.9058	3.9059	3.9060	3.9060	3.9061	3.9061	3.9062	3.9062	3.9063
14 20	3.9063	3.9064	3.9064	3.9065	3.9066	3.9066	3.9067	3.9067	3.9068	3.9068
14 30	3.9069	3.9069	3.9070	3.9070	3.9071	3.9071	3.9072	3.9073	3.9073	3.9074
14 40	3.9074	3.9075	3.9075	3.9076	3.9076	3.9077	3.9077	3.9078	3.9078	3.9079
14 50	3.9079	3.9080	3.9081	3.9081	3.9082	3.9082	3.9083	3.9083	3.9084	3.9084
2 15 0	3.9085	3.9085	3.9086	3.9086	3.9087	3.9088	3.9088	3.9089	3.9089	3.9090
15 10	3.9090	3.9091	3.9091	3.9092	3.9092	3.9093	3.9093	3.9094	3.9094	3.9095
15 20	3.9096	3.9096	3.9097	3.9097	3.9098	3.9098	3.9099	3.9099	3.9100	3.9100
15 30	3.9101	3.9101	3.9102	3.9103	3.9103	3.9104	3.9104	3.9105	3.9105	3.9106
15 40	3.9106	3.9107	3.9107	3.9108	3.9108	3.9109	3.9109	3.9110	3.9111	3.9111
15 50	3.9112	3.9112	3.9113	3.9113	3.9114	3.9114	3.9115	3.9115	3.9116	3.9116
2 16 0	3.9117	3.9117	3.9118	3.9118	3.9119	3.9120	3.9120	3.9121	3.9121	3.9122
16 10	3.9122	3.9123	3.9123	3.9124	3.9124	3.9125	3.9125	3.9126	3.9126	3.9127
16 20	3.9128	3.9128	3.9129	3.9129	3.9130	3.9130	3.9131	3.9131	3.9132	3.9132
16 30	3.9133	3.9133	3.9134	3.9134	3.9135	3.9135	3.9136	3.9137	3.9137	3.9138
16 40	3.9138	3.9139	3.9139	3.9140	3.9140	3.9141	3.9141	3.9142	3.9142	3.9143
16 50	3.9143	3.9144	3.9144	3.9145	3.9146	3.9146	3.9147	3.9147	3.9148	3.9148
2 17 0	3.9149	3.9149	3.9150	3.9150	3.9151	3.9151	3.9152	3.9152	3.9153	3.9153
17 10	3.9154	3.9155	3.9155	3.9156	3.9156	3.9157	3.9157	3.9158	3.9158	3.9159
17 20	3.9159	3.9160	3.9160	3.9161	3.9161	3.9162	3.9162	3.9163	3.9163	3.9164
17 30	3.9165	3.9165	3.9166	3.9166	3.9167	3.9167	3.9168	3.9168	3.9169	3.9169
17 40	3.9170	3.9170	3.9171	3.9171	3.9172	3.9172	3.9173	3.9173	3.9174	3.9175
17 50	3.9175	3.9176	3.9176	3.9177	3.9177	3.9178	3.9178	3.9179	3.9179	3.9180
2 18 0	3.9180	3.9181	3.9181	3.9182	3.9182	3.9183	3.9183	3.9184	3.9184	3.9185
18 10	3.9186	3.9186	3.9187	3.9187	3.9188	3.9188	3.9189	3.9189	3.9190	3.9190
18 20	3.9191	3.9191	3.9192	3.9192	3.9193	3.9193	3.9194	3.9194	3.9195	3.9195
18 30	3.9196	3.9197	3.9197	3.9198	3.9198	3.9199	3.9199	3.9200	3.9200	3.9201
18 40	3.9201	3.9202	3.9202	3.9203	3.9203	3.9204	3.9204	3.9205	3.9205	3.9206
18 50	3.9206	3.9207	3.9207	3.9208	3.9209	3.9209	3.9210	3.9210	3.9211	3.9211
2 19 0	3.9212	3.9212	3.9213	3.9213	3.9214	3.9214	3.9215	3.9215	3.9216	3.9216
19 10	3.9217	3.9217	3.9218	3.9218	3.9219	3.9219	3.9220	3.9221	3.9221	3.9222
19 20	3.9222	3.9223	3.9223	3.9224	3.9224	3.9225	3.9225	3.9226	3.9226	3.9227
19 30	3.9227	3.9228	3.9228	3.9229	3.9229	3.9230	3.9230	3.9231	3.9231	3.9232
19 40	3.9232	3.9233	3.9233	3.9234	3.9235	3.9235	3.9236	3.9236	3.9237	3.9237
19 50	3.9238	3.9238	3.9239	3.9239	3.9240	3.9240	3.9241	3.9241	3.9242	3.9242

# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
2 <sup>h</sup> . 20 <sup>m</sup> . 0 <sup>s</sup> .	3.9243	3.9243	3.9244	3.9244	3.9245	3.9245	3.9246	3.9246	3.9247	3.9247
20 10	3.9248	3.9248	3.9249	3.9250	3.9250	3.9251	3.9251	3.9252	3.9252	3.9253
20 20	3.9253	3.9254	3.9254	3.9255	3.9255	3.9256	3.9256	3.9257	3.9257	3.9258
20 30	3.9258	3.9259	3.9259	3.9260	3.9260	3.9261	3.9261	3.9262	3.9262	3.9263
20 40	3.9263	3.9264	3.9264	3.9265	3.9265	3.9266	3.9267	3.9267	3.9268	3.9268
20 50	3.9269	3.9269	3.9270	3.9270	3.9271	3.9271	3.9272	3.9272	3.9273	3.9273
2 21 0	3.9274	3.9274	3.9275	3.9275	3.9276	3.9276	3.9277	3.9277	3.9278	3.9278
21 10	3.9279	3.9279	3.9280	3.9280	3.9281	3.9281	3.9282	3.9282	3.9283	3.9283
21 20	3.9284	3.9284	3.9285	3.9285	3.9286	3.9287	3.9287	3.9288	3.9288	3.9289
21 30	3.9289	3.9290	3.9290	3.9291	3.9291	3.9292	3.9292	3.9293	3.9293	3.9294
21 40	3.9294	3.9295	3.9295	3.9296	3.9296	3.9297	3.9297	3.9298	3.9298	3.9299
21 50	3.9299	3.9300	3.9300	3.9301	3.9301	3.9302	3.9302	3.9303	3.9303	3.9304
2 22 0	3.9304	3.9305	3.9305	3.9306	3.9306	3.9307	3.9307	3.9308	3.9308	3.9309
22 10	3.9309	3.9310	3.9311	3.9311	3.9312	3.9312	3.9313	3.9313	3.9314	3.9314
22 20	3.9315	3.9315	3.9316	3.9316	3.9317	3.9317	3.9318	3.9318	3.9319	3.9319
22 30	3.9320	3.9320	3.9321	3.9321	3.9322	3.9322	3.9323	3.9323	3.9324	3.9324
22 40	3.9325	3.9325	3.9326	3.9326	3.9327	3.9327	3.9328	3.9328	3.9329	3.9329
22 50	3.9330	3.9330	3.9331	3.9331	3.9332	3.9332	3.9333	3.9333	3.9334	3.9334
2 23 0	3.9335	3.9335	3.9336	3.9336	3.9337	3.9337	3.9338	3.9338	3.9339	3.9339
23 10	3.9340	3.9340	3.9341	3.9341	3.9342	3.9342	3.9343	3.9343	3.9344	3.9344
23 20	3.9345	3.9345	3.9346	3.9346	3.9347	3.9348	3.9348	3.9349	3.9349	3.9350
23 30	3.9350	3.9351	3.9351	3.9352	3.9352	3.9353	3.9353	3.9354	3.9354	3.9355
23 40	3.9355	3.9356	3.9356	3.9357	3.9357	3.9358	3.9358	3.9359	3.9359	3.9360
23 50	3.9360	3.9361	3.9361	3.9362	3.9362	3.9363	3.9363	3.9364	3.9364	3.9365
2 24 0	3.9365	3.9366	3.9366	3.9367	3.9367	3.9368	3.9368	3.9369	3.9369	3.9370
24 10	3.9370	3.9371	3.9371	3.9372	3.9372	3.9373	3.9373	3.9374	3.9374	3.9375
24 20	3.9375	3.9376	3.9376	3.9377	3.9377	3.9378	3.9378	3.9379	3.9379	3.9380
24 30	3.9380	3.9381	3.9381	3.9382	3.9382	3.9383	3.9383	3.9384	3.9384	3.9385
24 40	3.9385	3.9386	3.9386	3.9387	3.9387	3.9388	3.9388	3.9389	3.9389	3.9390
24 50	3.9390	3.9391	3.9391	3.9392	3.9392	3.9393	3.9393	3.9394	3.9394	3.9395
2 25 0	3.9395	3.9396	3.9396	3.9397	3.9397	3.9398	3.9398	3.9399	3.9399	3.9400
25 10	3.9400	3.9401	3.9401	3.9402	3.9402	3.9403	3.9403	3.9404	3.9404	3.9405
25 20	3.9405	3.9406	3.9406	3.9407	3.9407	3.9408	3.9408	3.9409	3.9409	3.9410
25 30	3.9410	3.9411	3.9411	3.9412	3.9412	3.9413	3.9413	3.9414	3.9414	3.9415
25 40	3.9415	3.9416	3.9416	3.9417	3.9417	3.9418	3.9418	3.9419	3.9419	3.9420
25 50	3.9420	3.9421	3.9421	3.9422	3.9422	3.9423	3.9423	3.9424	3.9424	3.9425
2 26 0	3.9425	3.9426	3.9426	3.9427	3.9427	3.9428	3.9428	3.9429	3.9429	3.9430
26 10	3.9430	3.9430	3.9431	3.9431	3.9432	3.9432	3.9433	3.9433	3.9434	3.9434
26 20	3.9435	3.9435	3.9436	3.9436	3.9437	3.9437	3.9438	3.9438	3.9439	3.9439
26 30	3.9440	3.9440	3.9441	3.9441	3.9442	3.9442	3.9443	3.9443	3.9444	3.9444
26 40	3.9445	3.9445	3.9446	3.9446	3.9447	3.9447	3.9448	3.9448	3.9449	3.9449
26 50	3.9450	3.9450	3.9451	3.9451	3.9452	3.9452	3.9453	3.9453	3.9454	3.9454
2 27 0	3.9455	3.9455	3.9456	3.9456	3.9457	3.9457	3.9458	3.9458	3.9459	3.9459
27 10	3.9460	3.9460	3.9461	3.9461	3.9462	3.9462	3.9463	3.9463	3.9464	3.9464
27 20	3.9465	3.9465	3.9466	3.9466	3.9467	3.9467	3.9468	3.9468	3.9469	3.9469
27 30	3.9469	3.9470	3.9470	3.9471	3.9471	3.9472	3.9472	3.9473	3.9473	3.9474
27 40	3.9474	3.9475	3.9475	3.9476	3.9476	3.9477	3.9477	3.9478	3.9478	3.9479
27 50	3.9479	3.9480	3.9480	3.9481	3.9481	3.9482	3.9482	3.9483	3.9483	3.9484
2 28 0	3.9484	3.9485	3.9485	3.9486	3.9486	3.9487	3.9487	3.9488	3.9488	3.9489
28 10	3.9489	3.9490	3.9490	3.9490	3.9491	3.9491	3.9492	3.9492	3.9493	3.9493
28 20	3.9494	3.9494	3.9495	3.9495	3.9496	3.9496	3.9497	3.9497	3.9498	3.9498
28 30	3.9499	3.9499	3.9500	3.9500	3.9501	3.9501	3.9502	3.9502	3.9503	3.9503
28 40	3.9504	3.9504	3.9505	3.9505	3.9506	3.9506	3.9507	3.9507	3.9508	3.9508
28 50	3.9509	3.9509	3.9509	3.9510	3.9510	3.9511	3.9511	3.9512	3.9512	3.9513
2 29 0	3.9513	3.9514	3.9514	3.9515	3.9515	3.9516	3.9516	3.9517	3.9517	3.9518
29 10	3.9518	3.9519	3.9519	3.9520	3.9520	3.9521	3.9521	3.9522	3.9522	3.9523
29 20	3.9523	3.9524	3.9524	3.9525	3.9525	3.9526	3.9526	3.9527	3.9527	3.9528
29 30	3.9528	3.9528	3.9529	3.9529	3.9530	3.9530	3.9531	3.9531	3.9532	3.9532
29 40	3.9533	3.9533	3.9534	3.9534	3.9535	3.9535	3.9536	3.9536	3.9537	3.9537
29 50	3.9538	3.9538	3.9539	3.9539	3.9540	3.9540	3.9541	3.9541	3.9542	3.9542



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
0 <sup>h</sup> . 30 <sup>m</sup> . 0 <sup>s</sup> .	3.9542	3.9543	3.9543	3.9544	3.9544	3.9545	3.9545	3.9546	3.9546	3.9547
30 10	3.9547	3.9548	3.9548	3.9549	3.9549	3.9550	3.9550	3.9551	3.9551	3.9552
30 20	3.9552	3.9553	3.9553	3.9554	3.9554	3.9554	3.9555	3.9555	3.9556	3.9556
30 30	3.9557	3.9557	3.9558	3.9558	3.9559	3.9559	3.9560	3.9560	3.9561	3.9561
30 40	3.9562	3.9562	3.9563	3.9563	3.9564	3.9564	3.9565	3.9565	3.9566	3.9566
30 50	3.9566	3.9567	3.9567	3.9568	3.9568	3.9569	3.9569	3.9570	3.9570	3.9571
2 31 0	3.9571	3.9572	3.9572	3.9573	3.9573	3.9574	3.9574	3.9575	3.9575	3.9576
31 10	3.9576	3.9577	3.9577	3.9578	3.9578	3.9578	3.9579	3.9579	3.9580	3.9580
31 20	3.9581	3.9581	3.9582	3.9582	3.9583	3.9583	3.9584	3.9584	3.9585	3.9585
31 30	3.9586	3.9586	3.9587	3.9587	3.9588	3.9588	3.9589	3.9589	3.9589	3.9590
31 40	3.9590	3.9591	3.9591	3.9592	3.9592	3.9593	3.9593	3.9594	3.9594	3.9595
31 50	3.9595	3.9596	3.9596	3.9597	3.9597	3.9598	3.9598	3.9599	3.9599	3.9599
2 32 0	3.9600	3.9600	3.9601	3.9601	3.9602	3.9602	3.9603	3.9603	3.9604	3.9604
32 10	3.9605	3.9605	3.9606	3.9606	3.9607	3.9607	3.9608	3.9608	3.9609	3.9609
32 20	3.9609	3.9610	3.9610	3.9611	3.9611	3.9612	3.9612	3.9613	3.9613	3.9614
32 30	3.9614	3.9615	3.9615	3.9616	3.9616	3.9617	3.9617	3.9618	3.9618	3.9618
32 40	3.9619	3.9619	3.9620	3.9620	3.9621	3.9621	3.9622	3.9622	3.9623	3.9623
32 50	3.9624	3.9624	3.9625	3.9625	3.9626	3.9626	3.9627	3.9627	3.9627	3.9628
2 33 0	3.9628	3.9629	3.9629	3.9630	3.9630	3.9631	3.9631	3.9632	3.9632	3.9633
33 10	3.9633	3.9634	3.9634	3.9634	3.9635	3.9635	3.9636	3.9636	3.9637	3.9637
33 20	3.9638	3.9638	3.9639	3.9639	3.9640	3.9640	3.9641	3.9641	3.9642	3.9642
33 30	3.9642	3.9643	3.9643	3.9644	3.9644	3.9645	3.9645	3.9646	3.9646	3.9647
33 40	3.9647	3.9648	3.9648	3.9649	3.9649	3.9650	3.9650	3.9651	3.9651	3.9652
33 50	3.9652	3.9653	3.9653	3.9653	3.9654	3.9654	3.9655	3.9655	3.9656	3.9656
2 34 0	3.9657	3.9657	3.9658	3.9658	3.9658	3.9659	3.9659	3.9660	3.9660	3.9661
34 10	3.9661	3.9662	3.9662	3.9663	3.9663	3.9664	3.9664	3.9665	3.9665	3.9665
34 20	3.9666	3.9666	3.9667	3.9667	3.9668	3.9668	3.9669	3.9669	3.9670	3.9670
34 30	3.9671	3.9671	3.9672	3.9672	3.9672	3.9673	3.9673	3.9674	3.9674	3.9675
34 40	3.9675	3.9676	3.9676	3.9677	3.9677	3.9678	3.9678	3.9679	3.9679	3.9680
34 50	3.9680	3.9681	3.9681	3.9682	3.9682	3.9683	3.9683	3.9683	3.9684	3.9684
2 35 0	3.9685	3.9685	3.9686	3.9686	3.9687	3.9687	3.9688	3.9688	3.9689	3.9689
35 10	3.9689	3.9690	3.9690	3.9691	3.9691	3.9692	3.9692	3.9693	3.9693	3.9694
35 20	3.9694	3.9695	3.9695	3.9696	3.9696	3.9696	3.9697	3.9697	3.9698	3.9698
35 30	3.9699	3.9699	3.9700	3.9700	3.9701	3.9701	3.9702	3.9702	3.9703	3.9703
35 40	3.9703	3.9704	3.9704	3.9705	3.9705	3.9706	3.9706	3.9707	3.9707	3.9708
35 50	3.9708	3.9709	3.9709	3.9710	3.9710	3.9710	3.9711	3.9711	3.9712	3.9712
2 36 0	3.9713	3.9713	3.9714	3.9714	3.9715	3.9715	3.9716	3.9716	3.9716	3.9717
36 10	3.9717	3.9718	3.9718	3.9719	3.9719	3.9720	3.9720	3.9721	3.9721	3.9722
36 20	3.9722	3.9722	3.9723	3.9723	3.9724	3.9724	3.9725	3.9725	3.9726	3.9726
36 30	3.9727	3.9727	3.9728	3.9728	3.9729	3.9729	3.9729	3.9730	3.9730	3.9731
36 40	3.9731	3.9732	3.9732	3.9733	3.9733	3.9734	3.9734	3.9735	3.9735	3.9735
36 50	3.9736	3.9736	3.9737	3.9737	3.9738	3.9738	3.9739	3.9739	3.9740	3.9740
2 37 0	3.9741	3.9741	3.9741	3.9742	3.9742	3.9743	3.9743	3.9744	3.9744	3.9745
37 10	3.9745	3.9746	3.9746	3.9746	3.9747	3.9747	3.9748	3.9748	3.9749	3.9749
37 20	3.9750	3.9750	3.9751	3.9751	3.9752	3.9752	3.9752	3.9753	3.9753	3.9754
37 30	3.9754	3.9755	3.9755	3.9756	3.9756	3.9757	3.9757	3.9758	3.9758	3.9758
37 40	3.9759	3.9759	3.9760	3.9760	3.9761	3.9761	3.9762	3.9762	3.9763	3.9763
37 50	3.9763	3.9764	3.9764	3.9765	3.9765	3.9766	3.9766	3.9767	3.9767	3.9768
2 38 0	3.9768	3.9769	3.9769	3.9769	3.9770	3.9770	3.9771	3.9771	3.9772	3.9772
38 10	3.9773	3.9773	3.9774	3.9774	3.9774	3.9775	3.9775	3.9776	3.9776	3.9777
38 20	3.9777	3.9778	3.9778	3.9779	3.9779	3.9779	3.9780	3.9780	3.9781	3.9781
38 30	3.9782	3.9782	3.9783	3.9783	3.9784	3.9784	3.9785	3.9785	3.9785	3.9786
38 40	3.9786	3.9787	3.9787	3.9788	3.9788	3.9789	3.9789	3.9790	3.9790	3.9790
38 50	3.9791	3.9791	3.9792	3.9792	3.9793	3.9793	3.9794	3.9794	3.9795	3.9795
2 39 0	3.9795	3.9796	3.9796	3.9797	3.9797	3.9798	3.9798	3.9799	3.9799	3.9800
39 10	3.9800	3.9800	3.9801	3.9801	3.9802	3.9802	3.9803	3.9803	3.9804	3.9804
39 20	3.9805	3.9805	3.9805	3.9806	3.9806	3.9807	3.9807	3.9808	3.9808	3.9809
39 30	3.9809	3.9810	3.9810	3.9810	3.9811	3.9811	3.9812	3.9812	3.9813	3.9813
39 40	3.9814	3.9814	3.9815	3.9815	3.9815	3.9816	3.9816	3.9817	3.9817	3.9818
39 50	3.9818	3.9819	3.9819	3.9819	3.9820	3.9820	3.9821	3.9821	3.9822	3.9822



# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
2 <sup>h</sup> .40 <sup>m</sup> .0 <sup>s</sup>	3.9823	3.9823	3.9824	3.9824	3.9825	3.9825	3.9825	3.9826	3.9826	3.9827
40 10	3.9827	3.9828	3.9828	3.9829	3.9829	3.9829	3.9830	3.9830	3.9831	3.9831
40 20	3.9832	3.9832	3.9833	3.9833	3.9834	3.9834	3.9834	3.9835	3.9835	3.9836
40 30	3.9836	3.9837	3.9837	3.9838	3.9838	3.9839	3.9839	3.9839	3.9840	3.9840
40 40	3.9841	3.9841	3.9842	3.9842	3.9843	3.9843	3.9843	3.9844	3.9844	3.9845
40 50	3.9845	3.9846	3.9846	3.9847	3.9847	3.9848	3.9848	3.9848	3.9849	3.9849
2 41 0	3.9850	3.9850	3.9851	3.9851	3.9852	3.9852	3.9852	3.9853	3.9853	3.9854
41 10	3.9854	3.9855	3.9855	3.9856	3.9856	3.9857	3.9857	3.9857	3.9858	3.9858
41 20	3.9859	3.9859	3.9860	3.9860	3.9861	3.9861	3.9861	3.9862	3.9862	3.9863
41 30	3.9863	3.9864	3.9864	3.9865	3.9865	3.9865	3.9866	3.9866	3.9867	3.9867
41 40	3.9868	3.9868	3.9869	3.9869	3.9870	3.9870	3.9870	3.9871	3.9871	3.9872
41 50	3.9872	3.9873	3.9873	3.9874	3.9874	3.9874	3.9875	3.9875	3.9876	3.9876
2 42 0	3.9877	3.9877	3.9878	3.9878	3.9878	3.9879	3.9879	3.9880	3.9880	3.9881
42 10	3.9881	3.9882	3.9882	3.9882	3.9883	3.9883	3.9884	3.9884	3.9885	3.9885
42 20	3.9886	3.9886	3.9886	3.9887	3.9887	3.9888	3.9888	3.9889	3.9889	3.9890
42 30	3.9890	3.9890	3.9891	3.9891	3.9892	3.9892	3.9893	3.9893	3.9894	3.9894
42 40	3.9894	3.9895	3.9895	3.9896	3.9896	3.9897	3.9897	3.9898	3.9898	3.9898
42 50	3.9899	3.9899	3.9900	3.9900	3.9901	3.9901	3.9902	3.9902	3.9903	3.9903
2 43 0	3.9903	3.9904	3.9904	3.9905	3.9905	3.9906	3.9906	3.9906	3.9907	3.9907
43 10	3.9908	3.9908	3.9909	3.9909	3.9910	3.9910	3.9910	3.9911	3.9911	3.9912
43 20	3.9912	3.9913	3.9913	3.9914	3.9914	3.9914	3.9915	3.9915	3.9916	3.9916
43 30	3.9917	3.9917	3.9918	3.9918	3.9918	3.9919	3.9919	3.9920	3.9920	3.9921
43 40	3.9921	3.9922	3.9922	3.9922	3.9923	3.9923	3.9924	3.9924	3.9925	3.9925
43 50	3.9926	3.9926	3.9926	3.9927	3.9927	3.9928	3.9928	3.9929	3.9929	3.9930
2 44 0	3.9930	3.9930	3.9931	3.9931	3.9932	3.9932	3.9933	3.9933	3.9933	3.9934
44 10	3.9934	3.9935	3.9935	3.9936	3.9936	3.9937	3.9937	3.9937	3.9938	3.9938
44 20	3.9939	3.9939	3.9940	3.9940	3.9941	3.9941	3.9941	3.9942	3.9942	3.9943
44 30	3.9943	3.9944	3.9944	3.9944	3.9945	3.9945	3.9946	3.9946	3.9947	3.9947
44 40	3.9948	3.9948	3.9948	3.9949	3.9949	3.9950	3.9950	3.9951	3.9951	3.9952
44 50	3.9952	3.9952	3.9953	3.9953	3.9954	3.9954	3.9955	3.9955	3.9955	3.9956
2 45 0	3.9956	3.9957	3.9957	3.9958	3.9958	3.9959	3.9959	3.9959	3.9960	3.9960
45 10	3.9961	3.9961	3.9962	3.9962	3.9962	3.9963	3.9963	3.9964	3.9964	3.9965
45 20	3.9965	3.9966	3.9966	3.9966	3.9967	3.9967	3.9968	3.9968	3.9969	3.9969
45 30	3.9969	3.9970	3.9970	3.9971	3.9971	3.9972	3.9972	3.9973	3.9973	3.9973
45 40	3.9974	3.9974	3.9975	3.9975	3.9976	3.9976	3.9976	3.9977	3.9977	3.9978
45 50	3.9978	3.9979	3.9979	3.9980	3.9980	3.9980	3.9981	3.9981	3.9982	3.9982
2 46 0	3.9983	3.9983	3.9983	3.9984	3.9984	3.9985	3.9985	3.9986	3.9986	3.9987
46 10	3.9987	3.9987	3.9988	3.9988	3.9989	3.9989	3.9990	3.9990	3.9990	3.9991
46 20	3.9991	3.9992	3.9992	3.9993	3.9993	3.9993	3.9994	3.9994	3.9995	3.9995
46 30	3.9996	3.9996	3.9997	3.9997	3.9997	3.9998	3.9998	3.9999	3.9999	4.0000
46 40	4.0000	4.0000	4.0001	4.0001	4.0002	4.0002	4.0003	4.0003	4.0003	4.0004
46 50	4.0004	4.0005	4.0005	4.0006	4.0006	4.0007	4.0007	4.0007	4.0008	4.0008
2 47 0	4.0009	4.0009	4.0010	4.0010	4.0010	4.0011	4.0011	4.0012	4.0012	4.0013
47 10	4.0013	4.0013	4.0014	4.0014	4.0015	4.0015	4.0016	4.0016	4.0016	4.0017
47 20	4.0017	4.0018	4.0018	4.0019	4.0019	4.0019	4.0020	4.0020	4.0021	4.0021
47 30	4.0022	4.0022	4.0023	4.0023	4.0023	4.0024	4.0024	4.0025	4.0025	4.0026
47 40	4.0026	4.0026	4.0027	4.0027	4.0028	4.0028	4.0029	4.0029	4.0029	4.0030
47 50	4.0030	4.0031	4.0031	4.0032	4.0032	4.0032	4.0033	4.0033	4.0034	4.0034
2 48 0	4.0035	4.0035	4.0035	4.0036	4.0036	4.0037	4.0037	4.0038	4.0038	4.0038
48 10	4.0039	4.0039	4.0040	4.0040	4.0041	4.0041	4.0041	4.0042	4.0042	4.0043
48 20	4.0043	4.0044	4.0044	4.0045	4.0045	4.0045	4.0046	4.0046	4.0047	4.0047
48 30	4.0048	4.0048	4.0048	4.0049	4.0049	4.0050	4.0050	4.0051	4.0051	4.0051
48 40	4.0052	4.0052	4.0053	4.0053	4.0054	4.0054	4.0054	4.0055	4.0055	4.0056
48 50	4.0056	4.0057	4.0057	4.0057	4.0058	4.0058	4.0059	4.0059	4.0060	4.0060
2 49 0	4.0060	4.0061	4.0061	4.0062	4.0062	4.0063	4.0063	4.0063	4.0064	4.0064
49 10	4.0065	4.0065	4.0066	4.0066	4.0066	4.0067	4.0067	4.0068	4.0068	4.0069
49 20	4.0069	4.0069	4.0070	4.0070	4.0071	4.0071	4.0072	4.0072	4.0072	4.0073
49 30	4.0073	4.0074	4.0074	4.0074	4.0075	4.0075	4.0076	4.0076	4.0077	4.0077
49 40	4.0077	4.0078	4.0078	4.0079	4.0079	4.0080	4.0080	4.0080	4.0081	4.0081
49 50	4.0082	4.0082	4.0083	4.0083	4.0083	4.0084	4.0084	4.0085	4.0085	4.0086

# TABLE IX.

## LOGARITHMS OF SMALL ARCS IN SPACE OR TIME.

Arc.	0	1	2	3	4	5	6	7	8	9
<sup>o</sup> 2 <sup>h</sup> .50 <sup>m</sup> . <sup>''</sup> 0 <sup>s</sup>	4.0086	4.0086	4.0087	4.0087	4.0088	4.0088	4.0089	4.0089	4.0089	4.0090
50 10	4.0090	4.0091	4.0091	4.0092	4.0092	4.0092	4.0093	4.0093	4.0094	4.0094
50 20	4.0095	4.0095	4.0095	4.0096	4.0096	4.0097	4.0097	4.0097	4.0098	4.0098
50 30	4.0099	4.0099	4.0100	4.0100	4.0100	4.0101	4.0101	4.0102	4.0102	4.0103
50 40	4.0103	4.0103	4.0104	4.0104	4.0105	4.0105	4.0106	4.0106	4.0106	4.0107
50 50	4.0107	4.0108	4.0108	4.0109	4.0109	4.0109	4.0110	4.0110	4.0111	4.0111
2 51 0	4.0111	4.0112	4.0112	4.0113	4.0113	4.0114	4.0114	4.0114	4.0115	4.0115
51 10	4.0116	4.0116	4.0117	4.0117	4.0117	4.0118	4.0118	4.0119	4.0119	4.0120
51 20	4.0120	4.0120	4.0121	4.0121	4.0122	4.0122	4.0122	4.0123	4.0123	4.0124
51 30	4.0124	4.0125	4.0125	4.0125	4.0126	4.0126	4.0127	4.0127	4.0128	4.0128
51 40	4.0128	4.0129	4.0129	4.0130	4.0130	4.0130	4.0131	4.0131	4.0132	4.0132
51 50	4.0133	4.0133	4.0133	4.0134	4.0134	4.0135	4.0135	4.0136	4.0136	4.0136
2 52 0	4.0137	4.0137	4.0138	4.0138	4.0138	4.0139	4.0139	4.0140	4.0140	4.0141
52 10	4.0141	4.0141	4.0142	4.0142	4.0143	4.0143	4.0144	4.0144	4.0144	4.0145
52 20	4.0145	4.0146	4.0146	4.0146	4.0147	4.0147	4.0148	4.0148	4.0149	4.0149
52 30	4.0149	4.0150	4.0150	4.0151	4.0151	4.0152	4.0152	4.0153	4.0153	4.0153
52 40	4.0154	4.0154	4.0154	4.0155	4.0155	4.0156	4.0156	4.0157	4.0157	4.0157
52 50	4.0158	4.0158	4.0159	4.0159	4.0159	4.0160	4.0160	4.0161	4.0161	4.0162
2 53 0	4.0162	4.0162	4.0163	4.0163	4.0164	4.0164	4.0164	4.0165	4.0165	4.0166
53 10	4.0166	4.0167	4.0167	4.0167	4.0168	4.0168	4.0169	4.0169	4.0169	4.0170
53 20	4.0170	4.0171	4.0171	4.0172	4.0172	4.0172	4.0173	4.0173	4.0174	4.0174
53 30	4.0175	4.0175	4.0175	4.0176	4.0176	4.0177	4.0177	4.0177	4.0178	4.0178
53 40	4.0179	4.0179	4.0180	4.0180	4.0180	4.0181	4.0181	4.0182	4.0182	4.0182
53 50	4.0183	4.0183	4.0184	4.0184	4.0185	4.0185	4.0185	4.0186	4.0186	4.0187
2 54 0	4.0187	4.0187	4.0188	4.0188	4.0189	4.0189	4.0190	4.0190	4.0190	4.0191
54 10	4.0191	4.0192	4.0192	4.0192	4.0193	4.0193	4.0194	4.0194	4.0194	4.0195
54 20	4.0195	4.0196	4.0196	4.0197	4.0197	4.0197	4.0198	4.0198	4.0199	4.0199
54 30	4.0199	4.0200	4.0200	4.0201	4.0201	4.0202	4.0202	4.0202	4.0203	4.0203
54 40	4.0204	4.0204	4.0204	4.0205	4.0205	4.0206	4.0206	4.0207	4.0207	4.0207
54 50	4.0208	4.0208	4.0209	4.0209	4.0209	4.0210	4.0210	4.0211	4.0211	4.0211
2 55 0	4.0212	4.0212	4.0213	4.0213	4.0214	4.0214	4.0214	4.0215	4.0215	4.0216
55 10	4.0216	4.0216	4.0217	4.0217	4.0218	4.0218	4.0219	4.0219	4.0219	4.0220
55 20	4.0220	4.0221	4.0221	4.0221	4.0222	4.0222	4.0223	4.0223	4.0223	4.0224
55 30	4.0224	4.0225	4.0225	4.0225	4.0226	4.0226	4.0227	4.0227	4.0228	4.0228
55 40	4.0228	4.0229	4.0229	4.0230	4.0230	4.0230	4.0231	4.0231	4.0232	4.0232
55 50	4.0233	4.0233	4.0233	4.0234	4.0234	4.0235	4.0235	4.0235	4.0236	4.0236
2 56 0	4.0237	4.0237	4.0237	4.0238	4.0238	4.0239	4.0239	4.0240	4.0240	4.0240
56 10	4.0241	4.0241	4.0242	4.0242	4.0242	4.0243	4.0243	4.0244	4.0244	4.0244
56 20	4.0245	4.0245	4.0246	4.0246	4.0246	4.0247	4.0247	4.0248	4.0248	4.0249
56 30	4.0249	4.0249	4.0250	4.0250	4.0251	4.0251	4.0251	4.0252	4.0252	4.0253
56 40	4.0253	4.0253	4.0254	4.0254	4.0255	4.0255	4.0256	4.0256	4.0256	4.0257
56 50	4.0257	4.0258	4.0258	4.0258	4.0259	4.0259	4.0260	4.0260	4.0260	4.0261
2 57 0	4.0261	4.0262	4.0262	4.0262	4.0263	4.0263	4.0264	4.0264	4.0265	4.0265
57 10	4.0265	4.0266	4.0266	4.0267	4.0267	4.0267	4.0268	4.0268	4.0269	4.0269
57 20	4.0269	4.0270	4.0270	4.0271	4.0271	4.0271	4.0272	4.0272	4.0273	4.0273
57 30	4.0273	4.0274	4.0274	4.0275	4.0275	4.0276	4.0276	4.0276	4.0277	4.0277
57 40	4.0278	4.0278	4.0278	4.0279	4.0279	4.0280	4.0280	4.0280	4.0281	4.0281
57 50	4.0282	4.0282	4.0282	4.0283	4.0283	4.0284	4.0284	4.0284	4.0285	4.0285
2 58 0	4.0286	4.0286	4.0287	4.0287	4.0287	4.0288	4.0288	4.0289	4.0289	4.0289
58 10	4.0290	4.0290	4.0291	4.0291	4.0291	4.0292	4.0292	4.0293	4.0293	4.0293
58 20	4.0294	4.0294	4.0295	4.0295	4.0295	4.0296	4.0296	4.0297	4.0297	4.0297
58 30	4.0298	4.0298	4.0299	4.0299	4.0300	4.0300	4.0300	4.0301	4.0301	4.0302
58 40	4.0302	4.0302	4.0303	4.0303	4.0304	4.0304	4.0304	4.0305	4.0305	4.0306
58 50	4.0306	4.0306	4.0307	4.0307	4.0308	4.0308	4.0308	4.0309	4.0309	4.0310
2 59 0	4.0310	4.0310	4.0311	4.0311	4.0312	4.0312	4.0312	4.0313	4.0313	4.0314
59 10	4.0314	4.0314	4.0315	4.0315	4.0316	4.0316	4.0317	4.0317	4.0317	4.0318
59 20	4.0318	4.0319	4.0319	4.0319	4.0320	4.0320	4.0321	4.0321	4.0321	4.0322
59 30	4.0322	4.0323	4.0323	4.0323	4.0324	4.0324	4.0325	4.0325	4.0325	4.0326
59 40	4.0326	4.0327	4.0327	4.0327	4.0328	4.0328	4.0329	4.0329	4.0329	4.0330
59 50	4.0330	4.0331	4.0331	4.0331	4.0332	4.0332	4.0333	4.0333	4.0333	4.0334



# TABLE X.

TABLE, SHOWING THE CORRECTION REQUIRED, ON ACCOUNT OF  
SECOND DIFFERENCES OF THE MOON'S MOTION, IN FINDING  
THE GREENWICH TIME CORRESPONDING TO A  
CORRECTED LUNAR DISTANCE.

Approximate Interval.		Difference of the Proportional Logarithms in the Ephemeris.																											
		2	4	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	42	44	46	48	50	52		
h. m.	h. m.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.		
0 0	3 0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0		
0 10	2 50	0	0	0	1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2	3	3	3	3	3	3	3		
0 20	2 40	0	1	1	1	1	2	2	2	2	2	3	3	3	3	4	4	4	4	5	5	5	5	6	6	6	6		
0 30	2 30	0	1	1	2	2	2	2	3	3	3	4	4	5	5	5	6	6	6	7	7	7	8	8	8	9	9		
0 40	2 20	0	1	1	2	2	3	3	3	4	4	5	5	6	6	6	7	7	8	8	9	9	10	10	10	11	11		
0 50	2 10	1	1	2	2	3	3	4	4	5	5	5	6	6	7	7	8	8	9	9	10	10	11	12	12	13	13		
1 0	2 0	1	1	2	2	3	3	4	4	5	6	6	7	7	8	8	9	9	10	10	11	12	12	13	13	14	14		
1 10	1 50	1	1	2	2	3	4	4	5	5	6	6	7	8	8	9	9	10	11	11	12	12	13	14	14	15	15		
1 20	1 40	1	1	2	3	3	4	4	5	6	6	7	7	8	9	9	10	10	11	12	12	13	14	14	15	16	16		
1 30	1 30	1	1	2	3	3	4	4	5	6	6	7	8	8	9	9	10	11	11	12	12	13	14	14	15	16	16		

Difference of the Proportional Logarithms in the Ephemeris.																											
		54	56	58	60	62	64	66	68	70	72	74	76	78	80	82	84	86	88	90	92	94	96	98	100	102	
h. m.	h. m.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	
0 0	3 0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
0 10	2 50	4	4	4	4	4	4	4	4	5	5	5	5	5	5	5	6	6	6	6	6	6	6	7	7	7	
0 20	2 40	7	7	7	7	8	8	8	8	9	9	9	9	10	10	10	10	11	11	11	11	12	12	12	12	13	
0 30	2 30	9	10	10	10	11	11	12	12	12	13	13	13	14	14	14	14	15	15	16	16	16	17	17	17	18	
0 40	2 20	12	12	13	13	13	14	14	15	15	16	16	16	17	17	18	18	19	19	19	20	20	21	21	22	22	
0 50	2 10	14	14	15	15	16	16	16	17	17	18	19	19	20	20	21	21	22	22	22	23	23	24	24	25	26	
1 0	2 0	15	16	16	17	17	18	18	19	19	20	21	21	22	22	23	23	24	24	25	25	26	27	27	28	28	
1 10	1 50	16	17	17	18	18	19	19	20	21	21	22	22	23	24	24	25	25	26	27	27	28	28	29	30	30	
1 20	1 40	17	17	18	19	19	20	20	21	21	22	23	23	24	25	25	26	26	27	28	28	29	29	30	31	31	
1 30	1 30	17	18	18	19	19	20	21	21	22	23	23	24	24	25	25	26	27	27	28	29	29	30	31	31	32	

Difference of the Proportional Logarithms in the Ephemeris.															
		104	106	108	110	112	114	116	118	120	122	124	126	128	130
h. m.	h. m.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
0 0	3 0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
0 10	2 50	7	7	7	7	7	7	8	8	8	8	8	8	8	9
0 20	2 40	13	13	13	14	14	14	14	15	15	15	15	15	16	16
0 30	2 30	18	18	19	19	19	20	20	20	21	21	21	22	22	23
0 40	2 20	22	23	23	24	24	25	25	25	26	26	27	27	28	28
0 50	2 10	26	26	27	27	28	29	29	29	30	30	31	31	32	33
1 0	2 0	29	29	30	30	31	31	32	33	33	34	34	35	35	36
1 10	1 50	31	31	32	32	33	34	34	35	35	36	37	37	38	39
1 20	1 40	32	33	33	34	34	35	35	36	37	38	38	39	39	40
1 30	1 30	32	33	34	34	35	35	36	36	37	38	39	39	40	41

The Correction is to be *added* to the approximate Greenwich Time when the Proportional Logarithms in the Ephemeris are *decreasing*, and *subtracted* when they are *increasing*.



# TABLE XI.

LOG. N FOR DISTANCES FROM THE SUN.

1855.	0 <sup>h</sup> .	3 <sup>h</sup> .	6 <sup>h</sup> .	9 <sup>h</sup> .	12 <sup>h</sup> .	15 <sup>h</sup> .	18 <sup>h</sup> .	21 <sup>h</sup> .	1855.	0 <sup>h</sup> .	3 <sup>h</sup> .	6 <sup>h</sup> .	9 <sup>h</sup> .	12 <sup>h</sup> .	15 <sup>h</sup> .	18 <sup>h</sup> .	21 <sup>h</sup> .
Jan. 7	—0.76	.78	.79	.80	.81	.82	.83	.84	Apr. 10	+0.66	.69	.72	.74	.77	.79	.81	.83
8	0.85	.86	.87	.87	.88	.89	.89	.90	11	0.85	0.86	0.88	0.89	0.91	0.92	0.93	0.95
9	0.90	.91	.91	.91	.92	.92	.92	.92	12	0.96	0.97	0.98	0.99	1.00	1.01	1.02	1.02
10	0.92	.92	.92	.92	.92	.92	.92	.92	13	+1.03	1.04	1.04	1.05	1.06	1.06	1.07	1.07
11	0.92	.91	.91	.91	.90	.90	.89	.89	18	—0.80	0.78	0.77	0.76	0.75	0.73	0.71	0.69
12	—0.88	.87	.86	.85	.84	.83	.82	.81	19	—0.67	.65	.63	.60	.58	.55	.52	.48
13	0.80	.78	.77	.75	.73	.71	.69	.66	20	—0.46	.41	.36	.31	.25	.18	.10	.00
14	0.63	.60	.56	.52	.48	.42	.36	.28	22	+0.12	.20	.26	.32	.37	.41	.45	.49
20	0.71	.74	.76	.79	.81	.83	.84	.85	23	0.52	.55	.58	.60	.63	.65	.67	.69
21	0.87	.88	.89	.89	.90	.91	.91	.92	24	0.71	.73	.75	.76	.78	.79	.80	.82
22	—0.92	.93	.93	.93	.93	.93	.93	.93	25	+0.83	.84	.85	.86	.87	.88	.89	.90
23	0.93	.93	.93	.92	.92	.91	.91	.90	26	0.91	.92	.93	.93	.94	.95	0.95	0.96
24	0.90	.89	.89	.88	.87	.86	.85	.84	27	0.97	0.97	0.98	.98	.99	.99	0.99	1.00
25	0.83	.82	.81	.80	.78	.77	.75	.74	May 6	9.77	9.96	0.10	.20	.28	.35	0.40	0.46
26	0.72	.70	.68	.66	.64	.61	.59	.56	7	0.50	0.54	0.58	.61	.64	.67	.70	.72
27	—0.53	.49	.45	.41	.37	.31	.25	.18	8	+0.75	.77	.79	.80	.82	.84	.85	.87
Feb. 6	0.88	.88	.88	.89	.89	.89	.89	.89	9	0.88	.89	.90	.92	.93	.94	0.94	0.95
7	0.89	.89	.89	.89	.89	.88	.88	.88	10	0.96	0.97	0.97	0.98	0.99	0.99	0.99	1.00
8	0.87	.87	.86	.86	.85	.84	.83	.82	11	1.00	1.01	1.01	1.01	1.01	1.01	1.01	1.01
9	0.82	.80	.79	.78	.77	.75	.74	.72	12	1.01	1.01	1.01	1.01	1.01	1.00	1.00	1.00
10	—0.70	.68	.66	.64	.61	.58	.55	.52	13	+0.99	0.09	0.98	0.97	0.97	0.96	0.95	0.94
11	—0.48	.43	.38	.32	.25	.16	.05	9.89	18	—0.46	.41	.35	.29	.22	.13	0.03	9.89
13	+0.43	.49	.55	.60	.64	.69	.73	.76	20	+0.23	.29	.34	.39	.43	.47	0.51	0.54
14	+0.80	.83	.87	.90	.93	.96	.99	1.02	21	0.57	.59	.62	.64	.66	.68	.70	.72
18	—0.78	.80	.82	.84	.85	.86	.87	.88	22	0.73	.75	.76	.78	.79	.81	.82	.83
19	—0.89	.89	.90	.90	.90	.90	.90	.90	23	+0.84	.85	.86	.87	.88	.88	.89	.90
20	0.90	.89	.89	.89	.88	.88	.87	.86	24	0.91	.91	.92	.93	.93	.94	.94	.94
21	0.86	.85	.84	.83	.82	.81	.79	.78	25	0.95	.95	.96	.96	.96	.96	.96	.97
22	0.77	.75	.74	.72	.70	.69	.67	.64	26	0.97	.97	.97	.97	.97	.97	.97	.96
23	0.62	.60	.57	.54	.51	.48	.44	.40	27	0.96	.96	.96	.95	.95	.94	.94	.98
26	+0.30	.36	.41	.45	.49	.53	.56	.60	June 3	+0.05	.18	.28	.35	.42	.47	.52	.56
Mar. 8	—0.83	.82	.81	.80	.79	.78	.77	.76	4	0.60	.63	.66	.69	.72	.74	.76	.78
9	0.74	.73	.71	.70	.68	.66	.64	.60	5	0.80	.82	.83	.85	.86	.87	.88	.89
10	—0.59	.56	.53	.49	.45	.41	.36	.30	6	0.90	.91	.92	.93	.94	.94	.95	.95
12	+0.91	.06	.17	.26	.34	.40	.46	.51	7	0.96	.96	.97	.97	.97	.97	.98	.98
13	+0.55	.59	.63	.67	.70	.73	.76	.78	8	+0.98	.98	.98	.98	.97	.97	.97	.97
14	0.81	0.83	0.85	0.87	0.89	0.91	0.93	0.95	9	0.96	.96	.95	.95	.94	.94	.93	.92
15	+0.97	0.99	1.00	1.02	1.03	1.05	1.06	1.08	10	0.91	.91	.90	.88	.87	.86	.85	.83
19	—0.78	0.80	0.82	0.84	0.84	0.85	0.85	0.86	11	0.82	.80	.78	.76	.73	.71	.68	.64
20	—0.86	.86	.85	.85	.84	.84	.83	.82	17	0.18	.26	.32	.38	.43	.47	.51	.54
21	—0.82	.81	.80	.78	.77	.76	.74	.73	18	+0.57	.60	.63	.65	.67	.69	.71	.73
22	0.71	.69	.67	.65	.63	.61	.58	.55	19	0.74	.76	.77	.79	.80	.81	.82	.83
23	—0.53	.49	.46	.42	.38	.33	.27	.21	20	0.84	.85	.86	.87	.87	.88	.89	.89
25	+0.84	.98	.08	.16	.23	.29	.35	.39	21	0.90	.90	.91	.91	.92	.92	.93	.93
26	0.44	.48	.51	.54	.57	.60	.63	.65	22	0.93	.93	.92	.94	.94	.94	.94	.94
27	+0.67	.69	.71	.73	.75	.77	.79	.80	23	+0.94	.94	.94	.93	.93	.93	.93	.92
28	+0.82	.83	.85	.86	.87	.88	.90	.91	24	—0.92	.91	.91	.90	.90	.89	.88	.88
Apr. 6	—0.67	.65	.63	.61	.58	.55	.52	.48	25	0.87	.86	.85	.83	.83	.82	.81	.79
7	—0.44	.40	.35	.29	.22	.13	.03	.88	July 3	0.82	.84	.85	.87	.88	.89	.90	.91
9	+0.31	.37	.43	.48	.52	.56	.60	.63	4	0.92	.92	.93	.93	.93	.94	.95	.95

TABLE XI.

LOG. N FOR DISTANCES FROM THE SUN.

1855.	0 <sup>b</sup> .	3 <sup>b</sup> .	6 <sup>b</sup> .	9 <sup>b</sup> .	12 <sup>b</sup> .	15 <sup>b</sup> .	18 <sup>b</sup> .	21 <sup>b</sup> .	1855.	0 <sup>b</sup> .	3 <sup>b</sup> .	6 <sup>b</sup> .	9 <sup>b</sup> .	12 <sup>b</sup> .	15 <sup>b</sup> .	18 <sup>b</sup> .	21 <sup>b</sup> .
July 5	+0.95	.95	.96	.96	.96	.95	.95	.95	Oct. 3	-0.40	.44	.48	.52	.55	.58	.61	.63
6	0.95	.95	.94	.94	.94	.93	.93	.92	4	0.66	.68	.70	.72	.74	.76	.77	.79
7	0.91	.91	.90	.89	.88	.87	.86	.85	5	0.80	.82	.83	.84	.86	.87	.88	.89
8	0.84	.83	.81	.80	.78	.76	.75	.73	6	0.90	.91	.92	0.93	0.94	0.95	0.96	0.96
9	0.70	.68	.66	.63	.60	0.57	0.53	0.49	7	-0.97	.98	.98	0.99	1.00	1.00	1.01	1.02
10	+0.44	.39	.38	.25	.16	0.04	9.86	9.54	13	+0.84	.83	.82	0.81	0.80	0.78	0.77	0.76
16	0.47	.52	.56	.60	.63	0.66	0.68	0.70	14	0.74	.72	.71	.69	.67	.64	.62	.59
17	0.72	.74	.76	.77	.79	.80	.81	.82	15	+0.56	.53	.50	.46	.42	.37	.31	.25
18	0.83	.84	.85	.86	.86	.87	.88	.88	17	-9.98	.10	.20	.28	.34	.40	.45	.50
19	0.89	.89	.90	.90	.90	.91	.91	.91	18	0.54	.58	.61	.64	.67	.70	.73	.75
20	+0.91	.91	.91	.91	.91	.91	.91	.91	19	-0.77	.79	.81	.83	.85	.86	.88	.89
21	0.91	.91	.90	.90	.90	.89	.89	.88	20	0.91	.92	.93	.94	.96	.97	.98	.99
22	0.88	.87	.86	.85	.85	.84	.83	.82	30	9.99	.10	.18	.25	.31	.36	.41	.45
23	0.80	.79	.78	.76	.75	.73	.71	.69	31	0.49	.52	.55	.58	.61	.63	.65	.68
24	0.67	.64	.62	.59	.55	.52	.47	.42	Nov. 1	0.70	.72	.73	.75	.77	.78	.79	.81
Aug. 1	+0.91	.92	.92	.93	.93	.93	.94	.94	2	-0.82	.83	.84	.85	.86	.87	.88	.89
2	0.94	.94	.94	.94	.93	.93	.93	.92	3	0.90	.91	.91	.92	.93	.93	.94	.94
3	0.92	.91	.91	.90	.90	.89	.88	.87	4	0.95	.95	.96	.96	.96	.97	.97	.97
4	0.86	.85	.86	.83	.82	.80	.79	.77	5	0.98	.98	.98	.98	.98	.98	.98	.98
5	0.76	.74	.72	.70	.68	.66	.63	.61	6	-0.98	.98	.98	.98	.98	.97	.97	.97
6	+0.58	.55	.51	.47	.43	.38	.33	.27	12	+0.53	.48	.43	.37	.31	.23	.14	.02
8	-9.93	.07	.17	.26	.33	.39	.45	.50	14	-0.28	.35	.41	.46	.50	.54	.58	.61
9	-0.54	.58	.62	.66	.69	.72	.75	.78	15	0.64	.67	.76	.72	.74	.77	.79	.80
15	+0.79	.81	.82	.83	.84	.85	.86	.86	16	0.82	.84	.85	.87	.88	.89	.90	.91
16	0.87	.87	.88	.88	.88	.89	.89	.89	17	0.92	.93	.94	.95	.96	0.96	0.97	0.97
17	+0.89	.89	.89	.89	.89	.88	.88	.88	18	-0.98	0.98	0.99	0.99	0.99	1.00	1.00	1.00
18	0.88	.87	.87	.86	.86	.85	.84	.84	19	1.00	1.00	1.00	1.00	1.00	1.00	0.99	0.99
19	0.83	.82	.81	.80	.79	.78	.76	.75	27	0.06	0.16	0.24	0.31	0.36	0.41	.45	.49
20	0.73	.72	.70	.68	.66	.64	.61	.58	28	0.53	.56	.59	.62	.64	.66	.68	.70
21	+0.55	.52	.48	.44	.40	.34	.28	.20	29	0.72	.73	.75	.76	.78	.79	.80	.82
23	-0.17	.27	.35	.42	.48	.53	.58	.63	30	-0.83	.84	.85	.86	.86	.87	.88	.89
30	+0.92	.92	.92	.91	.91	.91	.90	.90	Dec. 1	0.89	.90	.91	.91	.92	.92	.93	.93
31	0.89	.88	.87	.87	.86	.85	.84	.82	2	0.93	.94	.94	.94	.94	.95	.95	.95
Sept. 1	0.81	.70	.78	.77	.75	.74	.72	.70	3	0.95	.95	.95	.95	.95	.95	.94	.94
2	0.67	.65	.63	.60	.57	.54	.51	.47	4	0.94	.94	.93	.93	.93	.92	.92	.91
3	+0.43	.38	.33	.27	.20	.12	.02	.88	5	-0.90	.90	.89	.88	.87	.86	.85	.84
5	-0.24	.31	.36	.41	.46	.50	.53	.57	6	0.82	.81	.79	.77	.75	.73	.70	.67
6	0.60	.63	.65	.68	.70	.73	.75	.77	12	0.38	.44	.49	.54	.58	.62	.65	.68
7	-0.79	.81	.83	.84	.86	.88	.89	.71	13	0.71	.73	.75	.77	.79	.81	.83	.84
13	+0.83	.84	.85	.85	.86	.86	.86	.87	14	0.86	.87	.88	.89	.90	.91	.92	.93
14	+0.87	.87	.87	.86	.86	.86	.86	.85	15	-0.93	.94	.95	.95	.96	.96	.96	.97
15	0.85	.84	.83	.83	.82	.81	.80	.79	16	0.97	.97	.97	.97	.97	.97	.97	.97
16	0.78	.77	.75	.74	.72	.71	.69	.67	17	0.97	.96	.96	.96	.95	.95	.94	.94
17	0.65	.63	.60	.58	.55	0.52	0.48	0.44	18	0.93	.92	.91	.90	.89	.88	.87	.86
18	+0.40	.35	.29	.22	.14	0.03	9.89	9.67	27	0.72	.73	.75	.76	.78	.79	.80	.81
20	-0.39	.44	.49	.54	.58	0.62	0.65	0.69	28	-0.83	.84	.84	.85	.86	.87	.88	.88
21	-0.71	.74	.77	.79	.82	.84	.86	.88	29	0.89	.89	.90	.90	.91	.91	.91	.92
29	+0.77	.75	.74	.72	.70	.68	.65	.63	30	0.92	.92	.92	.92	.93	.93	.93	.93
30	+0.60	0.57	0.54	0.50	.46	.42	.37	.32	31	-0.93	.92	.92	.92	.92	.92	.91	.91
Oct. 2	-9.58	9.81	9.97	0.08	.16	.24	.30	.35									

# TABLE XII.

For finding the value of *N* for correcting lunar distances for the compression of the earth.

TABLE XII. A. giving 1st Part of *N*.

App. Dist.	D's Declination.										
	0	3	6	9	12	15	18	21	24	27	30
20	0	3	6	10	13	16	19	22	25	28	31
22	0	3	6	9	12	14	17	20	23	25	28
24	0	3	5	8	11	13	16	18	21	23	25
26	0	2	5	7	10	12	14	17	19	21	23
28	0	2	4	7	9	11	13	15	17	19	21
30	0	2	4	6	8	10	12	14	16	18	20
32	0	2	4	6	8	9	11	13	15	16	18
34	0	2	4	5	7	9	10	12	14	15	17
36	0	2	3	5	7	8	10	11	13	14	16
38	0	2	3	5	6	8	9	10	12	13	14
40	0	1	3	4	6	7	8	10	11	12	13
42	0	1	3	4	5	7	8	9	10	11	13
44	0	1	2	4	5	6	7	8	10	11	12
46	0	1	2	3	5	6	7	8	9	10	11
48	0	1	2	3	4	5	6	7	8	9	10
50	0	1	2	3	4	5	6	7	8	9	10
52	0	1	2	3	4	5	6	7	8	9	10
54	0	1	2	3	4	5	6	7	7	8	9
56	0	1	2	3	4	5	5	6	7	7	8
58	0	1	1	2	3	4	4	5	6	6	7
60	0	1	1	2	3	3	4	5	5	6	7
62	0	1	1	2	3	3	4	4	5	5	6
64	0	1	1	2	2	3	3	4	4	5	6
66	0	1	1	2	2	3	3	4	4	5	5
68	0	0	1	1	2	2	3	3	4	4	5
70	0	0	1	1	2	2	3	3	3	4	4
72	0	0	1	1	2	2	2	3	3	3	4
74	0	0	1	1	1	2	2	2	3	3	3
76	0	0	1	1	1	1	2	2	2	3	3
78	0	0	0	1	1	1	1	2	2	2	2
80	0	0	0	1	1	1	1	1	2	2	2
82	0	0	0	0	1	1	1	1	1	1	2
84	0	0	0	0	0	1	1	1	1	1	1
86	0	0	0	0	0	0	0	1	1	1	1
88	0	0	0	0	0	0	0	0	0	0	0
90	0	0	0	0	0	0	0	0	0	0	0
92	0	0	0	0	0	0	0	0	0	0	0
94	0	0	0	0	0	0	0	1	1	1	1
96	0	0	0	0	0	1	1	1	1	1	1
98	0	0	0	0	1	1	1	1	1	1	2
100	0	0	0	1	1	1	1	1	2	2	2
102	0	0	0	1	1	1	1	1	2	2	2
104	0	0	1	1	1	1	2	2	2	3	3
106	0	0	1	1	1	2	2	2	3	3	3
108	0	0	1	1	2	2	2	3	3	3	4
110	0	1	1	2	2	3	3	3	4	4	4
112	0	1	1	2	2	3	3	4	4	5	5
114	0	1	1	2	2	3	3	4	4	5	5
116	0	1	1	2	2	3	3	4	4	5	6
118	0	1	1	2	3	3	4	4	5	5	6
120	0	1	1	2	3	3	4	5	5	6	7
122	0	1	1	2	3	4	4	5	6	6	7
124	0	1	2	2	3	4	5	5	6	7	8
126	0	1	2	3	3	4	5	6	7	7	8
128	0	1	2	3	4	5	5	6	7	8	9
130	0	1	2	3	4	5	6	7	8	9	10

TABLE XII. B. giving 2d Part of *N*.

App. Dist.	*s Declination.										
	0	3	6	9	12	15	18	21	24	27	30
20	0	3	7	10	14	17	20	24	27	30	33
22	0	3	6	9	13	16	19	22	25	27	30
24	0	3	6	9	12	14	17	20	23	25	28
26	0	3	5	8	11	13	16	18	21	23	26
28	0	3	5	8	10	12	15	17	20	22	24
30	0	2	5	7	9	12	14	16	18	21	23
32	0	2	4	7	9	11	13	15	17	19	21
34	0	2	4	6	8	11	13	15	16	18	20
36	0	2	4	6	8	10	12	14	16	17	19
38	0	2	4	6	8	10	11	13	15	17	18
40	0	2	4	6	7	9	11	13	14	16	18
42	0	2	4	5	7	9	10	12	14	15	17
44	0	2	3	5	7	8	10	12	13	15	16
46	0	2	3	5	6	8	10	11	13	14	16
48	0	2	3	5	6	8	9	11	12	14	15
50	0	2	3	5	6	8	9	11	12	13	15
52	0	2	3	4	6	7	9	10	12	13	14
54	0	1	3	4	6	7	9	10	11	13	14
56	0	1	3	4	6	7	8	10	11	12	14
58	0	1	3	4	6	7	8	10	11	12	13
60	0	1	3	4	5	7	8	9	11	12	13
62	0	1	3	4	5	7	8	9	10	12	13
64	0	1	3	4	5	7	8	9	10	11	13
66	0	1	3	4	5	6	8	9	10	11	12
68	0	1	3	4	5	6	8	9	10	11	12
70	0	1	3	4	5	6	7	9	10	11	12
72	0	1	2	4	5	6	7	9	10	11	12
74	0	1	2	4	5	6	7	8	10	11	12
76	0	1	2	4	5	6	7	8	9	11	12
78	0	1	2	4	5	6	7	8	9	11	12
80	0	1	2	4	5	6	7	8	9	10	11
82	0	1	2	4	5	6	7	8	9	10	11
84	0	1	2	4	5	6	7	8	9	10	11
86	0	1	2	4	5	6	7	8	9	10	11
88	0	1	2	4	5	6	7	8	9	10	11
90	0	1	2	4	5	6	7	8	9	10	11
92	0	1	2	4	5	6	7	8	9	10	11
94	0	1	2	4	5	6	7	8	9	10	11
96	0	1	2	4	5	6	7	8	9	10	11
98	0	1	2	4	5	6	7	8	9	10	11
100	0	1	2	4	5	6	7	8	9	10	11
102	0	1	2	4	5	6	7	8	9	11	12
104	0	1	2	4	5	6	7	8	9	11	12
106	0	1	2	4	5	6	7	8	10	11	12
108	0	1	2	4	5	6	7	9	10	11	12
110	0	1	3	4	5	6	7	9	10	11	12
112	0	1	3	4	5	6	8	9	10	11	12
114	0	1	3	4	5	6	8	9	10	11	12
116	0	1	3	4	5	7	8	9	10	11	13
118	0	1	3	4	5	7	8	9	10	12	13
120	0	1	3	4	5	7	8	9	11	12	13
122	0	1	3	4	6	7	8	10	11	12	13
124	0	1	3	4	6	7	8	10	11	12	14
126	0	1	3	4	6	7	9	10	11	13	14
128	0	2	3	4	6	7	9	10	12	13	14
130	0	2	3	5	6	8	9	11	12	13	15

The signs in the 0° column apply to all the numbers in the same line, and are to be used when the declination is North. When the declination is South, change the sign + to - and - to +.





IMPROVED METHOD

OF FINDING THE

ERROR AND RATE OF A CHRONOMETER

BY EQUAL ALTITUDES.

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FROM THE APPENDIX TO THE AMERICAN EPHEMERIS AND NAUTICAL  
ALMANAC FOR 1856.





# M E T H O D

OF FINDING THE

## ERROR AND RATE OF A CHRONOMETER BY EQUAL ALTITUDES.

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To regulate a chronometer to Greenwich time, we must determine its error and rate at a place whose longitude is well known. The most accurate method of doing this is by observing the transit of the sun or a star over the meridian. For the navigator, the most simple and accurate substitute for the meridian observation is that of equal altitudes of the same object on each side of the meridian. In the case of a star, the mean of the two chronometer times corresponding to the equal altitudes is the chronometer time of transit; but in the case of the sun, the mean of these times differs somewhat from the time of transit, since, in consequence of the change of the sun's declination between the observations, the equal altitudes do not occur at equal intervals before and after the transit.

The small correction necessary, when the sun is observed, to reduce the mean of the times to the time of transit, is called the *Equation of Equal Altitudes*.

The method of computing this equation given below is based upon that first given by GAUSS (*Monatliche Correspondenz*, Vol. XXIII.). We do not, however, follow him in using the double daily change of declination, or difference between the sun's declination on the noon preceding and the noon following that of the observation; but prefer to use the hourly difference, because this may be obtained directly from the American Ephemeris, and is at the same time even more accurate. We also extend our table so as to meet the case where one altitude is taken in the afternoon and the corresponding equal altitude on the following morning; in which case, the equation is computed for apparent midnight.\*

\* It should be observed, as a caution to navigators, that the rule for computing the equation for midnight is sometimes inaccurately, or incompletely, stated in works on navigation or astronomy. The rule in Lieut. RAPER'S *Practice of Navigation* is wholly erroneous. GALBRAITH'S rule (*Mathematical and Astronomical Tables*) is incomplete, in not noticing the case where the elapsed time is less than 12<sup>h</sup>. His rule for computing the equation for noon is similarly defective, in not noticing the case where the elapsed time is greater than 12<sup>h</sup>. In Professor INMAN'S rule there is a slight inaccuracy introduced, by taking the equation of time for mean, instead of apparent noon or midnight; and in all the books,

# EQUAL ALTITUDES.

## I. EQUAL ALTITUDES OF THE SUN, MORNING AND EVENING.

### THE OBSERVATION.

On shore, at a place whose longitude is *accurately* known, and whose latitude is *approximately* known, observe with an artificial horizon the same altitude both in the morning and in the afternoon, as near the prime vertical as convenient after the altitude is more than  $10^\circ$ , noting the times by the chronometer. In low latitudes, however, the method of equal altitudes will often give very accurate results, even when the observations are quite near to the meridian. In general, a sufficiently accurate result may be obtained if the observations are taken when the sun's change of altitude is not less than  $10''$  in  $0^{\circ}.5$ , or when the change in the double altitude taken with the artificial horizon is not less than  $20''$  in  $0^{\circ}.5$ .

It is most convenient, as well as conducive to accuracy, to take the observation in the following manner. In the morning, bring the lower limb of the sun, reflected from the sextant-mirrors, and the upper limb of that reflected from the mercury, into approximate contact; move the 0 of the vernier forward (say from  $10'$  to  $20'$ ), and set it on a division of the limb; the images will be *overlapped* and will be *separating*; wait for the instant of contact; note it by chronometer, and immediately set the vernier on the next division of the limb, that is,  $10'$  in advance; note the instant of contact again, and proceed in the same manner for as many observations as are thought necessary. If the sun rises too rapidly, let the intervals on the limb be  $20'$ . Find (roughly) the time when the sun will be at the same altitude in the afternoon, and just before that time set the vernier on the last altitude noted in the morning (of course using the same sextant); the images of the sun will be *separated*, but will be *approaching*; wait for the instant of contact; note it by chronometer; set the vernier *back* to the next division of the limb ( $10'$  or  $20'$ , as the case may be); note the contact again, and so proceed till all the A. M. altitudes have been again noted as P. M. altitudes.

### THE COMPUTATION.

Take the mean of the A. M. times and call it the *A. M. Chronometer Time*. The mean of the P. M. times call the *P. M. Chronometer Time*. If, instead of noting the times by the chronometer, a watch is used (compared with the chronometer both before and after each observation), it will generally be found necessary to make an allowance for its gain or loss on the chronometer, so as to obtain the exact difference between the watch and chronometer at the instant of observation. This difference being applied to the mean of the watch times, we have the mean chronometer time the same as would have been found by employing the chronometer directly.

the methods given of taking out the sun's change of declination, whether for  $48^h$  or for  $24^h$ , are not as accurate as they should be.

A perfectly accurate rule, with a special table, for the midnight correction, is given in SCHUMACHER'S *Hülfsstafeln* (Ed. by WARNSTORFF). It requires, however, one logarithm more than our method in the text, and is otherwise not so simple.

## EQUAL ALTITUDES.

The half sum of the A. M. and P. M. Chronometer Times is the *Middle Chronometer Time*, their difference is the *Elapsed Time*; observing that when the A. M. time is before 12<sup>h</sup> by chronometer, while the P. M. time is after 12<sup>h</sup>, the latter must be supposed to be increased by 12<sup>h</sup> in finding this half sum and difference.

Take from the Nautical Almanac the sun's declination, the hourly difference of declination, and the equation of time, reducing each to the instant of local apparent noon by applying the changes for the longitude.

Mark *north* latitude and *north* declination +

“ *south* latitude and *south* declination —

“ hourly diff. of decl. when *towards north* +

“ hourly diff. of decl. when *towards south* —.

Enter Table I. with the elapsed time, and take out log. A and log. B, prefixing to each its proper sign given in the table at the head of the page.

To log. A add the log. of the hourly diff., Table II., and the log. tangent of the latitude (Bowditch, Table XXVII.). Prefix to each log. the sign of the quantity it represents and to their sum the sign which results from the algebraic combination of the three signs.\* This sum is the log. (Table II.) of the number of seconds of time in the *first part* of equation of equal altitudes, to be marked + or — like its log.

To log. B. add the log. of the hourly diff. and the log. tangent of the declination, marking the signs as before. The sum is the log. of the *second part* of the equation of equal altitudes, to be marked + or — like its log.

Apply the two parts of the equation, according to their signs, to the *Middle Chronometer Time*; the result is the *Chronometer Time of Apparent Noon*.

To this apply the equation of time (adding, when the equation of time is additive to mean time, otherwise subtracting); the result is the *Chronometer Time of Mean Noon*, which, if the chronometer is regulated to local time, will be 12<sup>h</sup> 0<sup>m</sup> 0<sup>s</sup> when the chronometer is right; more than 12<sup>h</sup> when fast, less than 12<sup>h</sup> when slow.

If the chronometer is regulated to Greenwich time, apply the longitude (in time) to the chronometer time of mean noon (subtracting in west, adding in east); the result will be more or less than 12<sup>h</sup>, according as the chronometer is fast or slow.

Repeat this process on a subsequent day. The difference between the chronometer errors on the two days, divided by the number of days in the interval, is the *daily rate* of the chronometer, *gaining* or *losing* according as the chronometer goes too fast or too slow.

### EXAMPLE 1.

May 3d, 1856. At the United States Naval Academy, Lat. 38° 59' N., Long. 5<sup>h</sup> 5<sup>m</sup>. 55<sup>s</sup>.1 W., suppose the following observations of equal altitudes to be taken with an artificial horizon. Required the error of the chronometer on Greenwich time at noon of that day?

\* The algebraic rule being, that, when there is an *odd* number of factors with the sign minus, the result must have the sign minus, otherwise the sign plus. In the present application of this rule, when there is either *one* or *three* of the logs. marked —, their sum must be marked —; otherwise +.



# EQUAL ALTITUDES.

## A. M. Comparisons.

	h.	m.	s.
Chronom.	12	52	0.0
Watch	7	45	8.0
Diff.	5	6	52.0

Chronom.	1	20	0.0
Watch	8	13	9.5
Diff.	5	6	50.5

## Watch A. M.

	h.	m.	s.
	8	2	9.
	8	2	35.5
	8	3	0.5

Mean 8 2 35.0

Comparison 5 6 51.1

A. M. Chro. Time 1 9 26.1

P. M. Chro. Time 8 58 11.7

2)10 7 37.8

Middle Chro. T. 5 3 48.9

Equat. of Eq. Alts. —8.8

Chro. T. App. N. 5 3 40.1

Equat. of Time +3 19.4

Chro. T. Mean N. 5 6 59.5

Longitude 5 5 55.1 W.

Chro. fast 1 4.4

A. M., watch gains  $1^h.5$  in  $28^m$   
Interval to obs.  $17^m.5$   
 $28^m : 17^m.5 = 1^h.5 : 0^h.9$

P. M., watch gains  $2^h.2$  in  $34^m$   
Interval to obs.  $21^m$   
 $34^m : 21^m = 2^h.2 : 1^h.4$

## 2 ☉ Art. Hor.

	o	i
	65	50
	66	0
	66	10

(Eq. T.) —3 18.11 m. s. s. 0.258

1.32 5.1

Eq. T. —3 19.43 1.32

(D.) +15 48 50.5 (H.D.) +43.82 Decrease in  $24.0 = 0.66$   
3 42.8 —0.14 Decrease in  $5.1 = 0.14$

D. +15 52 33.3 H. D. +43.68

5.1

222.8

log. A. Tab. I. —9.4846 log. B. Tab. I. +9.2011  
H. D. +43<sup>h</sup>.68 log. Tab. II. +1.6403 . . . . . +1.6403  
Lat. +38° 59' log. tan. +9.9081 D. +15° 53' log. tan. +9.4542  
1st Pt. Eq. —10<sup>h</sup>.79 log. —1.0330 2d Pt. Eq. +1<sup>h</sup>.98 log. +0.2956

## P. M. Comparisons.

	h.	m.	s.
Chronom.	8	37	0.0
Watch	3	30	31.3
Diff.	5	6	28.7

Chronom.	9	11	0.0
Watch	4	4	33.5
Diff.	5	6	26.5

## Watch P. M.

	h.	m.	s.
	8	37	0.0
	3	52	10.7
	3	51	44.0
	3	51	18.5

Mean 3 51 44.4

Comparison 5 6 27.3

P. M. Chro. Time 8 58 11.7

A. M. Chro. Time 1 9 26.1

Elapsed Time 7 48 45.6

By similar observations on May 15th, suppose the chronometer is found to be fast  $12^h.5$ ; we have

	m.	s.
May 3d, fast	1	4.4
May 15th, fast		12.5
Loses in 12 days		51.9
Daily rate		4.33 losing.

## II. EQUAL ALTITUDES OF THE SUN, EVENING AND MORNING.

### THE OBSERVATION.

Take a set of altitudes, in the manner already explained, in the afternoon of one day, and the same altitudes in reverse order on the morning of the next, noting the times by the chronometer, or by a watch compared with it.

### THE COMPUTATION.

The half sum of the P. M. and A. M. Chronometer Times is the *Middle Chronometer Time*; their difference is the *Elapsed Time*; observing that when the P. M. time is before  $12^h$  by chronometer, while the A. M. time is after  $12^h$ , the latter must be supposed to be increased by  $12^h$  in finding this half sum and this difference.

Take from the Nautical Almanac the sun's declination, the hourly difference of declination, and the equation of time, reducing them each to the instant of local *apparent midnight*.

## EQUAL ALTITUDES.

Mark the sign of each quantity as before, and compute the two parts of the equation of equal altitudes precisely as in the preceding case, observing to mark the signs of log. A and log. B as given in the table for midnight.

Apply the two parts of the equation to the middle chronometer time, according to their signs; the result is the *Chronometer Time of Apparent Midnight*.

To this apply the equation of time (adding, when the equation of time is additive to mean time, otherwise subtracting); the result is the *Chronometer Time of Mean Midnight*, which, if the chronometer is regulated to local time, will be  $12^{\text{h}} 0^{\text{m}} 0^{\text{s}}$  when the chronometer is right; more than  $12^{\text{h}}$  when fast; less than  $12^{\text{h}}$  when slow.

If the chronometer is regulated to Greenwich time, apply the longitude, in time, to the chronometer time of mean midnight (subtracting in west, adding in east); the result will be more or less than  $12^{\text{h}}$  (or  $24^{\text{h}}$ ) according as the chronometer is fast or slow.

A repetition of this process at a subsequent day will give another error, whence the rate will be found as before. Or the rate may be found by comparing the results of an A. M. — P. M., and a P. M. — A. M. observation, remembering that the interval elapsed between two such observations is equal to the difference between the two dates *plus* or *minus* half a day.

### EXAMPLE 2.

May 3d, 1856, Lat.  $43^{\circ} 21' \text{ S.}$ , Long.  $9^{\text{h}} 50^{\text{m}} 8^{\text{s}} \text{ E.}$ , suppose the altitude of the sun to be observed in the afternoon and the same altitude again on the morning of the 4th, as below. Required the error of the chronometer on Greenwich time at midnight of the 3d?

Chronom., P. M.  
 $6^{\text{h}} 54^{\text{m}} 10^{\text{s}}.3$

$2 \odot$  Art. Hor.  
 $38^{\circ} 0'$

Chronom., A. M.  
 $9^{\text{h}} 9^{\text{m}} 17^{\text{s}}.5$

The A. M. time must be called  $21^{\text{h}} 9^{\text{m}} 17^{\text{s}}.5$ . The Greenwich time of midnight, for which the declination, &c. must be found, is May  $3^{\text{d}} 2^{\text{h}} 9^{\text{m}} 52^{\text{s}}$  ( $= 3^{\text{d}} 2^{\text{h}}.16$ ).

	h. m. s.
P. M. Chro. T.	6 54 10.3
A. M. Chro. T.	21 9 17.5
	2)28 3 27.8

Middle Chro. T.	14 1 43.9
Eq. of Eq. Alts.	—38.4
Chro. T. App. Midn.	14 1 5.5
Eq. of Time	+3 18.7
Chro. T. M'n Midn.	14 4 24.2
Longitude	9 50 8.0 E.
	23 54 32.2
	24 0 0.0

Chronom. *slow* 5 27.8

	m. s.	s.
(Eq. T.)	—3 18.11	0.258
	0.56	2.15
Eq. T.	—3 18.67	0.56

(D.)	$\overset{\circ}{+}15 \overset{'}{48} \overset{''}{50}.5$	(H. D.)	$\overset{''}{+}43.82$	Decrease in	h. 24.0	$= \overset{''}{0}.66$
	1 34.1		—0.06	Decrease in	2.16	$= 0.06$
D.	+15 50 24.6	H. D.	+43.76			
			2.15			
			94.1			

	log. A. Tab. I. +9.6958		log. B. Tab. I. —9.1586
H. D. +43' 76"	log. Tab. II. +1.6411		+1.6411
Lat. —43° 21'	log. tan. —9.9750	D. +15° 50'	log. tan. +9.4527
1st Pt. Eq. —20'.51	log. —1.3119	2d Pt. Eq. —17'.88	log. —0.2524

By an A. M. — P. M. observation on May 20th, suppose this chronometer is found to be slow  $8^{\text{m}} 14^{\text{s}}.6$ ; we have

	d. h.	m. s.
May 3 12	slow	5 27.8
May 20 0	slow	8 14.6
Losses in 16 <sup>d</sup> .5		2 46.8
Daily rate		10.11 losing.

# EQUAL ALTITUDES.

## III. EQUAL ALTITUDES OF A FIXED STAR.

### THE OBSERVATION.

In selecting stars for this observation, it is to be observed that the nearer the zenith the star passes, the less may the elapsed time be; and when the star passes exactly through the zenith, the two altitudes may be taken within a few minutes of each other. But with the ordinary sextants, altitudes near  $90^\circ$  cannot be taken with the artificial horizon, as the double altitude is then nearly  $180^\circ$ . The prismatic sextants, or still better, the prismatic circles of Pistor and Martin, are adapted for measuring angles of all magnitudes up to  $180^\circ$ , and are therefore especially suitable for this observation.

Set the sextant and wait for the coincidences of the two images of the star, as in the case of the sun's limb, noting the times by chronometer or watch.

### THE COMPUTATION.

Take the mean of the times before the meridian passage as the *A. M. Chronometer Time*, and the mean of those after the meridian passage as the *P. M. Chronometer Time*.

The mean of the A. M. and P. M. Chronometer Times is the *Chronometer Time of Star's Transit*. This time, if the chronometer is right, will agree with the true mean time of star's transit, which is to be computed as follows.

To the right ascension of the star apply the longitude of the place of observation (adding in west, subtracting in east); the result is the *Greenwich Sidereal Time of Star's Transit*, from which subtract the sidereal time at the *preceding* mean noon Greenwich (Nautical Almanac, page II. of the month); the remainder is the *Sidereal Interval* since mean noon. From Table IV. with the argument *Sidereal Interval*, take out the correction, which subtract from the sidereal interval; the remainder is the Greenwich Mean Time of the Star's Transit. The chronometer time will be more or less than this according as the chronometer is fast or slow.

If the chronometer is regulated to local time, apply the longitude to the Greenwich mean time of star's transit (subtracting in west, adding in east); the result is the *Local Mean Time of Star's Transit*, and the chronometer is fast or slow according as it shows more or less than this time.

### EXAMPLE 3.

July 15th, 1856, at the Cape of Good Hope, Lat.  $33^\circ 56' S.$ , Long.  $1^h 13^m 56^s E.$ , observed equal altitudes of *Antares* as follows: —



# EQUAL ALTITUDES.

Chronom. A. M.			2 Alt. Antares.		Chronom. P. M.				
	h.	m.	s.	°	'	h.	m.	s.	
	5	32	10.5	125	30	9	34	20.3	
	5	32	35.0		40	9	33	56.0	
	5	32	59.3		50	9	33	32.0	
A. M. Chro. T.	5	32	34.9			P. M. Chro. T.	9	33	56.1
P. M. Chro. T.	9	33	56.1						
	2)15	6	31.0				h.	m.	s.
Chro. T. * Transit	7	33	15.5	Antares R. A.			16	20	37.58
Gr. T. * Transit	7	31	22.1	Longitude			1	13	56.00 E.
Chro. fast	1	53.4		Gr. Sid. T.			15	6	41.58
				July 15, Gr. Sid. T. Mean Noon			7	34	5.25
				Sid. Interval			7	32	36.33
				Corréction, Table IV.			—1	14.15	
				Gr. M. T. * Transit			7	31	22.18

## IV. TO CORRECT FOR SMALL INEQUALITIES IN THE ALTITUDES.

Although the sextant readings are the same at the A. M. and P. M. observations, it may happen that neither the true nor even the apparent altitudes are the same. 1st. Supposing the sextant to remain unchanged, the atmospheric refraction may be different at the two observations in consequence of changes in the density and temperature of the air as shown by the barometer and thermometer. In this case, the apparent altitudes are equal, but the true altitudes are not so. 2d. The sextant may be affected by changes of temperature, particularly in day observations in the sun, so as to make the sextant readings the same for apparent altitudes slightly different. I do not think these changes in the sextant are to be eliminated by determining the index error at each observation, as has been supposed by some, since it is quite possible that the expansion and contraction of the various parts might leave the index correction unchanged while it affected the readings of the altitudes, or the reverse. The only course appears to be to guard the instrument as much as possible from changes of temperature, exposing it to the sun's rays only during the few minutes required for each observation.

But the correction for changes of refraction may be satisfactorily made as follows. Note the barometer and thermometer both A. M. and P. M.; take out the corresponding refractions for each observation from Tables III., III. A., and III. B., and find the difference of these refractions. Also take the difference between any two sextant readings and the difference between the two corresponding chronometer times. Then the correction of either noon or midnight will be found by the following proportion. The difference of the sextant readings is to the difference of the refractions as the difference of the chronometer times is to the required correction.

Apply this correction to the Chronometer Time of Noon or Midnight (obtained by the preceding rules) as follows: *add* it when the A. M. refraction is the greater; *subtract* it when the P. M. refraction is the greater. The result is the true Chronometer Time of Noon or Midnight.

EXAMPLE. — Suppose, in Example 1, we have in the morning, Barometer 30 inches, Thermometer 55°; in the afternoon, Barometer 29.5 inches, Thermometer 85°. The apparent altitude of sun's lower limb 33° 0'; the apparent altitude of sun's centre 33° 16'. We have

# EQUAL ALTITUDES.

A. M.		P. M.	
Mean refraction	1 29	Mean refraction	1 29
Barom. 30 in.	0	Barom. 29.5 in.	—1
Therm. 55°	—1	Therm. 85°	—6
True refraction	1 28	True refraction	1 22

Then the difference of the sextant readings is 10' (=600'') and the corresponding diff. of chronometer times is about 26'' ; whence

$$600 : 6'' = 26 : 0.26$$

The (approximate) Chronometer Time of Mean Noon was found to be	h. m. s.
Correction for change of refraction	+0.3
True Chronometer Time of Mean Noon	5 6 59.8

NOTE. — This correction may be found by the following rule, which we should have to resort to when but one altitude was taken at each observation. Add together the log. of the diff. of refractions (Tab. II.), log. cosine of the altitude, log. secant of the latitude, log. secant of the declination, log. cosecant of half elapsed time (or if the elapsed time is greater than 12<sup>h</sup>, half its supplement to 24<sup>h</sup>), and the constant log. 8.523; the sum is the log. (Table II.) of the required correction. Thus in the preceding example we have

Diff. refr.	6''	log.	0.778
Alt. ☉	33° 16'	log. cos.	9.922
Lat.	38° 59'	log. sec.	0.109
Dec.	15° 53'	log. sec.	0.017
El. T.	7 <sup>h</sup> 49 <sup>m</sup> .	log. cosec.*	0.069
		const. log.	8.523
Correction	0 <sup>s</sup> .26	log.	9.418

## DEGREE OF DEPENDENCE.

An error of 5' in the latitude would not affect the corresponding part of the equation of equal altitudes by more than one hundredth of its amount in the most unfavorable case, and in general would have no sensible effect. It is one of the advantages of the equal altitude method, therefore, that it does not require an accurate knowledge of the latitude. It is also plain that errors in the longitude affecting the declination and its hourly difference produce but small proportionate effects upon the computed equation. The absolute error of the chronometer on Greenwich will be affected by the whole error in the longitude, but the *rate* will still be correct. Hence we conclude that by this method the chronometer may be accurately *rated* at a place whose latitude and longitude are both imperfectly known.

The chief source of error is in the observation itself. The most practised observers with the sextant cannot depend on the noted time of a *single* contact within 0<sup>s</sup>.5, and hence the intervals between the successive chronometer times (which, if observations could be perfectly taken would be sensibly equal) may differ 2<sup>s</sup>. But the greatest probable error of the chronometer time of sun's or star's transit, from the mean of six such observations on each side of the meridian, is found to be not more than 0<sup>s</sup>.2, provided the rate of the chronometer between the observations is uniform.

Errors resulting from changes in the refraction may be almost wholly removed by computation as above.

\* Enter BOWDITCH'S Table XXVII., column P. M., with the *whole* elapsed time and take out the corresponding cosecant.

## EXPLANATION OF THE TABLES.

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TABLE I. — *Logarithms of A and B, for computing the Equation of Equal Altitudes*, are calculated by the formulas

$$A = \frac{E}{1800 \sin \frac{1}{2} E}, \quad B = \frac{E}{1800 \tan \frac{1}{2} E},$$

where E = elapsed time in minutes, and E in the denominator is the elapsed time expressed in arc.

If we put

$\phi$  = latitude of the place of observation, + north, — south,

$\delta$  = declination of the sun, + north, — south,

$\Delta$  = hourly change of declination, + north, — south,

$\chi$  = correction to reduce the middle chronometer time to chronometer time of apparent noon, algebraically additive,

$\chi'$  = the same for midnight,

we have

$$\begin{aligned} \chi &= - A \Delta \tan \phi + B \Delta \tan \delta \\ \chi' &= A \Delta \tan \phi + B \Delta \tan \delta. \end{aligned}$$

TABLE II. — *Logarithms of Numbers* to four decimal places. The first two figures of the number are found in the left-hand column, the third at the top, and the corresponding logarithm opposite and under these respectively. The proportional part for the fourth figure is found on the side in the same line with the logarithm taken out. The proper characteristic of the logarithm is to be supplied by the usual rule.

TABLE III. — *Mean Refraction*, reduced from BESSEL's Tables, to barometer 30 inches, and thermometer 50°.

TABLES III. A. and III. B. — *Corrections of the Mean Refraction for the Height of the Barometer and Thermometer*, also deduced from BESSEL's Tables. These are the same as Tables IV. A. and IV. B., given in the Appendix to the Nautical Almanac for 1855, where they are used for finding the corrections of the *Mean Reduced Refraction for Lunars*. It is for the purpose of having the same table for correcting both these mean refraction tables, that the argument in Tables III. A. and III. B. is the mean refraction instead of the apparent altitude.

TABLE IV. — *For converting Sidereal into Mean Solar Time*. This table gives the correction required to reduce a sidereal interval to its equivalent solar interval.



# TABLE I. LOG. A. AND LOG. B.

For Computing the Equation of Equal Altitudes.

For Noon, A — For Midnight, A + }		ARGUMENT = ELAPSED TIME.										{ For Noon or Midnight, B + }	
Elapsed Time.	0 <sup>h</sup> .		1 <sup>h</sup> .		2 <sup>h</sup> .		3 <sup>h</sup> .		4 <sup>h</sup> .		5 <sup>h</sup> .		
	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	
m													
0	9.4059	9.4059	9.4072	9.4034	9.4109	9.3959	9.4172	9.3828	9.4260	9.3635	9.4374	9.3369	
1	.4059	.4059	.4072	.4034	.4110	.3957	.4173	.3825	.4261	.3631	.4376	.3364	
2	.4059	.4059	.4073	.4033	.4111	.3955	.4174	.3822	.4263	.3627	.4378	.3358	
3	.4059	.4059	.4073	.4032	.4112	.3953	.4175	.3820	.4265	.3624	.4380	.3353	
4	.4059	.4059	.4074	.4031	.4113	.3952	.4177	.3817	.4266	.3620	.4383	.3348	
5	9.4059	9.4059	9.4074	9.4030	9.4113	9.3950	9.4178	9.3814	9.4268	9.3616	9.4385	9.3343	
6	.4060	.4059	.4074	.4029	.4114	.3948	.4179	.3811	.4270	.3612	.4387	.3337	
7	.4060	.4059	.4075	.4028	.4115	.3946	.4181	.3809	.4272	.3608	.4389	.3332	
8	.4060	.4059	.4075	.4027	.4116	.3944	.4182	.3806	.4273	.3604	.4391	.3327	
9	.4060	.4059	.4076	.4026	.4117	.3943	.4183	.3803	.4275	.3600	.4393	.3321	
10	9.4060	9.4059	9.4076	9.4025	9.4118	9.3941	9.4184	9.3800	9.4277	9.3596	9.4396	9.3316	
11	.4060	.4059	.4077	.4024	.4119	.3939	.4186	.3797	.4279	.3592	.4398	.3311	
12	.4060	.4058	.4077	.4023	.4120	.3937	.4187	.3794	.4280	.3588	.4400	.3305	
13	.4060	.4058	.4078	.4022	.4121	.3935	.4188	.3792	.4282	.3584	.4402	.3300	
14	.4060	.4058	.4078	.4021	.4121	.3933	.4190	.3789	.4284	.3580	.4405	.3294	
15	9.4060	9.4058	9.4079	9.4020	9.4122	9.3931	9.4191	9.3786	9.4286	9.3576	9.4407	9.3289	
16	.4060	.4058	.4079	.4019	.4123	.3929	.4193	.3783	.4288	.3572	.4409	.3283	
17	.4060	.4057	.4080	.4018	.4124	.3927	.4194	.3780	.4289	.3568	.4411	.3278	
18	.4061	.4057	.4080	.4017	.4125	.3925	.4195	.3777	.4291	.3564	.4414	.3272	
19	.4061	.4057	.4081	.4016	.4126	.3923	.4197	.3774	.4293	.3559	.4416	.3266	
20	9.4061	9.4057	9.4081	9.4015	9.4127	9.3921	9.4198	9.3771	9.4295	9.3555	9.4418	9.3261	
21	.4061	.4056	.4082	.4014	.4128	.3919	.4199	.3768	.4297	.3551	.4420	.3255	
22	.4061	.4056	.4083	.4013	.4129	.3917	.4201	.3765	.4299	.3547	.4423	.3249	
23	.4061	.4056	.4083	.4012	.4130	.3915	.4202	.3762	.4300	.3542	.4425	.3244	
24	.4061	.4055	.4084	.4010	.4131	.3913	.4204	.3759	.4302	.3538	.4427	.3238	
25	9.4062	9.4055	9.4084	9.4009	9.4132	9.3911	9.4205	9.3756	9.4304	9.3534	9.4430	9.3232	
26	.4062	.4055	.4085	.4008	.4133	.3909	.4207	.3752	.4306	.3530	.4432	.3226	
27	.4062	.4054	.4086	.4007	.4134	.3907	.4208	.3749	.4308	.3525	.4434	.3220	
28	.4062	.4054	.4086	.4006	.4135	.3905	.4209	.3746	.4310	.3521	.4437	.3214	
29	.4062	.4054	.4087	.4004	.4136	.3903	.4211	.3743	.4312	.3516	.4439	.3208	
30	9.4062	9.4053	9.4087	9.4003	9.4137	9.3900	9.4212	9.3740	9.4314	9.3512	9.4441	9.3203	
31	.4063	.4053	.4088	.4002	.4138	.3898	.4214	.3737	.4315	.3508	.4444	.3197	
32	.4063	.4052	.4089	.4001	.4139	.3896	.4215	.3733	.4317	.3503	.4446	.3191	
33	.4063	.4052	.4089	.3999	.4140	.3894	.4217	.3730	.4319	.3499	.4448	.3185	
34	.4063	.4051	.4090	.3998	.4141	.3892	.4218	.3727	.4321	.3494	.4451	.3178	
35	9.4064	9.4051	9.4091	9.3997	9.4142	9.3889	9.4220	9.3723	9.4323	9.3490	9.4453	9.3172	
36	.4064	.4050	.4091	.3995	.4144	.3887	.4221	.3720	.4325	.3485	.4456	.3166	
37	.4064	.4050	.4092	.3994	.4145	.3885	.4223	.3717	.4327	.3480	.4458	.3160	
38	.4064	.4049	.4093	.3993	.4146	.3882	.4224	.3713	.4329	.3476	.4460	.3154	
39	.4065	.4049	.4093	.3991	.4147	.3880	.4226	.3710	.4331	.3471	.4463	.3148	
40	9.4065	9.4048	9.4094	9.3990	9.4148	9.3878	9.4227	9.3707	9.4333	9.3467	9.4465	9.3142	
41	.4065	.4048	.4095	.3988	.4149	.3875	.4229	.3703	.4335	.3462	.4468	.3135	
42	.4065	.4047	.4095	.3987	.4150	.3873	.4231	.3700	.4337	.3457	.4470	.3129	
43	.4066	.4047	.4096	.3985	.4151	.3871	.4232	.3696	.4339	.3453	.4473	.3123	
44	.4066	.4046	.4097	.3984	.4152	.3868	.4234	.3693	.4341	.3448	.4475	.3116	
45	9.4066	9.4045	9.4097	9.3982	9.4154	9.3866	9.4235	9.3690	9.4343	9.3443	9.4477	9.3110	
46	.4067	.4045	.4098	.3981	.4155	.3863	.4237	.3686	.4345	.3438	.4480	.3103	
47	.4067	.4044	.4099	.3979	.4156	.3861	.4238	.3683	.4347	.3433	.4482	.3097	
48	.4067	.4043	.4100	.3978	.4157	.3859	.4240	.3679	.4349	.3429	.4485	.3091	
49	.4068	.4043	.4100	.3976	.4158	.3856	.4242	.3675	.4351	.3424	.4487	.3084	
50	9.4068	9.4042	9.4101	9.3975	9.4159	9.3854	9.4243	9.3672	9.4353	9.3419	9.4490	9.3078	
51	.4068	.4041	.4102	.3973	.4161	.3851	.4245	.3668	.4355	.3414	.4492	.3071	
52	.4069	.4041	.4103	.3972	.4162	.3849	.4246	.3665	.4357	.3409	.4494	.3064	
53	.4069	.4040	.4103	.3970	.4163	.3846	.4248	.3661	.4359	.3404	.4497	.3058	
54	.4069	.4039	.4104	.3969	.4164	.3843	.4250	.3657	.4361	.3399	.4500	.3051	
55	9.4070	9.4038	9.4105	9.3967	9.4165	9.3841	9.4251	9.3654	9.4363	9.3394	9.4503	9.3044	
56	.4070	.4038	.4106	.3965	.4167	.3838	.4253	.3650	.4366	.3389	.4505	.3038	
57	.4071	.4037	.4107	.3964	.4168	.3836	.4255	.3646	.4368	.3384	.4508	.3031	
58	.4071	.4036	.4107	.3962	.4169	.3833	.4256	.3643	.4370	.3379	.4510	.3024	
59	.4071	.4035	.4108	.3960	.4170	.3830	.4258	.3639	.4372	.3374	.4513	.3017	
60	9.4072	9.4034	9.4109	9.3959	9.4172	9.3828	9.4260	9.3635	9.4374	9.3369	9.4515	9.3010	

# TABLE I. LOG. A. AND LOG. B.

For Computing the Equation of Equal Altitudes.

For Noon, A — For Midnight, A + }		ARGUMENT = ELAPSED TIME.										{ For Noon or Midnight, B + }	
Elapsed Time.	6 <sup>h</sup> .		7 <sup>h</sup> .		8 <sup>h</sup> .		9 <sup>h</sup> .		10 <sup>h</sup> .		11 <sup>h</sup> .		
	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	
0	9.4515	9.3010	9.4685	9.2530	9.4884	9.1874	9.5115	9.0943	9.5379	8.9509	9.5680	8.6837	
1	.4518	.3003	.4688	.2520	.4888	.1861	.5119	.0925	.5384	.9478	.5685	.6770	
2	.4521	.2996	.4691	.2511	.4892	.1848	.5123	.0906	.5389	.9447	.5691	.6701	
3	.4523	.2989	.4694	.2502	.4895	.1835	.5127	.0887	.5393	.9416	.5696	.6631	
4	.4526	.2982	.4697	.2492	.4899	.1822	.5132	.0867	.5398	.9384	.5701	.6560	
5	9.4528	9.2975	9.4701	9.2483	9.4902	9.1809	9.5136	9.0848	9.5403	8.9352	9.5707	8.6488	
6	.4531	.2968	.4704	.2473	.4906	.1796	.5140	.0828	.5408	.9320	.5712	.6414	
7	.4534	.2961	.4707	.2463	.4910	.1782	.5144	.0809	.5412	.9287	.5718	.6359	
8	.4536	.2954	.4710	.2454	.4913	.1769	.5148	.0789	.5417	.9254	.5723	.6262	
9	.4539	.2947	.4713	.2444	.4917	.1756	.5153	.0769	.5422	.9221	.5728	.6183	
10	9.4542	9.2940	9.4716	9.2434	9.4921	9.1742	9.5157	9.0749	9.5427	8.9187	9.5734	8.6103	
11	.4544	.2932	.4719	.2425	.4924	.1728	.5161	.0729	.5432	.9153	.5739	.6021	
12	.4547	.2925	.4723	.2415	.4928	.1715	.5165	.0708	.5436	.9118	.5745	.5937	
13	.4550	.2918	.4726	.2405	.4932	.1701	.5169	.0688	.5441	.9083	.5750	.5852	
14	.4552	.2911	.4729	.2395	.4935	.1687	.5174	.0667	.5446	.9048	.5756	.5764	
15	9.4555	9.2903	9.4732	9.2385	9.4939	9.1673	9.5178	9.0646	9.5451	8.9013	9.5761	8.5674	
16	.4558	.2896	.4735	.2375	.4943	.1659	.5182	.0625	.5456	.8977	.5767	.5583	
17	.4561	.2888	.4738	.2365	.4946	.1645	.5186	.0604	.5461	.8940	.5772	.5488	
18	.4563	.2881	.4742	.2355	.4950	.1630	.5191	.0583	.5466	.8903	.5778	.5392	
19	.4566	.2873	.4745	.2344	.4954	.1616	.5195	.0561	.5470	.8866	.5783	.5293	
20	9.4569	9.2866	9.4748	9.2334	9.4958	9.1602	9.5199	9.0540	9.5475	8.8829	9.5789	8.5192	
21	.4572	.2858	.4751	.2324	.4961	.1587	.5204	.0518	.5480	.8791	.5794	.5088	
22	.4574	.2850	.4755	.2313	.4965	.1573	.5208	.0496	.5485	.8752	.5800	.4981	
23	.4577	.2843	.4758	.2303	.4969	.1558	.5212	.0474	.5490	.8713	.5806	.4871	
24	.4580	.2835	.4761	.2292	.4973	.1543	.5217	.0452	.5495	.8674	.5811	.4758	
25	9.4583	9.2827	9.4764	9.2282	9.4977	9.1528	9.5221	9.0429	9.5500	8.8634	9.5817	8.4641	
26	.4585	.2819	.4768	.2271	.4980	.1513	.5225	.0406	.5505	.8594	.5822	.4521	
27	.4588	.2812	.4771	.2261	.4984	.1498	.5230	.0383	.5510	.8553	.5828	.4397	
28	.4591	.2804	.4774	.2250	.4988	.1483	.5234	.0360	.5515	.8512	.5834	.4270	
29	.4594	.2796	.4778	.2239	.4992	.1468	.5238	.0337	.5520	.8470	.5839	.4138	
30	9.4597	9.2788	9.4781	9.2228	9.4996	9.1453	9.5243	9.0314	9.5525	8.8427	9.5845	8.4001	
31	.4600	.2780	.4784	.2217	.5000	.1437	.5247	.0290	.5530	.8384	.5851	.3860	
32	.4602	.2772	.4788	.2206	.5003	.1422	.5252	.0266	.5535	.8341	.5856	.3713	
33	.4605	.2764	.4791	.2195	.5007	.1406	.5256	.0242	.5540	.8297	.5862	.3561	
34	.4608	.2756	.4794	.2184	.5011	.1390	.5261	.0218	.5545	.8253	.5868	.3403	
35	9.4611	9.2747	9.4798	9.2173	9.5015	9.1375	9.5265	9.0194	9.5550	8.8208	9.5874	8.3239	
36	.4614	.2739	.4801	.2162	.5019	.1359	.5269	.0169	.5555	.8162	.5879	.3067	
37	.4617	.2731	.4804	.2151	.5023	.1343	.5274	.0144	.5560	.8115	.5885	.2888	
38	.4620	.2723	.4808	.2140	.5027	.1327	.5278	.0119	.5565	.8068	.5891	.2701	
39	.4622	.2714	.4811	.2128	.5031	.1310	.5283	.0094	.5570	.8020	.5897	.2505	
40	9.4625	9.2706	9.4815	9.2117	9.5035	9.1294	9.5287	9.0069	9.5576	8.7972	9.5902	8.2299	
41	.4628	.2698	.4818	.2105	.5038	.1278	.5292	.0043	.5581	.7923	.5908	.2082	
42	.4631	.2689	.4821	.2094	.5042	.1261	.5296	.0017	.5586	.7873	.5914	.1853	
43	.4634	.2681	.4825	.2082	.5046	.1244	.5301	8.9991	.5591	.7823	.5920	.1611	
44	.4637	.2672	.4828	.2070	.5050	.1228	.5305	.9965	.5596	.7772	.5926	.1354	
45	9.4640	9.2664	9.4832	9.2059	9.5054	9.1211	9.5310	8.9938	9.5601	8.7720	9.5931	8.1080	
46	.4643	.2655	.4835	.2047	.5058	.1194	.5315	.9911	.5606	.7668	.5937	.0786	
47	.4646	.2646	.4839	.2035	.5062	.1177	.5319	.9884	.5612	.7614	.5943	.0470	
48	.4649	.2638	.4842	.2023	.5066	.1159	.5324	.9857	.5617	.7560	.5949	.0128	
49	.4652	.2629	.4846	.2011	.5070	.1142	.5328	.9830	.5622	.7505	.5955	.79756	
50	9.4655	9.2620	9.4849	9.1999	9.5074	9.1125	9.5333	8.9802	9.5627	8.7449	9.5961	7.9348	
51	.4658	.2611	.4853	.1987	.5078	.1107	.5337	.9774	.5632	.7392	.5967	.8897	
52	.4661	.2602	.4856	.1974	.5082	.1089	.5342	.9745	.5638	.7335	.5973	.8391	
53	.4664	.2593	.4860	.1962	.5086	.1072	.5347	.9717	.5643	.7276	.5979	.7817	
54	.4667	.2584	.4863	.1950	.5091	.1054	.5351	.9688	.5648	.7217	.5985	.7154	
55	9.4670	9.2575	9.4867	9.1937	9.5095	9.1036	9.5356	8.9659	9.5654	8.7156	9.5991	7.6368	
56	.4673	.2566	.4870	.1925	.5099	.1017	.5361	.9630	.5659	.7094	.5997	.5405	
57	.4676	.2557	.4874	.1912	.5103	.0999	.5365	.9600	.5664	.7032	.6003	.4162	
58	.4679	.2548	.4877	.1900	.5107	.0981	.5370	.9570	.5669	.6968	.6009	.2407	
59	.4682	.2539	.4881	.1887	.5111	.0962	.5375	.9540	.5675	.6903	.6015	6.9591	
60	9.4685	9.2530	9.4884	9.1874	9.5115	9.0943	9.5379	8.9509	9.5680	8.6837	9.6021	Inf.	



# TABLE I. LOG. A. AND LOG. B.

For Computing the Equation of Equal Altitudes.

For Noon, A —  
For Midnight, A + }

ARGUMENT = ELAPSED TIME.

{ For Noon or  
Midnight, B —

Elapsed Time.	1 <sup>h</sup> .		1 <sup>h</sup> .		1 <sup>h</sup> .		1 <sup>h</sup> .		1 <sup>h</sup> .		1 <sup>h</sup> .	
	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.
m												
0	9.6021	<i>Inf.</i>	9.6406	8.7563	9.6841	9.0971	9.7333	9.3162	9.7895	9.4884	9.8539	9.6383
1	.6027	6.9603	.6412	.7641	.6848	.1014	.7342	.3194	.7905	.4911	.8550	.6407
2	.6033	7.2431	.6419	.7718	.6856	.1057	.7351	.3225	.7915	.4937	.8562	.6431
3	.6039	.4198	.6426	.7794	.6864	.1099	.7360	.3256	.7925	.4963	.8573	.6455
4	.6045	.5453	.6433	.7868	.6872	.1141	.7369	.3287	.7935	.4990	.8585	.6478
5	9.6051	7.6428	9.6440	8.7942	9.6879	9.1183	9.7378	9.3319	9.7945	9.5016	9.8597	9.6502
6	.6057	.7226	.6447	.8015	.6887	.1224	.7386	.3350	.7955	.5042	.8608	.6526
7	.6063	.7902	.6454	.8087	.6895	.1265	.7395	.3380	.7965	.5068	.8620	.6550
8	.6069	.8488	.6461	.8158	.6903	.1306	.7404	.3411	.7975	.5094	.8632	.6573
9	.6075	.9005	.6467	.8227	.6911	.1347	.7413	.3442	.7986	.5120	.8644	.6597
10	9.6082	7.9469	9.6474	8.8296	9.6919	9.1387	9.7422	9.3472	9.7996	9.5146	9.8655	9.6621
11	.6088	.9889	.6481	.8364	.6926	.1428	.7431	.3503	.8006	.5171	.8667	.6644
12	.6094	8.0273	.6488	.8432	.6934	.1468	.7440	.3533	.8016	.5197	.8679	.6668
13	.6100	.0627	.6495	.8498	.6942	.1507	.7449	.3563	.8027	.5223	.8691	.6691
14	.6106	.0955	.6502	.8564	.6950	.1547	.7458	.3593	.8037	.5248	.8703	.6715
15	9.6112	8.1260	9.6509	8.8628	9.6958	9.1586	9.7467	9.3623	9.8047	9.5274	9.8715	9.6738
16	.6119	.1547	.6516	.8692	.6966	.1625	.7476	.3653	.8058	.5300	.8727	.6762
17	.6125	.1816	.6523	.8756	.6974	.1664	.7485	.3683	.8068	.5325	.8739	.6785
18	.6131	.2071	.6530	.8818	.6982	.1703	.7494	.3713	.8078	.5351	.8751	.6809
19	.6137	.2312	.6538	.8880	.6990	.1741	.7503	.3742	.8089	.5376	.8763	.6832
20	9.6144	8.2541	9.6545	8.8941	9.6998	9.1779	9.7512	9.3772	9.8099	9.5401	9.8775	9.6856
21	.6150	.2759	.6552	.9002	.7006	.1817	.7522	.3801	.8110	.5427	.8787	.6879
22	.6156	.2967	.6559	.9062	.7014	.1855	.7531	.3831	.8120	.5452	.8799	.6903
23	.6163	.3166	.6566	.9121	.7022	.1893	.7540	.3860	.8131	.5477	.8812	.6926
24	.6169	.3357	.6573	.9180	.7030	.1930	.7549	.3889	.8141	.5502	.8824	.6949
25	9.6175	8.3540	9.6580	8.9238	9.7038	9.1967	9.7558	9.3918	9.8152	9.5528	9.8836	9.6973
26	.6182	.3717	.6588	.9295	.7047	.2004	.7568	.3947	.8162	.5553	.8848	.6996
27	.6188	.3887	.6595	.9352	.7055	.2041	.7577	.3976	.8173	.5578	.8861	.7019
28	.6194	.4051	.6602	.9408	.7063	.2078	.7586	.4005	.8184	.5603	.8873	.7043
29	.6201	.4210	.6609	.9464	.7071	.2114	.7595	.4033	.8194	.5628	.8885	.7066
30	9.6207	8.4363	9.6616	8.9519	9.7079	9.2150	9.7605	9.4062	9.8205	9.5653	9.8898	9.7089
31	.6214	.4512	.6624	.9573	.7088	.2186	.7614	.4090	.8216	.5677	.8910	.7112
32	.6220	.4657	.6631	.9627	.7096	.2222	.7624	.4119	.8227	.5702	.8923	.7136
33	.6226	.4796	.6638	.9681	.7104	.2258	.7633	.4147	.8237	.5727	.8935	.7159
34	.6233	.4932	.6645	.9734	.7112	.2293	.7642	.4175	.8248	.5752	.8948	.7182
35	9.6239	8.5064	9.6653	8.9787	9.7121	9.2329	9.7652	9.4204	9.8259	9.5777	9.8961	9.7205
36	.6246	.5192	.6660	.9839	.7129	.2364	.7661	.4232	.8270	.5801	.8973	.7228
37	.9252	.5318	.6667	.9891	.7137	.2399	.7671	.4260	.8281	.5826	.8986	.7251
38	.6259	.5440	.6675	.9942	.7146	.2434	.7680	.4288	.8292	.5850	.8999	.7275
39	.6265	.5559	.6682	.9993	.7154	.2468	.7690	.4316	.8303	.5875	.9011	.7298
40	9.6272	8.5675	9.6690	9.0043	9.7162	9.2503	9.7699	9.4343	9.8314	9.5900	9.9024	9.7321
41	.6279	.5788	.6697	.0093	.7171	.2537	.7709	.4371	.8325	.5924	.9037	.7344
42	.6285	.5899	.6704	.0142	.7179	.2571	.7718	.4399	.8336	.5948	.9050	.7367
43	.6292	.6008	.6712	.0191	.7187	.2605	.7728	.4426	.8347	.5973	.9063	.7390
44	.6298	.6114	.6719	.0240	.7196	.2639	.7738	.4454	.8358	.5997	.9075	.7413
45	9.6305	8.6218	9.6727	9.0288	9.7204	9.2673	9.7747	9.4481	9.8369	9.6022	9.9088	9.7436
46	.6311	.6320	.6734	.0336	.7213	.2706	.7757	.4509	.8380	.6046	.9101	.7459
47	.6318	.6419	.6742	.0384	.7221	.2740	.7767	.4536	.8391	.6070	.9114	.7482
48	.6325	.6517	.6749	.0431	.7230	.2773	.7776	.4563	.8402	.6094	.9127	.7505
49	.6331	.6613	.6757	.0478	.7238	.2806	.7786	.4590	.8414	.6119	.9140	.7529
50	9.6338	8.6707	9.6764	9.0524	9.7247	9.2839	9.7796	9.4617	9.8425	9.6143	9.9154	9.7552
51	.6345	.6799	.6772	.0570	.7256	.2872	.7806	.4644	.8436	.6167	.9167	.7575
52	.6351	.6899	.6779	.0616	.7264	.2905	.7815	.4671	.8447	.6191	.9180	.7598
53	.6358	.6979	.6787	.0662	.7273	.2937	.7825	.4698	.8459	.6215	.9193	.7621
54	.6365	.7067	.6795	.0707	.7281	.2970	.7835	.4725	.8470	.6239	.9206	.7644
55	9.6372	8.7153	9.6802	9.0752	9.7290	9.3002	9.7845	9.4752	9.8481	9.6263	9.9202	9.7667
56	.6378	.7237	.6810	.0796	.7299	.3034	.7855	.4778	.8493	.6287	.9233	.7690
57	.6385	.7321	.6818	.0840	.7307	.3066	.7865	.4805	.8504	.6311	.9246	.7713
58	.6392	.7402	.6825	.0884	.7316	.3098	.7875	.4831	.8516	.6335	.9260	.7736
59	.6399	.7483	.6833	.0928	.7324	.3130	.7885	.4858	.8527	.6359	.9273	.7759
60	9.6406	8.7563	9.6841	9.0971	9.7333	9.3162	9.7895	9.4884	9.8539	9.6383	9.9287	9.7782



# TABLE I. LOG. A. AND LOG. B.

For Computing the Equation of Equal Altitudes.

For Noon, A — }  
For Midnight, A + }

ARGUMENT = ELAPSED TIME.

{ For Noon or  
{ Midnight, B —

Elapsed Time. m	18 <sup>h</sup> .		19 <sup>h</sup> .		20 <sup>h</sup> .		21 <sup>h</sup> .		22 <sup>h</sup> .		23 <sup>h</sup> .	
	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.	Log. A.	Log. B.
0	9.9287	9.7782	0.0172	9.9167	0.1249	0.0625	0.2623	0.2279	0.4523	0.4372	0.7689	0.7652
1	.9300	.7804	.0188	.9190	.1269	.0650	.2649	.2309	.4562	.4414	.7765	.7729
2	.9314	.7827	.0204	.9213	.1290	.0676	.2676	.2339	.4601	.4455	.7842	.7807
3	.9327	.7850	.0221	.9237	.1310	.0701	.2702	.2370	.4640	.4497	.7920	.7886
4	.9341	.7873	.0237	.9260	.1330	.0727	.2729	.2401	.4680	.4540	.8000	.7967
5	9.9355	9.7896	0.0253	9.9284	0.1351	0.0753	0.2756	0.2431	0.4720	0.4582	0.8081	0.8049
6	.9368	.7919	.0270	.9307	.1371	.0779	.2783	.2462	.4761	.4625	.8163	.8133
7	.9382	.7942	.0286	.9331	.1392	.0805	.2810	.2493	.4801	.4668	.8247	.8218
8	.9396	.7965	.0303	.9355	.1412	.0830	.2838	.2524	.4842	.4711	.8333	.8305
9	.9410	.7988	.0319	.9378	.1433	.0856	.2865	.2556	.4884	.4755	.8420	.8393
10	9.9424	9.8011	0.0336	9.9402	0.1454	0.0882	0.2893	0.2587	0.4926	0.4799	0.8508	0.8483
11	.9437	.8034	.0353	.9426	.1475	.0909	.2921	.2619	.4968	.4844	.8599	.8574
12	.9451	.8057	.0370	.9449	.1496	.0935	.2949	.2650	.5010	.4889	.8691	.8667
13	.9465	.8080	.0386	.9473	.1517	.0961	.2977	.2682	.5053	.4934	.8786	.8763
14	.9479	.8103	.0403	.9497	.1538	.0987	.3005	.2714	.5097	.4980	.8882	.8860
15	9.9493	9.8126	0.0420	9.9520	0.1559	0.1013	0.3034	0.2746	0.5140	0.5026	0.8980	0.8959
16	.9508	.8149	.0437	.9544	.1581	.1040	.3063	.2778	.5184	.5072	.9080	.9060
17	.9522	.8172	.0454	.9568	.1602	.1066	.3091	.2811	.5229	.5118	.9183	.9164
18	.9536	.8195	.0472	.9592	.1623	.1093	.3120	.2843	.5274	.5165	.9288	.9270
19	.9550	.8218	.0489	.9616	.1645	.1119	.3150	.2876	.5319	.5213	.9396	.9378
20	9.9564	9.8241	0.0506	9.9640	0.1667	0.1146	0.3179	0.2909	0.5365	0.5261	0.9506	0.9489
21	.9579	.8264	.0523	.9664	.1689	.1173	.3208	.2942	.5411	.5309	.9618	.9603
22	.9593	.8287	.0541	.9687	.1711	.1200	.3238	.2975	.5458	.5358	.9734	.9719
23	.9607	.8310	.0558	.9711	.1733	.1226	.3268	.3008	.5505	.5407	.9853	.9839
24	.9622	.8333	.0576	.9735	.1755	.1253	.3298	.3041	.5553	.5457	.9975	.9961
25	9.9636	9.8356	0.0593	9.9760	0.1777	0.1280	0.3328	0.3075	0.5601	0.5507	1.0100	1.0087
26	.9651	.8379	.0611	.9784	.1799	.1308	.3359	.3109	.5649	.5557	.0228	.0216
27	.9665	.8402	.0628	.9808	.1821	.1335	.3389	.3143	.5698	.5608	.0361	.0350
28	.9680	.8425	.0646	.9832	.1844	.1362	.3420	.3177	.5748	.5660	.0497	.0487
29	.9695	.8448	.0664	.9856	.1867	.1389	.3451	.3211	.5798	.5712	.0638	.0628
30	9.9709	9.8471	0.0682	9.9880	0.1889	0.1417	0.3482	0.3245	0.5848	0.5764	1.0783	1.0774
31	.9724	.8494	.0700	.9904	.1912	.1444	.3514	.3280	.5899	.5817	.0934	.0925
32	.9739	.8517	.0718	.9929	.1935	.1472	.3545	.3315	.5951	.5871	.1089	.1081
33	.9754	.8540	.0736	.9953	.1958	.1499	.3577	.3350	.6003	.5925	.1250	.1242
34	.9769	.8563	.0754	.9977	.1981	.1527	.3609	.3385	.6056	.5979	.1416	.1409
35	9.9784	9.8586	0.0772	0.0002	0.2004	0.1555	0.3641	0.3420	0.6110	0.6034	1.1590	1.1583
36	.9798	.8609	.0790	.0026	.2028	.1582	.3674	.3456	.6164	.6090	.1770	.1764
37	.9813	.8632	.0809	.0051	.2051	.1610	.3706	.3491	.6218	.6147	.1958	.1952
38	.9829	.8655	.0827	.0075	.2075	.1638	.3739	.3527	.6273	.6204	.2154	.2149
39	.9844	.8678	.0845	.0100	.2098	.1667	.3772	.3563	.6329	.6261	.2359	.2354
40	9.9859	9.8701	0.0864	0.0124	0.2122	0.1695	0.3805	0.3599	0.6386	0.6319	1.2573	1.2569
41	.9874	.8724	.0883	.0149	.2146	.1723	.3839	.3636	.6443	.6378	.2799	.2795
42	.9889	.8748	.0901	.0173	.2170	.1751	.3873	.3673	.6501	.6438	.3037	.3033
43	.9904	.8771	.0920	.0198	.2194	.1780	.3907	.3710	.6560	.6498	.3288	.3285
44	.9920	.8794	.0939	.0223	.2218	.1808	.3941	.3747	.6619	.6559	.3554	.3552
45	9.9935	9.8817	0.0958	0.0248	0.2243	0.1837	0.3975	0.3784	0.6679	0.6621	1.3837	1.3835
46	.9951	.8840	.0976	.0272	.2267	.1866	.4010	.3822	.6740	.6684	.4140	.4138
47	.9966	.8863	.0995	.0297	.2292	.1895	.4045	.3859	.6802	.6747	.4465	.4463
48	.9982	.8887	.1015	.0322	.2316	.1924	.4080	.3897	.6865	.6811	.4815	.4814
49	.9998	.8910	.1034	.0347	.2341	.1953	.4115	.3936	.6928	.6876	.5196	.5195
50	0.0013	9.8933	0.1053	0.0372	0.2366	0.1982	0.4151	0.3974	0.6993	0.6942	1.5613	1.5612
51	.0029	.8956	.1072	.0397	.2391	.2011	.4187	.4013	.7058	.7008	.6074	.6073
52	.0044	.8980	.1092	.0422	.2416	.2040	.4223	.4052	.7124	.7076	.6588	.6587
53	.0060	.9003	.1111	.0447	.2442	.2070	.4260	.4091	.7191	.7144	.7171	.7171
54	.0076	.9026	.1131	.0473	.2467	.2099	.4297	.4130	.7259	.7214	.7844	.7843
55	0.0092	9.9050	0.1150	0.0498	0.2493	0.2129	0.4334	0.4170	0.7328	0.7284	1.8638	1.8638
56	.0108	.9073	.1170	.0523	.2518	.2159	.4371	.4210	.7398	.7355	.9610	.9610
57	.0124	.9096	.1190	.0548	.2544	.2189	.4408	.4250	.7469	.7428	2.0863	2.0863
58	.0140	.9120	.1209	.0574	.2570	.2219	.4446	.4291	.7541	.7501	.2627	.2627
59	.0156	.9143	.1229	.0599	.2596	.2249	.4485	.4331	.7615	.7576	2.5640	2.5640
60	0.0172	9.9167	0.1249	0.0625	0.2623	0.2279	0.4523	0.4372	0.7689	0.7652	Inf.	Inf.

# TABLE II.

## LOGARITHMS OF NUMBERS.

Natural Numbers.	0	1	2	3	4	5	6	7	8	9	Proportional Parts.								
											1	2	3	4	5	6	7	8	9
10	0000	0043	0086	0128	0170	0212	0253	0294	0334	0374	4	8	12	17	21	25	29	33	37
11	0414	0453	0492	0531	0569	0607	0645	0682	0719	0755	4	8	11	15	19	23	26	30	34
12	0792	0828	0864	0899	0934	0969	1004	1038	1072	1106	3	7	10	14	17	21	24	28	31
13	1139	1173	1206	1239	1271	1303	1335	1367	1399	1430	3	6	10	13	16	19	23	26	29
14	1461	1492	1523	1553	1584	1614	1644	1673	1703	1732	3	6	9	12	15	18	21	24	27
15	1761	1790	1818	1847	1875	1903	1931	1959	1987	2014	3	6	8	11	14	17	20	22	25
16	2041	2068	2095	2122	2148	2175	2201	2227	2253	2279	3	5	8	11	13	16	18	21	24
17	2304	2330	2355	2380	2405	2430	2455	2480	2504	2529	2	5	7	10	12	15	17	20	22
18	2553	2577	2601	2625	2648	2672	2695	2718	2742	2765	2	5	7	9	12	14	16	19	21
19	2788	2810	2833	2856	2878	2900	2923	2945	2967	2989	2	4	7	9	11	13	16	18	20
20	3010	3032	3054	3075	3096	3118	3139	3160	3181	3201	2	4	6	8	11	13	15	17	19
21	3222	3243	3263	3284	3304	3324	3345	3365	3385	3404	2	4	6	8	10	12	14	16	18
22	3424	3444	3464	3483	3502	3522	3541	3560	3579	3598	2	4	6	8	10	12	14	15	17
23	3617	3636	3655	3674	3692	3711	3729	3747	3766	3784	2	4	6	7	9	11	13	15	17
24	3802	3820	3838	3856	3874	3892	3909	3927	3945	3962	2	4	5	7	9	11	12	14	16
25	3979	3997	4014	4031	4048	4065	4082	4099	4116	4133	2	3	5	7	9	10	12	14	15
26	4150	4166	4183	4200	4216	4232	4249	4265	4281	4298	2	3	5	7	8	10	11	13	15
27	4314	4330	4346	4362	4378	4393	4409	4425	4440	4456	2	3	5	6	8	9	11	13	14
28	4472	4487	4502	4518	4533	4548	4564	4579	4594	4609	2	3	5	6	8	9	11	12	14
29	4624	4639	4654	4669	4683	4698	4713	4728	4742	4757	1	3	4	6	7	9	10	12	13
30	4771	4786	4800	4814	4829	4843	4857	4871	4886	4900	1	3	4	6	7	9	10	11	13
31	4914	4928	4942	4955	4969	4983	4997	5011	5024	5038	1	3	4	6	7	8	10	11	12
32	5051	5065	5079	5092	5105	5119	5132	5145	5159	5172	1	3	4	5	7	8	9	11	12
33	5185	5198	5211	5224	5237	5250	5263	5276	5289	5302	1	3	4	5	6	8	9	10	12
34	5315	5328	5340	5353	5366	5378	5391	5403	5416	5428	1	3	4	5	6	8	9	10	11
35	5441	5453	5465	5478	5490	5502	5514	5527	5539	5551	1	2	4	5	6	7	9	10	11
36	5563	5575	5587	5599	5611	5623	5635	5647	5658	5670	1	2	4	5	6	7	8	10	11
37	5682	5694	5705	5717	5729	5740	5752	5763	5775	5786	1	2	3	5	6	7	8	9	10
38	5798	5809	5821	5832	5843	5855	5866	5877	5888	5899	1	2	3	5	6	7	8	9	10
39	5911	5922	5933	5944	5955	5966	5977	5988	5999	6010	1	2	3	4	5	7	8	9	10
40	6021	6031	6042	6053	6064	6075	6085	6096	6107	6117	1	2	3	4	5	6	8	9	10
41	6128	6138	6149	6160	6170	6180	6191	6201	6212	6222	1	2	3	4	5	6	7	8	9
42	6232	6243	6253	6263	6274	6284	6294	6304	6314	6325	1	2	3	4	5	6	7	8	9
43	6335	6345	6355	6365	6375	6385	6395	6405	6415	6425	1	2	3	4	5	6	7	8	9
44	6435	6444	6454	6464	6474	6484	6493	6503	6513	6522	1	2	3	4	5	6	7	8	9
45	6532	6542	6551	6561	6571	6580	6590	6599	6609	6618	1	2	3	4	5	6	7	8	9
46	6628	6637	6646	6656	6665	6675	6684	6693	6702	6712	1	2	3	4	5	6	7	7	8
47	6721	6730	6739	6749	6758	6767	6776	6785	6794	6803	1	2	3	4	5	5	6	7	8
48	6812	6821	6830	6839	6848	6857	6866	6875	6884	6893	1	2	3	4	4	5	6	7	8
49	6902	6911	6920	6928	6937	6946	6955	6964	6972	6981	1	2	3	4	4	5	6	7	8
50	6990	6998	7007	7016	7024	7033	7042	7050	7059	7067	1	2	3	3	4	5	6	7	8
51	7076	7084	7093	7101	7110	7118	7126	7135	7143	7152	1	2	3	3	4	5	6	7	8
52	7160	7168	7177	7185	7193	7202	7210	7218	7226	7235	1	2	2	3	4	5	6	7	7
53	7243	7251	7259	7267	7275	7284	7292	7300	7308	7316	1	2	2	3	4	5	6	6	7
54	7324	7332	7340	7348	7356	7364	7372	7380	7388	7396	1	2	2	3	4	5	6	6	7



# TABLE II.

## LOGARITHMS OF NUMBERS.

Natural Numbers.	0	1	2	3	4	5	6	7	8	9	Proportional Parts.								
											1	2	3	4	5	6	7	8	9
55	7404	7412	7419	7427	7435	7443	7451	7459	7466	7474	1	2	2	3	4	5	5	6	7
56	7482	7490	7497	7505	7513	7520	7528	7536	7543	7551	1	2	2	3	4	5	5	6	7
57	7559	7566	7574	7582	7589	7597	7604	7612	7619	7627	1	2	2	3	4	5	5	6	7
58	7634	7642	7649	7657	7664	7672	7679	7686	7694	7701	1	1	2	3	4	4	5	6	7
59	7709	7716	7723	7731	7738	7745	7752	7760	7767	7774	1	1	2	3	4	4	5	6	7
60	7782	7789	7796	7803	7810	7818	7825	7832	7839	7846	1	1	2	3	4	4	5	6	6
61	7853	7860	7868	7875	7882	7889	7896	7903	7910	7917	1	1	2	3	4	4	5	6	6
62	7924	7931	7938	7945	7952	7959	7966	7973	7980	7987	1	1	2	3	3	4	5	6	6
63	7993	8000	8007	8014	8021	8028	8035	8041	8048	8055	1	1	2	3	3	4	5	5	6
64	8062	8069	8075	8082	8089	8096	8102	8109	8116	8122	1	1	2	3	3	4	5	5	6
65	8129	8136	8142	8149	8156	8162	8169	8176	8182	8189	1	1	2	3	3	4	5	5	6
66	8195	8202	8209	8215	8222	8228	8235	8241	8248	8254	1	1	2	3	3	4	5	5	6
67	8261	8267	8274	8280	8287	8293	8299	8306	8312	8319	1	1	2	3	3	4	5	5	6
68	8325	8331	8338	8344	8351	8357	8363	8370	8376	8382	1	1	2	3	3	4	4	5	6
69	8388	8395	8401	8407	8414	8420	8426	8432	8439	8445	1	1	2	2	3	4	4	5	6
70	8451	8457	8463	8470	8476	8482	8488	8494	8500	8506	1	1	2	2	3	4	4	5	6
71	8513	8519	8525	8531	8537	8543	8549	8555	8561	8567	1	1	2	2	3	4	4	5	5
72	8573	8579	8585	8591	8597	8603	8609	8615	8621	8627	1	1	2	2	3	4	4	5	5
73	8633	8639	8645	8651	8657	8663	8669	8675	8681	8686	1	1	2	2	3	4	4	5	5
74	8692	8698	8704	8710	8716	8722	8727	8733	8739	8745	1	1	2	2	3	4	4	5	5
75	8751	8756	8762	8768	8774	8779	8785	8791	8797	8802	1	1	2	2	3	3	4	5	5
76	8808	8814	8820	8825	8831	8837	8842	8848	8854	8859	1	1	2	2	3	3	4	5	5
77	8865	8871	8876	8882	8887	8893	8899	8904	8910	8915	1	1	2	2	3	3	4	4	5
78	8921	8927	8932	8938	8943	8949	8954	8960	8965	8971	1	1	2	2	3	3	4	4	5
79	8976	8982	8987	8993	8998	9004	9009	9015	9020	9025	1	1	2	2	3	3	4	4	5
80	9031	9036	9042	9047	9053	9058	9063	9069	9074	9079	1	1	2	2	3	3	4	4	5
81	9085	9090	9096	9101	9106	9112	9117	9122	9128	9133	1	1	2	2	3	3	4	4	5
82	9138	9143	9149	9154	9159	9165	9170	9175	9180	9186	1	1	2	2	3	3	4	4	5
83	9191	9196	9201	9206	9212	9217	9222	9227	9232	9238	1	1	2	2	3	3	4	4	5
84	9243	9248	9253	9258	9263	9269	9274	9279	9284	9289	1	1	2	2	3	3	4	4	5
85	9294	9299	9304	9309	9315	9320	9325	9330	9335	9340	1	1	2	2	3	3	4	4	5
86	9345	9350	9355	9360	9365	9370	9375	9380	9385	9390	1	1	2	2	3	3	4	4	5
87	9395	9400	9405	9410	9415	9420	9425	9430	9435	9440	0	1	1	2	2	3	3	4	4
88	9445	9450	9455	9460	9465	9469	9474	9479	9484	9489	0	1	1	2	2	3	3	4	4
89	9494	9499	9504	9509	9513	9518	9523	9528	9533	9538	0	1	1	2	2	3	3	4	4
90	9542	9547	9552	9557	9562	9566	9571	9576	9581	9586	0	1	1	2	2	3	3	4	4
91	9590	9595	9600	9605	9609	9614	9619	9624	9628	9633	0	1	1	2	2	3	3	4	4
92	9638	9643	9647	9652	9657	9661	9666	9671	9675	9680	0	1	1	2	2	3	3	4	4
93	9685	9689	9694	9699	9703	9708	9713	9717	9722	9727	0	1	1	2	2	3	3	4	4
94	9731	9736	9741	9745	9750	9754	9759	9763	9768	9773	0	1	1	2	2	3	3	4	4
95	9777	9782	9786	9791	9795	9800	9805	9809	9814	9818	0	1	1	2	2	3	3	4	4
96	9823	9827	9832	9836	9841	9845	9850	9854	9859	9863	0	1	1	2	2	3	3	4	4
97	9868	9872	9877	9881	9886	9890	9894	9899	9903	9908	0	1	1	2	2	3	3	4	4
98	9912	9917	9921	9926	9930	9934	9939	9943	9948	9952	0	1	1	2	2	3	3	4	4
99	9956	9961	9965	9969	9974	9978	9983	9987	9991	9996	0	1	1	2	2	3	3	3	4



# TABLE III. MEAN REFRACTION.

Barometer 30 inches. Fahrenheit's Thermometer 50°.

Apparent Altitude.	Mean Refraction.	Apparent Altitude.	Mean Refraction.	Apparent Altitude.	Mean Refraction.	Apparent Altitude.	Mean Refraction.	Apparent Altitude.	Mean Refraction.
° ' "	' "	° ' "	' "	° ' "	' "	° ' "	' "	° ' "	' "
0 0	36 29.4	9 30	5 35.1	15 0	3 34.1	25 0	2 4.4	42 0	1 4.7
1 0	24 53.6	35	5 32.4	10	3 31.7	10	2 3.4	20	1 3.9
2 0	18 25.5	40	5 29.6	20	3 29.4	20	2 2.5	40	1 3.2
3 0	14 25.1	45	5 27.0	30	3 27.1	30	2 1.6	43 0	1 2.4
4 0	11 44.4	50	5 24.3	40	3 24.8	40	2 0.7	20	1 1.7
		55	5 21.7	50	3 22.6	50	1 59.8	40	1 1.0
5 0	9 52.0	10 0	5 19.2	16 0	3 20.5	26 0	1 58.9	44 0	1 0.3
5	9 44.0	5	5 16.7	10	3 18.4	10	1 58.1	20	0 59.6
10	9 36.2	10	5 14.2	20	3 16.3	20	1 57.2	40	0 58.9
15	9 28.6	15	5 11.7	30	3 14.2	30	1 56.4	45 0	0 58.2
20	9 21.2	20	5 9.3	40	3 12.2	40	1 55.5	20	0 57.6
25	9 14.0	25	5 6.9	50	3 10.3	50	1 54.7	40	0 56.9
5 30	9 7.0	10 30	5 4.6	17 0	3 8.3	27 0	1 53.9	46 0	0 56.2
35	9 0.1	35	5 2.3	10	3 6.4	10	1 53.1	20	0 55.6
40	8 53.4	40	5 0.0	20	3 4.6	20	1 52.3	40	0 55.0
45	8 46.8	45	4 57.8	30	3 2.8	30	1 51.5	47 0	0 54.3
50	8 40.4	50	4 55.6	40	3 1.0	40	1 50.7	20	0 53.7
55	8 34.2	55	4 53.4	50	2 59.2	50	1 50.0	40	0 53.1
6 0	8 28.0	11 0	4 51.2	18 0	2 57.5	28 0	1 49.2	48 0	0 52.5
5	8 22.1	5	4 49.1	10	2 55.8	20	1 47.7	49 0	0 50.6
10	8 16.2	10	4 47.0	20	2 54.1	40	1 46.2	50 0	0 48.9
15	8 10.5	15	4 44.9	30	2 52.4	29 0	1 44.8	51 0	0 47.2
20	8 4.8	20	4 42.9	40	2 50.8	20	1 43.4	52 0	0 45.5
25	7 59.3	25	4 40.9	50	2 49.2	40	1 42.0	53 0	0 43.9
6 30	7 53.9	11 30	4 38.9	19 0	2 47.7	30 0	1 40.6	54 0	0 42.3
35	7 48.7	35	4 36.9	10	2 46.1	20	1 39.3	55 0	0 40.8
40	7 43.5	40	4 35.0	20	2 44.6	40	1 38.0	56 0	0 39.3
45	7 38.4	45	4 33.1	30	2 43.1	31 0	1 36.7	57 0	0 37.8
50	7 33.5	50	4 31.2	40	2 41.6	20	1 35.5	58 0	0 36.4
55	7 28.6	55	4 29.4	50	2 40.2	40	1 34.2	59 0	0 35.0
7 0	7 23.8	12 0	4 27.5	20 0	2 38.8	32 0	1 33.0	60 0	0 33.6
5	7 19.2	5	4 25.7	10	2 37.4	20	1 31.8	61 0	0 32.3
10	7 14.6	10	4 23.9	20	2 36.0	40	1 30.7	62 0	0 31.0
15	7 10.1	15	4 22.2	30	2 34.6	33 0	1 29.5	63 0	0 29.7
20	7 5.7	20	4 20.4	40	2 33.3	20	1 28.4	64 0	0 28.4
25	7 1.4	25	4 18.7	50	2 32.0	40	1 27.3	65 0	0 27.2
7 30	6 57.1	12 30	4 17.0	21 0	2 30.7	34 0	1 26.2	66 0	0 25.9
35	6 53.0	35	4 15.3	10	2 29.4	20	1 25.1	67 0	0 24.7
40	6 48.9	40	4 13.6	20	2 28.1	40	1 24.1	68 0	0 23.6
45	6 44.9	45	4 12.0	30	2 26.9	35 0	1 23.1	69 0	0 22.4
50	6 41.0	50	4 10.4	40	2 25.7	20	1 22.0	70 0	0 21.2
55	6 37.1	55	4 8.8	50	2 24.5	40	1 21.0	71 0	0 20.1
8 0	6 33.3	13 0	4 7.2	22 0	2 23.3	36 0	1 20.1	72 0	0 18.9
5	6 29.6	5	4 5.6	10	2 22.1	20	1 19.1	73 0	0 17.8
10	6 25.9	10	4 4.1	20	2 20.9	40	1 18.2	74 0	0 16.7
15	6 22.3	15	4 2.6	30	2 19.8	37 0	1 17.2	75 0	0 15.6
20	6 18.8	20	4 1.0	40	2 18.7	20	1 16.3	76 0	0 14.5
25	6 15.3	25	3 59.6	50	2 17.5	40	1 15.4	77 0	0 13.5
8 30	6 11.9	13 30	3 58.1	23 0	2 16.4	38 0	1 14.5	78 0	0 12.4
35	6 8.5	35	3 56.6	10	2 15.4	20	1 13.6	79 0	0 11.3
40	6 5.2	40	3 55.2	20	2 14.3	40	1 12.7	80 0	0 10.3
45	6 2.0	45	3 53.7	30	2 13.3	39 0	1 11.9	81 0	0 9.2
50	5 58.8	50	3 52.3	40	2 12.2	20	1 11.0	82 0	0 8.2
55	5 55.7	55	3 50.9	50	2 11.2	40	1 10.2	83 0	0 7.2
9 0	5 52.6	14 0	3 49.5	24 0	2 10.2	40 0	1 9.4	84 0	0 6.1
5	5 49.6	10	3 46.8	10	2 9.2	20	1 8.6	85 0	0 5.1
10	5 46.6	20	3 44.2	20	2 8.2	40	1 7.8	86 0	0 4.1
15	5 43.6	30	3 41.6	30	2 7.2	41 0	1 7.0	87 0	0 3.1
20	5 40.7	40	3 39.0	40	2 6.2	20	1 6.2	88 0	0 2.0
25	5 37.9	50	3 36.5	50	2 5.3	40	1 5.4	89 0	0 1.0
9 30	5 35.1	15 0	3 34.1	25 0	2 4.4	42 0	1 4.7	90 0	0 0.0

# TABLE III. A.

Correction of the Mean Refraction for the Height of the Barometer.

Barometer.		MEAN REFRACTION.																				Barometer.
Subtract.	0'		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'	Add.
	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	
27.50	0	2	5	7	10	12	15	17	20	23	25	28	30	33	35	38	40	43	45	48	51	
27.55	0	2	5	7	10	12	15	17	20	22	25	27	30	32	35	37	40	42	45	47	50	
27.60	0	2	5	7	10	12	14	17	19	22	24	27	29	31	34	36	39	41	44	46	49	
27.65	0	2	5	7	9	12	14	16	19	21	24	26	28	31	33	36	38	40	43	45	48	
27.70	0	2	5	7	9	11	14	16	18	21	23	25	28	30	32	35	37	39	42	44	47	
27.75	0	2	4	7	9	11	13	16	18	20	23	25	27	29	32	34	36	39	41	43	46	
27.80	0	2	4	7	9	11	13	15	18	20	22	24	27	29	31	33	35	38	40	42	45	
27.85	0	2	4	6	9	11	13	15	17	19	22	24	26	28	30	32	35	37	39	41	44	
27.90	0	2	4	6	8	10	13	15	17	19	21	23	25	27	30	32	34	36	38	40	43	
27.95	0	2	4	6	8	10	12	14	16	18	21	23	25	27	29	31	33	35	37	39	42	
28.00	0	2	4	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	41	
28.05	0	2	4	6	8	10	12	14	16	18	20	22	24	25	27	29	31	33	35	37	39	
28.10	0	2	4	6	8	9	11	13	15	17	19	21	23	25	27	29	31	33	34	36	38	
28.15	0	2	4	6	7	9	11	13	15	17	19	20	22	24	26	28	30	32	34	36	37	
28.20	0	2	4	5	7	9	11	13	14	16	18	20	22	24	25	27	29	31	33	35	36	
28.25	0	2	3	5	7	9	10	12	14	16	18	19	21	23	25	26	28	30	32	34	35	
28.30	0	2	3	5	7	8	10	12	14	15	17	19	21	22	24	26	27	29	31	33	34	
28.35	0	2	3	5	7	8	10	12	13	15	17	18	20	22	23	25	27	28	30	32	33	
28.40	0	2	3	5	6	8	10	11	13	14	16	18	19	21	23	24	26	27	29	31	32	
28.45	0	2	3	5	6	8	9	11	12	14	16	17	19	20	22	23	25	27	28	30	31	
28.50	0	1	3	4	6	7	9	10	12	14	15	17	18	20	21	23	24	26	27	29	30	31.50
28.55	0	1	3	4	6	7	9	10	12	13	15	16	17	19	20	22	23	25	26	28	29	31.45
28.60	0	1	3	4	6	7	8	10	11	13	14	15	17	18	20	21	23	24	25	27	28	31.40
28.65	0	1	3	4	5	7	8	9	11	12	14	15	16	18	19	20	22	23	25	26	27	31.35
28.70	0	1	3	4	5	6	8	9	10	12	13	14	16	17	18	20	21	22	24	25	26	31.30
28.75	0	1	2	4	5	6	7	9	10	11	13	14	15	16	18	19	20	21	23	24	25	31.25
28.80	0	1	2	4	5	6	7	8	10	11	12	13	14	16	17	18	19	21	22	23	24	31.20
28.85	0	1	2	3	5	6	7	8	9	10	12	13	14	15	16	17	19	20	21	22	23	31.15
28.90	0	1	2	3	4	5	7	8	9	10	11	12	13	14	16	17	18	19	20	21	22	31.10
28.95	0	1	2	3	4	5	6	7	8	9	11	12	13	14	15	16	17	18	19	20	21	31.05
29.00	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	31.00
29.05	0	1	2	3	4	5	6	7	8	9	10	11	11	12	13	14	15	16	17	18	19	30.95
29.10	0	1	2	3	4	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	30.90
29.15	0	1	2	3	3	4	5	6	7	8	9	9	10	11	12	13	14	15	16	17	18	30.85
29.20	0	1	2	2	3	4	5	6	6	7	8	9	10	10	11	12	13	14	15	16	17	30.80
29.25	0	1	1	2	3	4	4	5	6	7	8	8	9	10	11	11	12	13	14	14	15	30.75
29.30	0	1	1	2	3	3	4	5	6	6	7	8	8	9	10	11	11	12	13	13	14	30.70
29.35	0	1	1	2	3	3	4	5	5	6	7	7	8	9	9	10	10	11	12	13	13	30.65
29.40	0	1	1	2	2	3	4	4	5	5	6	7	7	8	8	9	10	10	11	12	12	30.60
29.45	0	1	1	2	2	3	3	4	4	5	6	6	7	7	8	8	9	9	10	11	11	30.55
29.50	0	0	1	1	2	2	3	3	4	5	5	6	6	7	7	8	8	9	9	10	10	30.50
29.55	0	0	1	1	2	2	3	3	4	4	5	5	6	6	7	7	8	8	9	9	9	30.45
29.60	0	0	1	1	2	2	2	3	3	4	4	4	5	5	6	6	6	7	7	8	8	30.40
29.65	0	0	1	1	1	2	2	2	3	3	4	4	4	5	5	6	6	6	7	7	7	30.35
29.70	0	0	1	1	1	1	2	2	2	3	3	3	4	4	4	5	5	5	5	6	6	30.30
29.75	0	0	0	1	1	1	1	2	2	2	3	3	3	3	4	4	4	4	5	5	5	30.25
29.80	0	0	0	1	1	1	1	1	2	2	2	2	2	3	3	3	3	3	4	4	4	30.20
29.85	0	0	0	0	1	1	1	1	1	1	2	2	2	2	2	2	2	3	3	3	3	30.15
29.90	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2	30.10
29.95	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	30.05
30.00	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	30.00
Subtract.	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	Add.
	0'	1'	2'	3'	4'	5'	6'	7'	8'	9'	10'	Barometer.										
Barometer.		MEAN REFRACTION.																				Barometer.

# TABLE III. B.

Correction of the Mean Refraction for the Height of the Thermometer.

Thermom.		MEAN REFRACTION.																				Thermom.			
Add.		0'		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'		Add.	
		0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	
—10		0	4	8	12	16	20	24	28	33	37	41	46	50	55	60	65	70	75	80	85	90		—10	
— 8		0	4	8	12	15	19	23	27	31	36	40	44	48	53	58	62	67	72	77	82	87		— 8	
— 6		0	4	7	11	15	19	22	26	30	34	38	42	47	51	55	60	64	69	74	79	84		— 6	
— 4		0	4	7	11	14	18	22	25	29	33	37	41	45	49	53	57	62	66	71	76	80		— 4	
— 2		0	3	7	10	14	17	21	24	28	31	35	39	43	47	51	55	59	64	68	72	77		— 2	
0		0	3	7	10	13	16	20	23	27	30	34	37	41	45	49	53	57	61	65	69	74		0	
2		0	3	6	9	12	16	19	22	25	29	32	36	39	43	47	50	54	58	62	66	70		2	
4		0	3	6	9	12	15	18	21	24	28	31	34	37	41	44	48	52	55	59	63	67		4	
6		0	3	6	8	11	14	17	20	23	26	29	32	36	39	42	46	49	53	56	60	64		6	
8		0	3	5	8	11	14	16	19	22	25	28	31	34	37	40	43	47	50	54	57	61		8	
10		0	3	5	8	10	13	15	18	21	24	26	29	32	35	38	41	44	48	51	54	58		10	
11		0	2	5	7	10	13	15	18	20	23	26	28	31	34	37	40	43	46	49	53	56		11	
12		0	2	5	7	10	12	15	17	20	22	25	28	30	33	36	39	42	45	48	51	54		12	
13		0	2	5	7	9	12	14	17	19	22	24	27	30	32	35	38	41	44	47	50	53		13	
14		0	2	5	7	9	11	14	16	19	21	24	26	29	31	34	37	40	42	45	48	51		14	
15		0	2	4	7	9	11	13	16	18	20	23	25	28	30	33	36	38	41	44	47	50		15	
16		0	2	4	6	9	11	13	15	18	20	22	25	27	29	32	35	37	40	43	45	48		16	
17		0	2	4	6	8	10	13	15	17	19	21	24	26	29	31	33	36	39	41	44	47		17	
18		0	2	4	6	8	10	12	14	16	19	21	23	25	28	30	32	35	37	40	43	45		18	
19		0	2	4	6	8	10	12	14	16	18	20	22	24	27	29	31	34	36	39	41	44		19	
20		0	2	4	6	8	9	11	13	15	17	19	22	24	26	28	30	33	35	37	40	42		20	
21		0	2	4	5	7	9	11	13	15	17	19	21	23	25	27	29	31	34	36	38	41		21	
22		0	2	3	5	7	9	11	12	14	16	18	20	22	24	26	28	30	32	35	37	39		22	
23		0	2	3	5	7	8	10	12	14	15	17	19	21	23	25	27	29	31	33	36	38		23	
24		0	2	3	5	6	8	10	11	13	15	17	18	20	22	24	26	28	30	32	34	36		24	
25		0	2	3	5	6	8	9	11	13	14	16	18	19	21	23	25	27	29	31	33	35		25	
26		0	1	3	4	6	7	9	11	12	14	15	17	19	20	22	24	26	28	29	31	33		26	
27		0	1	3	4	6	7	9	10	12	13	15	16	18	19	21	23	25	26	38	30	32		27	
28		0	1	3	4	5	7	8	10	11	12	14	15	17	19	20	22	23	25	27	29	30		28	
29		0	1	3	4	5	6	8	9	11	12	13	15	16	18	19	21	22	24	26	27	29		29	
30		0	1	2	4	5	6	7	9	10	11	13	14	15	17	18	20	21	23	24	26	28		30	
31		0	1	2	3	5	6	7	8	9	11	12	13	15	16	17	19	20	22	23	25	26		31	
32		0	1	2	3	4	6	7	8	9	10	11	13	14	15	16	18	19	20	22	23	25		32	
33		0	1	2	3	4	5	6	7	8	10	11	12	13	14	15	17	18	19	21	22	23		33	
34		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	16	17	18	19	21	22		34	
35		0	1	2	3	4	5	6	6	7	8	9	10	11	13	14	15	16	17	18	19	20		35	
36		0	1	2	3	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19		36	
37		0	1	2	2	3	4	5	6	6	7	8	9	10	11	12	13	14	15	16	17	18		37	
38		0	1	1	2	3	4	4	5	6	7	7	8	9	10	11	12	13	13	14	15	16		38	
39		0	1	1	2	3	3	4	5	5	6	7	8	8	9	10	11	11	12	13	14	15		39	
40		0	1	1	2	2	3	4	4	5	6	6	7	8	8	9	10	10	11	12	13	13		40	
41		0	1	1	2	2	3	3	4	4	5	6	6	7	7	8	9	9	10	11	11	12		41	
42		0	0	1	1	2	2	3	3	4	4	5	5	6	7	7	8	8	9	9	10	11		42	
43		0	0	1	1	2	2	3	3	3	4	4	5	5	6	6	7	7	8	8	9	9		43	
44		0	0	1	1	1	2	2	3	3	3	4	4	4	5	5	6	6	7	7	8	8		44	
45		0	0	1	1	1	1	2	2	2	3	3	3	4	4	4	5	5	6	6	6	7		45	
46		0	0	0	1	1	1	1	2	2	2	2	2	3	3	4	4	4	4	5	5	5		46	
47		0	0	0	1	1	1	1	1	1	2	2	2	2	2	3	3	3	3	4	4	4		47	
48		0	0	0	0	0	1	1	1	1	1	1	1	1	2	2	2	2	2	2	3	3		48	
49		0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1		49	
50		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0		50	
Add.		0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0		Add.	
Thermom.		0'	1'	2'	3'	4'	5'	6'	7'	8'	9'	10'	MEAN REFRACTION.										Thermom.		



# TABLE III. B.

Correction of the Mean Refraction for the Height of the Thermometer.

Thermom.		MEAN REFRACTION.																			Thermom.			
Subtract.		0'		1'		2'		3'		4'		5'		6'		7'		8'		9'		10'	Subtract.	
		0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0		
50	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	50	
51	0	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	51	
52	0	0	0	0	0	0	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2	3	52	
53	0	0	0	0	1	1	1	1	1	1	2	2	2	2	2	3	3	3	3	4	4	4	53	
54	0	0	0	0	1	1	1	1	2	2	2	2	3	3	3	3	4	4	4	5	5	5	54	
55	0	0	0	1	1	1	1	2	2	2	3	3	3	4	4	5	5	5	6	6	6	6	55	
56	0	0	0	1	1	1	2	2	2	3	3	4	4	4	5	5	6	6	7	7	7	8	56	
57	0	0	0	1	1	2	2	2	3	3	4	4	5	5	6	6	6	7	8	8	8	9	57	
58	0	0	0	1	1	2	2	3	3	4	4	5	5	6	6	7	7	8	9	9	10	10	58	
59	0	1	1	2	2	3	3	4	4	5	5	6	6	7	8	8	9	10	10	11	12	59		
60	0	1	1	2	2	3	3	4	5	5	6	7	7	8	9	9	10	11	11	12	13	60		
61	0	1	1	2	3	3	4	4	5	6	7	7	8	9	9	10	11	12	12	13	14	61		
62	0	1	1	2	3	3	4	5	6	6	7	8	9	9	10	11	12	13	14	15	15	62		
63	0	1	1	2	3	4	5	5	6	7	8	8	9	10	11	12	13	14	15	16	17	63		
64	0	1	2	2	3	4	5	6	7	7	8	9	10	11	12	13	14	15	16	17	18	64		
65	0	1	2	3	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	65		
66	0	1	2	3	4	5	6	6	7	8	9	10	11	12	14	15	16	17	18	19	20	66		
67	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	16	17	18	19	20	22	67		
68	0	1	2	3	4	5	6	7	8	9	11	11	13	14	15	16	18	19	20	22	23	68		
69	0	1	2	3	4	5	7	8	9	10	11	12	13	15	16	17	19	20	21	23	24	69		
70	0	1	2	3	5	6	7	8	9	10	12	12	14	16	17	18	20	21	22	24	25	70		
71	0	1	2	4	5	6	7	8	10	11	12	13	15	16	18	19	20	22	23	25	27	71		
72	0	1	2	4	5	6	8	9	10	11	13	14	16	17	18	20	21	23	25	26	28	72		
73	0	1	3	4	5	7	8	9	11	12	13	14	16	18	19	21	22	24	26	27	29	73		
74	0	1	3	4	5	7	8	10	11	12	14	15	17	18	20	22	23	25	27	28	30	74		
75	0	1	3	4	6	7	8	10	11	13	14	16	18	19	21	22	24	26	28	29	31	75		
76	0	1	3	4	6	7	9	10	12	13	15	16	18	20	22	23	25	27	29	31	32	76		
77	0	1	3	5	6	8	9	11	12	14	16	17	19	21	22	24	26	28	30	32	34	77		
78	0	2	3	5	6	8	9	11	13	14	16	18	20	21	23	25	27	29	31	33	35	78		
79	0	2	3	5	6	8	10	11	13	15	17	18	20	22	24	26	28	30	32	34	36	79		
80	0	2	3	5	7	8	10	12	14	15	17	19	21	23	25	27	29	31	33	35	37	80		
81	0	2	3	5	7	9	10	12	14	16	18	20	21	24	26	28	30	32	34	36	38	81		
82	0	2	4	5	7	9	11	13	14	16	18	20	22	24	26	28	31	33	35	37	40	82		
83	0	2	4	5	7	9	11	13	15	17	19	21	23	25	27	29	31	34	36	38	41	83		
84	0	2	4	6	8	9	11	13	15	17	19	21	23	26	28	30	32	35	37	39	42	84		
85	0	2	4	6	8	10	12	14	16	18	20	22	24	26	29	31	33	36	38	40	43	85		
86	0	2	4	6	8	10	12	14	16	18	20	23	25	27	29	32	34	37	39	42	44	86		
87	0	2	4	6	8	10	12	14	17	19	21	23	25	28	30	32	35	38	40	43	45	87		
88	0	2	4	6	8	10	13	15	17	19	21	24	26	28	31	33	36	38	41	44	46	88		
89	0	2	4	6	9	11	13	15	17	20	22	24	27	29	32	34	37	39	42	45	48	89		
90	0	2	4	7	9	11	13	16	18	20	23	25	27	30	32	35	38	40	43	46	49	90		
91	0	2	4	7	9	11	14	16	18	21	23	25	28	31	33	36	39	41	44	47	50	91		
92	0	2	5	7	9	11	14	16	19	21	24	26	29	31	34	37	39	42	45	48	51	92		
93	0	2	5	7	9	12	14	17	19	22	24	27	29	32	35	37	40	43	46	49	52	93		
94	0	2	5	7	10	12	14	17	19	22	25	27	30	33	35	38	41	44	47	50	53	94		
95	0	2	5	7	10	12	15	17	20	22	25	28	30	33	36	39	42	45	48	51	54	95		
96	0	2	5	7	10	12	15	18	20	23	26	28	31	34	37	40	43	46	49	52	55	96		
97	0	3	5	8	10	13	15	18	21	23	26	29	32	35	38	41	44	47	50	53	56	97		
98	0	3	5	8	10	13	16	18	21	24	27	29	32	35	38	41	44	48	51	54	58	98		
99	0	3	5	8	11	13	16	19	21	24	27	30	33	36	39	42	45	49	52	55	59	99		
100	0	3	5	8	11	13	16	19	22	25	28	31	34	37	40	43	46	50	53	56	60	100		
Subtract.	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	30	0	Subtract.
Thermom.	0	1'	2'	3'	4'	5'	6'	7'	8'	9'	10'	Thermom												
MEAN REFRACTION.																								

# TABLE IV. SIDEREAL INTO MEAN SOLAR TIME.

Side- real.	0 h.	1 h.	2 h.	3 h.	4 h.	5 h.	6 h.	7 h.	For Seconds.
m.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	s. s.
0	0 00.000	0 09.830	0 19.659	0 29.489	0 39.318	0 49.148	0 58.977	1 08.807	
1	0 00.164	0 09.993	0 19.823	0 29.653	0 39.482	0 49.312	0 59.141	1 08.971	1 0.003
2	0 00.328	0 10.157	0 19.987	0 29.816	0 39.646	0 49.475	0 59.305	1 09.135	2 .005
3	0 00.491	0 10.321	0 20.151	0 29.980	0 39.810	0 49.639	0 59.469	1 09.298	3 .008
4	0 00.655	0 10.485	0 20.314	0 30.144	0 39.974	0 49.803	0 59.633	1 09.462	4 .011
5	0 00.819	0 10.649	0 20.478	0 30.308	0 40.137	0 49.967	0 59.796	1 09.626	5 .014
6	0 00.983	0 10.813	0 20.642	0 30.472	0 40.301	0 50.131	0 59.960	1 09.790	6 .016
7	0 01.147	0 10.976	0 20.806	0 30.635	0 40.465	0 50.295	1 00.124	1 09.954	7 .019
8	0 01.311	0 11.140	0 20.970	0 30.799	0 40.629	0 50.458	1 00.288	1 10.118	8 .022
9	0 01.474	0 11.304	0 21.134	0 30.963	0 40.793	0 50.622	1 00.452	1 10.281	9 .025
10	0 01.638	0 11.468	0 21.297	0 31.127	0 40.956	0 50.786	1 00.616	1 10.445	10 .027
11	0 01.802	0 11.632	0 21.461	0 31.291	0 41.120	0 50.950	1 00.779	1 10.609	11 .030
12	0 01.966	0 11.795	0 21.625	0 31.455	0 41.284	0 51.114	1 00.943	1 10.773	12 .033
13	0 02.130	0 11.959	0 21.789	0 31.618	0 41.448	0 51.278	1 01.107	1 10.937	13 .035
14	0 02.294	0 12.123	0 21.953	0 31.782	0 41.612	0 51.441	1 01.271	1 11.100	14 .038
15	0 02.457	0 12.287	0 22.117	0 31.946	0 41.776	0 51.605	1 01.435	1 11.264	15 .041
16	0 02.621	0 12.451	0 22.280	0 32.110	0 41.939	0 51.769	1 01.599	1 11.428	16 .044
17	0 02.785	0 12.615	0 22.444	0 32.274	0 42.103	0 51.933	1 01.762	1 11.592	17 .046
18	0 02.949	0 12.778	0 22.608	0 32.438	0 42.267	0 52.097	1 01.926	1 11.756	18 .049
19	0 03.113	0 12.942	0 22.772	0 32.601	0 42.431	0 52.260	1 02.090	1 11.920	19 .052
20	0 03.277	0 13.106	0 22.936	0 32.765	0 42.595	0 52.424	1 02.254	1 12.083	20 .055
21	0 03.440	0 13.270	0 23.099	0 32.929	0 42.759	0 52.588	1 02.418	1 12.247	21 .057
22	0 03.604	0 13.434	0 23.263	0 33.093	0 42.922	0 52.752	1 02.582	1 12.411	22 .060
23	0 03.768	0 13.598	0 23.427	0 33.257	0 43.086	0 52.916	1 02.745	1 12.575	23 .063
24	0 03.932	0 13.761	0 23.591	0 33.420	0 43.250	0 53.080	1 02.909	1 12.739	24 .066
25	0 04.096	0 13.925	0 23.755	0 33.584	0 43.414	0 53.243	1 03.073	1 12.903	25 .068
26	0 04.259	0 14.089	0 23.919	0 33.748	0 43.578	0 53.407	1 03.237	1 13.066	26 .071
27	0 04.423	0 14.253	0 24.082	0 33.912	0 43.742	0 53.571	1 03.401	1 13.230	27 .074
28	0 04.587	0 14.417	0 24.246	0 34.076	0 43.905	0 53.735	1 03.564	1 13.394	28 .076
29	0 04.751	0 14.581	0 24.410	0 34.240	0 44.069	0 53.899	1 03.728	1 13.558	29 .079
30	0 04.915	0 14.744	0 24.574	0 34.403	0 44.233	0 54.063	1 03.892	1 13.722	30 .082
31	0 05.079	0 14.908	0 24.738	0 34.567	0 44.397	0 54.226	1 04.056	1 13.886	31 .085
32	0 05.242	0 15.072	0 24.902	0 34.731	0 44.561	0 54.390	1 04.220	1 14.049	32 .087
33	0 05.406	0 15.236	0 25.065	0 34.895	0 44.724	0 54.554	1 04.384	1 14.213	33 .090
34	0 05.570	0 15.400	0 25.229	0 35.059	0 44.888	0 54.718	1 04.547	1 14.377	34 .093
35	0 05.734	0 15.563	0 25.393	0 35.223	0 45.052	0 54.882	1 04.711	1 14.541	35 .096
36	0 05.898	0 15.727	0 25.557	0 35.386	0 45.216	0 55.046	1 04.875	1 14.705	36 .098
37	0 06.062	0 15.891	0 25.721	0 35.550	0 45.380	0 55.209	1 05.039	1 14.868	37 .101
38	0 06.225	0 16.055	0 25.885	0 35.714	0 45.544	0 55.373	1 05.203	1 15.032	38 .104
39	0 06.389	0 16.219	0 26.048	0 35.878	0 45.707	0 55.537	1 05.367	1 15.196	39 .106
40	0 06.553	0 16.383	0 26.212	0 36.042	0 45.871	0 55.701	1 05.530	1 15.360	40 .109
41	0 06.717	0 16.546	0 26.376	0 36.206	0 46.035	0 55.865	1 05.694	1 15.524	41 .112
42	0 06.881	0 16.710	0 26.540	0 36.369	0 46.199	0 56.028	1 05.858	1 15.688	42 .115
43	0 07.045	0 16.874	0 26.704	0 36.533	0 46.363	0 56.192	1 06.022	1 15.851	43 .117
44	0 07.208	0 17.038	0 26.867	0 36.697	0 46.527	0 56.356	1 06.186	1 16.015	44 .120
45	0 07.372	0 17.202	0 27.031	0 36.861	0 46.690	0 56.520	1 06.350	1 16.179	45 .123
46	0 07.536	0 17.366	0 27.195	0 37.025	0 46.854	0 56.684	1 06.513	1 16.343	46 .126
47	0 07.700	0 17.529	0 27.359	0 37.188	0 47.018	0 56.848	1 06.677	1 16.507	47 .128
48	0 07.864	0 17.693	0 27.523	0 37.352	0 47.182	0 57.011	1 06.841	1 16.671	48 .131
49	0 08.027	0 17.857	0 27.687	0 37.516	0 47.346	0 57.175	1 07.005	1 16.834	49 .134
50	0 08.191	0 18.021	0 27.850	0 37.680	0 47.510	0 57.339	1 07.169	1 16.998	50 .137
51	0 08.355	0 18.185	0 28.014	0 37.844	0 47.673	0 57.503	1 07.332	1 17.162	51 .139
52	0 08.519	0 18.349	0 28.178	0 38.008	0 47.837	0 57.667	1 07.496	1 17.326	52 .142
53	0 08.683	0 18.512	0 28.342	0 38.171	0 48.001	0 57.831	1 07.660	1 17.490	53 .145
54	0 08.847	0 18.676	0 28.506	0 38.335	0 48.165	0 57.994	1 07.824	1 17.654	54 .147
55	0 09.010	0 18.840	0 28.670	0 38.499	0 48.329	0 58.158	1 07.988	1 17.817	55 .150
56	0 09.174	0 19.004	0 28.833	0 38.663	0 48.492	0 58.322	1 08.152	1 17.981	56 .153
57	0 09.338	0 19.168	0 28.997	0 38.827	0 48.656	0 58.486	1 08.315	1 18.145	57 .156
58	0 09.502	0 19.331	0 29.161	0 38.991	0 48.820	0 58.650	1 08.479	1 18.309	58 .158
59	0 09.666	0 19.495	0 29.325	0 39.154	0 48.984	0 58.814	1 08.643	1 18.473	59 .161



# TABLE IV. SIDEREAL INTO MEAN SOLAR TIME.

Sidereal	8 h.	9 h.	10 h.	11 h.	12 h.	13 h.	14 h.	15 h.	For Seconds.
m.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	s. s.
0	1 18.636	1 28.466	1 38.296	1 48.125	1 57.955	2 07.784	2 17.614	2 27.443	
1	1 18.800	1 28.630	1 38.459	1 48.289	1 58.119	2 07.948	2 17.778	2 27.607	1 0.003
2	1 18.964	1 28.794	1 38.623	1 48.453	1 58.282	2 08.112	2 17.941	2 27.771	2 .005
3	1 19.128	1 28.958	1 38.787	1 48.617	1 58.446	2 08.276	2 18.105	2 27.935	3 .008
4	1 19.292	1 29.121	1 38.951	1 48.780	1 58.610	2 08.440	2 18.269	2 28.099	4 .011
5	1 19.456	1 29.285	1 39.115	1 48.944	1 58.774	2 08.603	2 18.433	2 28.263	5 .014
6	1 19.619	1 29.449	1 39.279	1 49.108	1 58.938	2 08.767	2 18.597	2 28.426	6 .016
7	1 19.783	1 29.613	1 39.442	1 49.272	1 59.101	2 08.931	2 18.761	2 28.590	7 .019
8	1 19.947	1 29.777	1 39.606	1 49.436	1 59.265	2 09.095	2 18.924	2 28.754	8 .022
9	1 20.111	1 29.940	1 39.770	1 49.600	1 59.429	2 09.259	2 19.088	2 28.918	9 .025
10	1 20.275	1 30.104	1 39.934	1 49.763	1 59.593	2 09.423	2 19.252	2 29.082	10 .027
11	1 20.439	1 30.268	1 40.098	1 49.927	1 59.757	2 09.586	2 19.416	2 29.245	11 .030
12	1 20.602	1 30.432	1 40.261	1 50.091	1 59.921	2 09.750	2 19.580	2 29.409	12 .033
13	1 20.766	1 30.596	1 40.425	1 50.255	2 00.084	2 09.914	2 19.744	2 29.573	13 .035
14	1 20.930	1 30.760	1 40.589	1 50.419	2 00.248	2 10.078	2 19.907	2 29.737	14 .038
15	1 21.094	1 30.923	1 40.753	1 50.583	2 00.412	2 10.242	2 20.071	2 29.901	15 .041
16	1 21.258	1 31.087	1 40.917	1 50.746	2 00.576	2 10.405	2 20.235	2 30.065	16 .044
17	1 21.422	1 31.251	1 41.081	1 50.910	2 00.740	2 10.569	2 20.399	2 30.228	17 .046
18	1 21.585	1 31.415	1 41.244	1 51.074	2 00.904	2 10.733	2 20.563	2 30.392	18 .049
19	1 21.749	1 31.579	1 41.408	1 51.238	2 01.067	2 10.897	2 20.727	2 30.556	19 .052
20	1 21.913	1 31.743	1 41.572	1 51.402	2 01.231	2 11.061	2 20.890	2 30.720	20 .055
21	1 22.077	1 31.906	1 41.736	1 51.565	2 01.395	2 11.225	2 21.054	2 30.884	21 .057
22	1 22.241	1 32.070	1 41.900	1 51.729	2 01.559	2 11.388	2 21.218	2 31.048	22 .060
23	1 22.404	1 32.234	1 42.064	1 51.893	2 01.723	2 11.552	2 21.382	2 31.211	23 .063
24	1 22.568	1 32.398	1 42.227	1 52.057	2 01.887	2 11.716	2 21.546	2 31.375	24 .066
25	1 22.732	1 32.562	1 42.391	1 52.221	2 02.050	2 11.880	2 21.709	2 31.539	25 .068
26	1 22.896	1 32.726	1 42.555	1 52.385	2 02.214	2 12.044	2 21.873	2 31.703	26 .071
27	1 23.060	1 32.889	1 42.719	1 52.548	2 02.378	2 12.208	2 22.037	2 31.867	27 .074
28	1 23.224	1 33.053	1 42.883	1 52.712	2 02.542	2 12.371	2 22.201	2 32.031	28 .076
29	1 23.387	1 33.217	1 43.047	1 52.876	2 02.706	2 12.535	2 22.365	2 32.194	29 .079
30	1 23.551	1 33.381	1 43.210	1 53.040	2 02.869	2 12.699	2 22.529	2 32.358	30 .082
31	1 23.715	1 33.545	1 43.374	1 53.204	2 03.033	2 12.863	2 22.692	2 32.522	31 .085
32	1 23.879	1 33.708	1 43.538	1 53.368	2 03.197	2 13.027	2 22.856	2 32.686	32 .087
33	1 24.043	1 33.872	1 43.702	1 53.531	2 03.361	2 13.191	2 23.020	2 32.850	33 .090
34	1 24.207	1 34.036	1 43.866	1 53.695	2 03.525	2 13.354	2 23.184	2 33.013	34 .093
35	1 24.370	1 34.200	1 44.029	1 53.859	2 03.689	2 13.518	2 23.348	2 33.177	35 .096
36	1 24.534	1 34.364	1 44.193	1 54.023	2 03.852	2 13.682	2 23.512	2 33.341	36 .098
37	1 24.698	1 34.528	1 44.357	1 54.187	2 04.016	2 13.846	2 23.675	2 33.505	37 .101
38	1 24.862	1 34.691	1 44.521	1 54.351	2 04.180	2 14.010	2 23.839	2 33.669	38 .104
39	1 25.026	1 34.855	1 44.685	1 54.514	2 04.344	2 14.173	2 24.003	2 33.833	39 .106
40	1 25.190	1 35.019	1 44.849	1 54.678	2 04.508	2 14.337	2 24.167	2 33.996	40 .109
41	1 25.353	1 35.183	1 45.012	1 54.842	2 04.672	2 14.501	2 24.331	2 34.160	41 .112
42	1 25.517	1 35.347	1 45.176	1 55.006	2 04.835	2 14.665	2 24.495	2 34.324	42 .115
43	1 25.681	1 35.511	1 45.340	1 55.170	2 04.999	2 14.829	2 24.658	2 34.488	43 .117
44	1 25.845	1 35.674	1 45.504	1 55.333	2 05.163	2 14.993	2 24.822	2 34.652	44 .120
45	1 26.009	1 35.838	1 45.668	1 55.497	2 05.327	2 15.156	2 24.986	2 34.816	45 .123
46	1 26.172	1 36.002	1 45.832	1 55.661	2 05.491	2 15.320	2 25.150	2 34.979	46 .126
47	1 26.336	1 36.166	1 45.995	1 55.825	2 05.655	2 15.484	2 25.314	2 35.143	47 .128
48	1 26.500	1 36.330	1 46.159	1 55.989	2 05.818	2 15.648	2 25.477	2 35.307	48 .131
49	1 26.664	1 36.493	1 46.323	1 56.153	2 05.982	2 15.812	2 25.641	2 35.471	49 .134
50	1 26.828	1 36.657	1 46.487	1 56.316	2 06.146	2 15.976	2 25.805	2 35.635	50 .137
51	1 26.992	1 36.821	1 46.651	1 56.480	2 06.310	2 16.139	2 25.969	2 35.798	51 .139
52	1 27.155	1 36.985	1 46.815	1 56.644	2 06.474	2 16.303	2 26.133	2 35.962	52 .142
53	1 27.319	1 37.149	1 46.978	1 56.808	2 06.637	2 16.467	2 26.297	2 36.126	53 .145
54	1 27.483	1 37.313	1 47.142	1 56.972	2 06.801	2 16.631	2 26.460	2 36.290	54 .147
55	1 27.647	1 37.476	1 47.306	1 57.136	2 06.965	2 16.795	2 26.624	2 36.454	55 .150
56	1 27.811	1 37.640	1 47.470	1 57.299	2 07.129	2 16.959	2 26.788	2 36.618	56 .153
57	1 27.975	1 37.804	1 47.634	1 57.463	2 07.293	2 17.122	2 26.952	2 36.781	57 .156
58	1 28.138	1 37.968	1 47.797	1 57.627	2 07.457	2 17.286	2 27.116	2 36.945	58 .158
59	1 28.302	1 38.132	1 47.961	1 57.791	2 07.620	2 17.450	2 27.280	2 37.109	59 .161



# TABLE IV. SIDEREAL INTO MEAN SOLAR TIME.

Sidereal	16 h.	17 h.	18 h.	19 h.	20 h.	21 h.	22 h.	23 h.	For Seconds.
m.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	s.
0	2 37.273	2 47.102	2 56.932	3 06.762	3 16.591	3 26.421	3 36.250	3 46.080	1 0.003
1	2 37.437	2 47.266	2 57.096	3 06.925	3 16.755	3 26.585	3 36.414	3 46.244	2 .005
2	2 37.601	2 47.430	2 57.260	3 07.089	3 16.919	3 26.748	3 36.578	3 46.407	3 .008
3	2 37.764	2 47.594	2 57.424	3 07.253	3 17.083	3 26.912	3 36.742	3 46.571	4 .011
4	2 37.928	2 47.758	2 57.587	3 07.417	3 17.246	3 27.076	3 36.906	3 46.735	5 .014
5	2 38.092	2 47.922	2 57.751	3 07.581	3 17.410	3 27.240	3 37.069	3 46.899	6 .016
6	2 38.256	2 48.085	2 57.915	3 07.745	3 17.574	3 27.404	3 37.233	3 47.063	7 .019
7	2 38.420	2 48.249	2 58.079	3 07.908	3 17.738	3 27.568	3 37.397	3 47.227	8 .022
8	2 38.584	2 48.413	2 58.243	3 08.072	3 17.902	3 27.731	3 37.561	3 47.390	9 .025
9	2 38.747	2 48.577	2 58.406	3 08.236	3 18.066	3 27.895	3 37.725	3 47.554	10 .027
10	2 38.911	2 48.741	2 58.570	3 08.400	3 18.229	3 28.059	3 37.889	3 47.718	11 .030
11	2 39.075	2 48.905	2 58.734	3 08.564	3 18.393	3 28.223	3 38.052	3 47.882	12 .033
12	2 39.239	2 49.068	2 58.898	3 08.728	3 18.557	3 28.387	3 38.216	3 48.046	13 .035
13	2 39.403	2 49.232	2 59.062	3 08.891	3 18.721	3 28.550	3 38.380	3 48.210	14 .038
14	2 39.566	2 49.396	2 59.226	3 09.055	3 18.885	3 28.714	3 38.544	3 48.373	15 .041
15	2 39.730	2 49.560	2 59.389	3 09.219	3 19.049	3 28.878	3 38.708	3 48.537	16 .044
16	2 39.894	2 49.724	2 59.553	3 09.383	3 19.212	3 29.042	3 38.871	3 48.701	17 .046
17	2 40.058	2 49.888	2 59.717	3 09.547	3 19.376	3 29.206	3 39.035	3 48.865	18 .049
18	2 40.222	2 50.051	2 59.881	3 09.710	3 19.540	3 29.370	3 39.199	3 49.029	19 .052
19	2 40.386	2 50.215	3 00.045	3 09.874	3 19.704	3 29.533	3 39.363	3 49.193	20 .055
20	2 40.549	2 50.379	3 00.209	3 10.038	3 19.868	3 29.697	3 39.527	3 49.356	21 .057
21	2 40.713	2 50.543	3 00.372	3 10.202	3 20.032	3 29.861	3 39.691	3 49.520	22 .060
22	2 40.877	2 50.707	3 00.536	3 10.366	3 20.195	3 30.025	3 39.854	3 49.684	23 .063
23	2 41.041	2 50.870	3 00.700	3 10.530	3 20.359	3 30.189	3 40.018	3 49.848	24 .066
24	2 41.205	2 51.034	3 00.864	3 10.693	3 20.523	3 30.353	3 40.182	3 50.012	25 .068
25	2 41.369	2 51.198	3 01.028	3 10.857	3 20.687	3 30.516	3 40.346	3 50.175	26 .071
26	2 41.532	2 51.362	3 01.192	3 11.021	3 20.851	3 30.680	3 40.510	3 50.339	27 .074
27	2 41.696	2 51.526	3 01.355	3 11.185	3 21.014	3 30.844	3 40.674	3 50.503	28 .076
28	2 41.860	2 51.690	3 01.519	3 11.349	3 21.178	3 31.008	3 40.837	3 50.667	29 .079
29	2 42.024	2 51.853	3 01.683	3 11.513	3 21.342	3 31.172	3 41.001	3 50.831	30 .082
30	2 42.188	2 52.017	3 01.847	3 11.676	3 21.506	3 31.336	3 41.165	3 50.995	31 .085
31	2 42.352	2 52.181	3 02.011	3 11.840	3 21.670	3 31.499	3 41.329	3 51.158	32 .087
32	2 42.515	2 52.345	3 02.174	3 12.004	3 21.834	3 31.663	3 41.493	3 51.322	33 .090
33	2 42.679	2 52.509	3 02.338	3 12.168	3 21.997	3 31.827	3 41.657	3 51.486	34 .093
34	2 42.843	2 52.673	3 02.502	3 12.332	3 22.161	3 31.991	3 41.820	3 51.650	35 .096
35	2 43.007	2 52.836	3 02.666	3 12.496	3 22.325	3 32.155	3 41.984	3 51.814	36 .098
36	2 43.171	2 53.000	3 02.830	3 12.659	3 22.489	3 32.318	3 42.148	3 51.978	37 .101
37	2 43.334	2 53.164	3 02.994	3 12.823	3 22.653	3 32.482	3 42.312	3 52.141	38 .104
38	2 43.498	2 53.328	3 03.157	3 12.987	3 22.817	3 32.646	3 42.476	3 52.305	39 .106
39	2 43.662	2 53.492	3 03.321	3 13.151	3 22.980	3 32.810	3 42.639	3 52.469	40 .109
40	2 43.826	2 53.656	3 03.485	3 13.315	3 23.144	3 32.974	3 42.803	3 52.633	41 .112
41	2 43.990	2 53.819	3 03.649	3 13.478	3 23.308	3 33.138	3 42.967	3 52.797	42 .115
42	2 44.154	2 53.983	3 03.813	3 13.642	3 23.472	3 33.301	3 43.131	3 52.961	43 .117
43	2 44.317	2 54.147	3 03.977	3 13.806	3 23.636	3 33.465	3 43.295	3 53.124	44 .120
44	2 44.481	2 54.311	3 04.140	3 13.970	3 23.800	3 33.629	3 43.459	3 53.288	45 .123
45	2 44.645	2 54.475	3 04.304	3 14.134	3 23.963	3 33.793	3 43.622	3 53.452	46 .126
46	2 44.809	2 54.638	3 04.468	3 14.298	3 24.127	3 33.957	3 43.786	3 53.616	47 .128
47	2 44.973	2 54.802	3 04.632	3 14.461	3 24.291	3 34.121	3 43.950	3 53.780	48 .131
48	2 45.137	2 54.966	3 04.796	3 14.625	3 24.455	3 34.284	3 44.114	3 53.943	49 .134
49	2 45.300	2 55.130	3 04.960	3 14.789	3 24.619	3 34.448	3 44.278	3 54.107	50 .137
50	2 45.464	2 55.294	3 05.123	3 14.953	3 24.782	3 34.612	3 44.442	3 54.271	51 .139
51	2 45.628	2 55.458	3 05.287	3 15.117	3 24.946	3 34.776	3 44.605	3 54.435	52 .142
52	2 45.792	2 55.621	3 05.451	3 15.281	3 25.110	3 34.940	3 44.769	3 54.599	53 .145
53	2 45.956	2 55.785	3 05.615	3 15.444	3 25.274	3 35.104	3 44.933	3 54.763	54 .147
54	2 46.120	2 55.949	3 05.779	3 15.608	3 25.438	3 35.267	3 45.097	3 54.926	55 .150
55	2 46.283	2 56.113	3 05.942	3 15.772	3 25.602	3 35.431	3 45.261	3 55.090	56 .153
56	2 46.447	2 56.277	3 06.106	3 15.936	3 25.765	3 35.595	3 45.425	3 55.254	57 .156
57	2 46.611	2 56.441	3 06.270	3 16.100	3 25.929	3 35.759	3 45.588	3 55.418	58 .158
58	2 46.775	2 56.604	3 06.434	3 16.264	3 26.093	3 35.923	3 45.752	3 55.582	59 .161
59	2 46.939	2 56.768	3 06.598	3 16.427	3 26.257	3 36.086	3 45.916	3 55.746	

# ADDRESS

OF

PROFESSOR BENJAMIN PEIRCE,

PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE YEAR 1853,

ON RETIRING FROM THE DUTIES OF PRESIDENT.

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[Printed by Order of the Association.]

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MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR  
THE ADVANCEMENT OF SCIENCE : —

In most offices, the duties terminate with the office, and the thing of the past, the ex-officer, is to the present an unknown quantity. But it is not so with your President. Science, with its time-annihilating power, which gives life to the fossil, which hurries the embryo future into premature birth, which ventures beyond the grave even to the foot of the invisible throne, sternly drags forward its reluctant presidents to their hardest trial when they have ceased to be, to a judgment after death severer than that of Rhadamanthus. This calling out of the actor upon the stage after the night of performance, when the blood is no longer warm, is all the worse to him who has never before made a set speech, all whose habits of thought are unknown to æsthetic display, and the Arctic latitudes of whose frigid studies are impenetrable to the God of eloquence and to the Muses who vibrate the silver-toned chords of human sympathy.

Geometry, to which I have devoted my life, is honored with the title of the Key of the Sciences ; but it is the key of

an ever-open door, which refuses to be shut, and through which the whole world is crowding, to make free, in unrestrained license, with the precious treasures within, thoughtless both of lock and key, of the door itself, and even of science, to which it owes such boundless possessions, this New World included. The door is wide open, and all may enter; but all do not enter with equal thoughtlessness. There are a few who wonder, as they approach, at the exhaustless wealth, as the sacred shepherd wondered at the burning bush of Horeb, which was ever burning and never consumed. Casting their shoes from off their feet, and the world's iron-shod doubts from their understanding, these children of the faithful take their first step upon the holy ground with reverential awe, and advance almost with timidity, fearful, as the signs of Deity break upon them, lest they shall be brought face to face with the Almighty. They are the searchers after truth, and do not pass the door or the key without careful scrutiny.

The key! It is of wonderful construction, with its infinity of combination, and its unlimited capacity to fit every lock, however varied in form and size; it closes the massive arches which guard the vaults whence the mechanic arts supply the warehouses of commerce, and it opens the minute cabinet in which the queen of the fairies protects her microscopic jewels; it is the great master-key, which unlocks every door of knowledge, and without which no discovery — no discovery which deserves the name, which is law and not isolated fact — has been or ever can be made. Fascinated by its symmetry, the geometer may, at times, have been too exclusively engrossed with his science, forgetful of its applications; he may have exalted it into his idol, and worshipped it; he may have degraded it into his toy, and childishly amused himself with the singular shapes which it would assume, when he should have been hard at work with it, using it for the benefit of mankind and the glory of his Creator. I have seen a watchmaker, who came into possession of a remarkable chronometer, which was made by a prince of the craft, — by one who only made a single



chronometer in a year; but that single watch was a masterpiece of art, and in every part a model of exquisite workmanship. The single-hearted watchmaker would sit and gaze at the neat key of his chronometer by the hour together, wasting in admiration the precious time, which his faithful watch continued to measure. With the same simplicity of devotion, the mathematician, unable to resist her charms, may embrace his science too ardently, when it lies close to his heart; and thus her integrity may be suspected. But ascend with me above the dust, above the cloud, to the realms of the higher geometry, where the heavens are never obscured; where there is no impure vapor and no delusive or imperfect observation; where the new truths are already arisen, while they are yet dimly dawning upon the earth below; where the earth is a little planet; where the sun has dwindled to a star; where all the stars are lost in the Milky-Way to which they belong; where the Milky-Way is seen floating through space, like any other nebula; where the whole great girdle of the *nebulæ* has diminished to an atom, and has become as readily and completely submissive to the pen of the geometer, and the slave of his formula, as the single drop, which falls from the cloud, instinct with all the forces of the material world. Try with me the precision of measure with which the universe has been meted out; observe how exactly all the parts are fitted to the whole and to each other, and then declare who was present in the council-chamber when the Lord laid the foundations of the earth.

Begin with the heavens themselves; see how precisely the motions of the firmament have endured through the friction of the ages; observe the exactness of the revolutions of the stars; if these mighty orbs cannot resist the law, what can the atom do? Let, then, the resources of art be exhausted in this scrutiny. Let neither time nor labor nor money be spared. A slight defect of motion is just detected; it is slight, very slight, but it is unquestionable. We dare not hide it out of sight. Science must admit this triumph of art, and be true even if the stars are false. The names of fixed star

and pole-star must not be suffered to impose upon the trusting world, and guide it in a delusive chase after an *ignis fatuus*. Geometry! to the rescue! Geometry is at her post, faithful among the faithless. The pen is at work, the midnight oil consumed, the magic circles drawn by the wise men of the East, and the wizard logarithm summoned from the North. The tables are turned. The defect of motion is transformed into the discovery of a new law. It becomes the proof of the atmosphere to bend the ray from its course as it shoots down, laden with the image of Arcturus and the sweet influences of the Pleiades; it becomes the proof of the moving light, of the unseen planet, and of the invisible star, and hence a new proof of the precision of the measure. Honor to Bradley, to Bessel, to Adams, and to Leverrier! The stars are not false. Question them as you may, they give the same evidence, and do not contradict each other's testimony. They tell us that ours is not the central sun, and that we are moving in the procession of the stars; they tell us that we move among the others, towards the constellation of Hercules, so that, while we grow in wisdom, we approach the strong man's home. They tell us that we are moving at such a rate, that the distance from star to star is but just a good geological day's journey; and hereby they confirm the story which is written upon the crust of our globe, and prove that the earth and the skies have been measured out with the same unit of measure.

Descend from the infinite to the infinitesimal. Long before Bacon and Galileo, before observation had begun to penetrate the veil under which Nature has hidden her mysteries, the restless mind sought some principle of power, strong enough, and of sufficient variety, to collect and bind together all the parts of a world. This seemed to be found, where one might least expect it, in abstract number. Everywhere the exactest numerical proportion was seen to constitute the spiritual element of the highest beauty. It was the harmony of music, and the music of song; the fastidious eye of the Athenian required the delicately curved outlines of the temple

in which he worshipped his goddess to conform to the exact law of the hyperbola, and he traced the graceful features of her statue from the repulsive wrinkles of Arithmetic. Throughout nature, the omnipresent beautiful revealed an all-pervading language spoken to the human mind, and to man's highest capacity of comprehension. By whom was it spoken? Whether by the gods of the ocean and the land, by the ruling divinities of the sun, moon, and stars, or by the nymphs of the forest and the dryads of the fountain, it was one speech, and its written cipher was cabalistic. The cabala were those of number, and even if they transcended the gematric skill of the Rabbi and the hieroglyphical learning of the priest of Osiris, they were, distinctly and unmistakably, expressions of thought, uttered to mind by mind; they were the solutions of mathematical problems of extraordinary complexity. The bee of Hymettus solved its great problem of isoperimetry on the morning of creation; and the sword which threatened the life of Damocles vibrated the elliptic functions two thousand years before Legendre, Abel, and Jacobi had gained immortality by their discussion. The very spirits of the winds, when they were sent to carry the grateful harvest to the thirsting fields of Calabria, did not forget the geometry which they had studied in the caverns of *Æolus*, and of which the geologist is daily discovering their diagrams. When they traversed the forest, they vibrated the bending branches and the hanging vines into every variety of elastic and catenarian curve; as they passed over the city, they wreathed the rising smoke into spirals, at which the ancient philosopher could only gaze in admiration, as it ascended with double curvature in its lofty exponential path, and was lost to computation ere it vanished from sight; and even when, forgetting their beneficent mission, they raged, that awful night at sea, in a fearful struggle with gravitation for the dominion of the ocean, amidst the shrieks of the drowning sailor, they heaped up the waves into such majestic mountains, that the genius of the storm thundered his approbation,



and man's analysis shrunk from the investigation of the strange forms, not daring even to give them a name.

Ancient philosophy, perceiving this power of number, did homage to it in all the simplicity, earnestness, and truthfulness which distinguished the early thinkers. Pythagoras and Plato, the founders of pure mathematics, turned their search inward to find in their own minds the origin of that force, which a universe of phenomena could only reveal in its effects, but not in its essence. They found there a principle, capable of ruling, restraining, and satisfying the extravagant fancy, the ardent imagination, and the licentious will, and of reducing all the faculties to harmonious and consistent action. Its dignified exterior was cold and forbidding, and marked with the gloomy inflexibility of the representative of justice, rather than with the gracious supremacy of a sovereign. Boldly penetrating it, they were rewarded with visions of sublime contemplation, such as the world had never yet beheld; and the majestic glories which surround the throne of number, to those few who are permitted to behold them, took their hearts captive. In the intensity of their enthusiasm, they unconsciously overstepped the bounds of human knowledge, and strove to grope their way where the torch of observation was not yet lighted. They sought in the monad, the duad, and the triad, the mysteries of the Divine nature; in the perfect number, the archetype of the highest good; and in such simple numerical ratios as their unaided reason could devise, the complicated logic of all life. The school-boy of the nineteenth century can detect and ridicule their errors, but the lovers of truth will always revere their memories, and the great discoverers will never cease to find in their magnificent investigations the elements of further progress. Ay! more than this! Modern science has realized some of the most fanciful of the Pythagorean and Platonic doctrines, and thereby justified the divinity of their spiritual instincts. Is it not significant of the nature of the creative intellect, the simplicity of the great laws of force? the fact that the same curious series

of numbers is developed by the growing plant which assisted in marshalling the order of the planets? and that the marriage of the elements cannot be consummated except in strict accordance with the laws of definite proportion? This last extraordinary discovery has rescued chemistry for ever from the blighting thrall of superstition, and, elevating it to the rank of a science, has endowed it with the rudiments of a peculiar speech. The promise has just been given, by one of our own number, of a large extension of this fruitful law; Young America has given the pledge, and we have faith in her chivalry that she will redeem it; we may then hope that the atomic force will submit to some Newton of chemistry, and the formula of the crystal become as legible as that of the solar system. In all parts of the physical world, in sound and light, in electricity and magnetism, in the elements of the air and the ocean, the same precision is everywhere predominant. The tints of the morning cloud reflecting the smiles of Aurora, and the angry flash of the tempest, are equally exact expressions of the unwavering formula; and the geological Titans, sons of Vulcan and Neptune, who once piled Pelion upon Ossa and strewed the earth with the fragments of their battles, and more recently have arrayed the armies of science in unnatural conflict, are at length bound to the primitive rock of immutable law, by the same strong, embracing, golden chain of inductive argument, with which our Franklin "dragged the thunderer down to earth."

Every new discovery in science has now become a new conquest to geometry. Quantitative analysis is regarded as the only safe instrument of research, and the question of the "What kind?" is universally merged into that of the "How much?" This is not limited to the physical sciences; even in politics, the statesman, finding in each land all kinds of men and every element of public economy, is forced to inquire how much there is of each, and to be guided, in the conduct of government, by the figures of statistics. Can it, then, be otherwise than that the science which takes especial charge

of the theory and laws of exact measurement should be of universal application? However distorted it may be in its technical forms, and diverted from its natural position by the injudicious zeal of its votaries, its fundamental principles are those of sound logic and good common-sense, and whatever it touches it elucidates and illuminates. There are many questions in which it might be advantageously consulted further than has yet been done, and it appears to me that the difficulty in regard to the claims to discovery would often be settled by its judicious application, although it might sometimes, perhaps, be tempted to divide an ill-begotten child with the sword of justice, so as to give each claimant his worthless share. But it would grant no countenance to that miserable spirit of scientific adventure, which, by a moderate fertility in suggesting possible solutions of an abstruse problem, lays the foundation for a claim adverse to the just rights of him who, by exact and profound investigation, has demonstrated the true doctrine. By the severity of the standard which it would establish, it might even compel science to renounce some of its pretended acquisitions. In Astronomy, for instance, it must be conceded that Saturn and Jupiter, Mars and Venus, are yet subject to unexplained irregularities of motion; that the theory of the asteroids has not advanced beyond the earliest stage of arithmetic; that the rings of Saturn are connected with their primary by a force not less mysterious than that which holds its golden representative upon the finger of the fair betrothed; and that the laws under which the tides obey the attractions of the sun and moon are quite undeveloped. The remarkable researches upon this subject made in the Coast Survey, have established that here still remains another world to be conquered, worthy the ambition of the Alexander of Geodesy.

There is, however, a broader basis than that of numerical accuracy for maintaining the central position of Geometry among the sciences. It is that of form; the grand type of structural combination. This element may often be deficient



in the technical mathematician, but it is the characteristic feature of the imperial intellects of geometry, of Archimedes and Hipparchus, Newton and Leibnitz, Laplace and Lagrange, Monge and Gauss. It equally belongs to great ability in every department of knowledge and art, and directs all successful effort, from the brilliant campaign of the conqueror to the invention of the printing-press. It is the alpha and omega of intelligible speech, the architect of the poetic temple, the founder of empires, and the maker of constitutions. It is the power of combining innumerable details into a consistent whole, the highest exertion of human genius, and that which approaches nearest to the act of creation. It deciphers the hieroglyphic of events, and, uniting the present with the past and the future, it is the veracity of history and the inspiration of prophecy. It planned the vast fabric of the Reformation, and it touched with its miraculous finger the eyeballs of that statesman who foresaw, in its full development, the mighty tree which now overshadows this continent, when it was concentrated in the seed of liberty, and just germinating in the blood of the patriot. But with all its grandeur, this principle is subject to the laws of necessity and exactness, and when it ventures to build upon the sands of hypothesis and speculation, or the quicksands of *a priori* argument, the fall of its cathedral is certain, and only hastened by the weight of the massive towers. The rash system of philosophy which, despising the science it cannot comprehend, presumes to soar capriciously above the well-established theories of inductive demonstration, must melt its ill-cemented pinions as it approaches the source of truth, and sink, like Icarus, into deserved ridicule and contempt. The imagination of the immortal Kepler himself would have wasted all its strength in a wild and whimsical race with the mysteries of cosmography, if it had not been restrained from its extravagance by his sincere love of truth, and soberly harnessed to the observations of Tycho. The gaudy firmament of the artificial globe of the astronomer's studio is a singular illustration of the impossi-

bility of devising a well-ordered plan, when there are no proper materials for classification and distribution. For more than twice the period of the millennium, the most savage beasts and horrible giants have been sporting upon it with infants and gentle maidens, and have clustered the stars in the mingled confusion of the wilderness and the nursery. The additions which modern taste and sycophancy have made to this curiosity-shop have not diminished its peculiar interest, increased its classic elegance, or relieved the perplexed interweaving of the constellations.

But the exactness required in the development of form is that of unity, order, and continuity, more than that of number; and it is better expressed in the curve than in the formula. Such accuracy may be developed in its highest perfection, in minds to which the processes of arithmetical computation are utterly distasteful. There is one, whom I am proud to call my friend, to whom I have more than once tried to communicate some conception of algebraic analysis and its modes of research. Whether the fault was in the obscurity of the teacher, or the too great density of the pupil's brain, my excess of modesty dares not decide. Whatever was the cause, the attempt was a total failure; I could not bring my friend to comprehend the product of two by two, when both the twos were negative; and I am firmly convinced that he would rather have yielded his fine teeth to the dentist, than his radical and absurd repugnance to the extraction of an impossible root. But of all men who ever set foot upon American soil, there is not one who has made so many and so great scientific discoveries as this man; there is not one who has opened so many new treasures of knowledge; and, paradoxical as it may seem, he has unlocked every door with the key of geometry. How was it, for instance, that he drew the outline of the fossil fish, from that of the single scale? and how did it happen that the original lithograph, when it was discovered, was identical with his design? When he was challenged before the British Association to portray the form of fish proper

to a geological stratum in which he did not know that one was found, what power guided the hand which held the chalk? And when the cloth was removed, which, to his surprise, concealed the newly discovered fish, how and where came the extraordinary coincidence? and whence did they acquire the selfsame lineaments with the drawing upon the blackboard? To what other science than that of form is such a wonderful knowledge of form to be attributed? And are we not sorely tempted to confound all scientific distinctions, and claim him, even against his will, as one of the greatest of geometers, who has advanced the science far beyond the highest flight of the transcendental formula into the domain of the organic kingdom? When he was commissioned by the illustrious chief of the Coast Survey to examine the reefs of Florida, by what a consummate mathematical logic did he trace back their history for more than a thousand centuries, to the probable beginning of the present geological period!

Of the many difficult questions with which science is disturbed, none are so serious as those which are connected with religion. There are men, and pious men too, who seem honestly to think that science and religion are naturally opposed to each other; than which I cannot conceive a more monstrous absurdity. How can there be a more faithless species of infidelity, than to believe that the Deity has written his word upon the material universe and a contradiction of it in the Gospel? And is it possible that such a belief has ever been seriously professed? Or is the other alternative less unreasonable, that the serpent has wound its coils around the tree of knowledge, and that the alluring fruit which has been dropping at the foot of man from the days of Adam to those of Newton, is poisoned to its core? Shall we believe that the voices of Nature are the songs of the Siren and the artful temptations of the Devil, to divert man from his devotions? If this be so, how singularly forgetful has the Enemy been of his interests, or how divided against himself and his own



household, that he is thus praising God and magnifying him for ever, "in all the works of creation; in the sun and moon, and the stars of heaven; in the showers and dew; in the winds; the fire and heat; the winter and summer; the light and darkness; the lightnings and clouds; the mountains and hills; and all the green things upon the earth"! that he is "for ever blessing the Lord in the whales and all that move in the waters; in the fowls of the air; and in the beasts and cattle"! Can it be that this universal anthem, this all-resounding chorus of hallelujah, is a cunning device of hell to gain the souls of men? and that the arch-hypocrite stole from the fire upon the tongues of the holy children whom he dared not touch? and that he has ventured behind the altar into the inmost sanctuary of the church, in the garb of a high-priest, and inscribed the sublime canticle upon the book of prayer, with the pen which was dipped in the blood of Christ? Or is it to be supposed that unconscious matter has abjured its Creator, and, having no will, has fallen with the infinite sin of man's rebellion, melting in the fires of its volcano the impress of the seal of approbation which was imprinted on the day of creation? Such absurd hypotheses may, in this age, be left to their own refutation.

There is proof enough furnished by every science, but by none more than by geometry, that the world to which we have been allotted is peculiarly adapted to our minds, and admirably fitted to promote our intellectual progress. There can be no reasonable doubt that it was part of the Creator's plan. How easily might the whole order have been transposed! How readily might we have been assigned to some complicated system which our feeble and finite powers could not have unravelled! to some one, perchance, of the stars, in the immense cluster of Hereules, where "the countless and unending orbs" are "in mazy motion intermingled," and where,

"Above, below, around,  
The circling systems form  
A wilderness of harmony,"

the sound of which would never have penetrated to the desolate heart of man, nor have stirred his motionless spirit to a divine thought!

What a contrast to this humanly incomprehensible world is that in which we have our being, where the obvious simplicity and regularity of the daily phenomena invite our contemplation with so fine and irresistible a persuasion; where all the developments of art and nature occur in a happy and consistent gradation, ascending amid innumerable forms of beauty to the temple of science! Had it been the sole object of the physical universe to contribute to our intellectual nutriment, its arrangements could not have been more skilfully contrived. The angel of the Lord is ever sent with the message of new truth at the time when it is most welcome, when the heart of philosophy is heavy with the obscure mists which hang over the paths of knowledge, or when the faith of the fainting world requires the reviving influence of some brilliant discovery. How wonderful are the relations in the progress of the observing and the abstract sciences! With what a profound harmony do they sustain their several parts in the heaven-born symphony! Attracted by the symmetry of a few graceful curves, geometry undertook their investigation, and prosecuted the study of their curious and intricate properties, until ridicule threatened to point at such a useless expenditure of labor. But when, in the course of its revolving cycles, astronomy had grown to its grand era, it began by preaching to the astounded doctors of theology the amazing fact that these selfsame curves have been drawn, by the Creator's own hand, from the beginning of time, in the paths of the planet, the comet, and our own fixed earth; and thenceforth the treatise upon conic sections has become a chapter of the celestial Principia. And so it has ever been; in the ambitious flights to which geometry has been impelled by its impatience of the restraints of observation, it has never soared above the Almighty presence. And so it must ever be; the true thought of the created mind must have had its origin from the Crea-

tor; but with him thought is reality. It must then be that the loftiest conceptions of transcendental mathematics have been outwardly formed, in their complete expression and manifestation, in some region or other of the physical world; and that there must always be interwoven with the discoveries of observation these striking coincidences of human thought and nature's law. They are the reflections of the divine image of man's spirit from the clear surface of the eternal fountain of truth. Is then religion so false to God as to avert its face from science? Is the Church willing to declare a divorce of this holy marriage tie? Can she afford to renounce the external proofs of a God, having sympathy with man? Dare she excommunicate science, and answer at the judgment for the souls which are thus reluctantly compelled to infidelity? We reject the authority of the blind Scribes and Pharisees, who have hidden themselves from the light of heaven under such a darkness of bigotry. We claim our just rights and our share in the Church. The man of science is a man, and knows sin as much as other men, and equally with other men he needs the salvation of the Gospel. We acknowledge that the revelations of the physical world are addressed to the head, and do not minister to the wants of the heart; we acknowledge that science has no authority to interfere with the Scriptures and perplex the Holy Writ with forced and impossible constructions of language. This admission does not derogate from the dignity of science; and we claim that the sanctity of the Bible is equally undisturbed by the denial that it was endowed with authority over the truths of physical science. But we, nevertheless, as sons of men, claim our share in its messages of forgiveness, and will not be hindered of our inheritance by the unintelligible technicalities of sectarianism; as children, we kneel to the Church, and implore its sustenance, and entreat the constant aid and countenance of those great and good men who are its faithful servants and its surest support, whose presence and cheering sympathies are a perpetual benediction, and among whom shine the brightest lights of



science as well as of religion. Moreover, as scientific men, we need the Bible to strengthen and confirm our faith in a supreme intellectual Power, to assure us that we are not imposing our forms of thought upon a fortuitous combination of dislocated atoms, but that we may study His works humbly, hopefully, and trusting that the treasury is not yet exhausted, but that there is still left an infinite vein of spiritual ore to be worked by American intellect.

Gentlemen of the American Association, I cannot conclude without a few words to recall our duty to the country to which we owe our allegiance. We must despise the base servility to foreign superiority, which affects to look down from the heights of the cosmopolite upon the duty of patriotism, and scorn it as an abomination. We must love our country with the same devoted, noble, and generous love which inspired the lives of Niebuhr and Arago, of Bowditch and our late lamented Walker, which is the living fountain of the labors of men like Humboldt, Henry, and Bache, which won for Franklin the affection, admiration, and reverence of France, and without which there can be no worthy respect and esteem for the labors of the men of other nations. The heart which is too small to hold its own country cannot assume to embrace the whole world, and still less to contain a science. Of all the virtues, patriotism is the least selfish, and that which is most kindred to the grand sentiments of the heroic soul. It repels foreign arrogance with dignified contempt, its proud spirit rejoices to do homage to true greatness wherever it may be found, and it frankly opens its hospitable door to the reception of the learned guest of every land. It was the larger half of the greatest name in history, and from the tomb of Washington it invokes us to be faithful to posterity.

The time is ripe for some important improvement in the public condition of science and its relations to government. For the first time in the history of the republic, "the men of genius of our country, who, by their inventions and discoveries in science and art, have contributed largely to the improve-

ments of the age, without in many instances securing for themselves anything like an adequate reward," have been commended to the favorable consideration of Congress by our Chief Magistrate. For this great and good word, let the benedictions of science rest upon his head. It is now our fault if the occasion be not properly improved. No government, in proportion to its opportunities, has surpassed our own in its readiness to promote science. The Coast Survey, the National Observatory, the Nautical Almanac, the military and naval academies, the expeditions for astronomical and other scientific purposes, and the munificent grants for special researches, are conclusive evidence of the willingness to advance high and useful forms of philosophical inquiry. The confidence which has been inspired in other countries is shown in the trust of that magnificent endowment for the diffusion of knowledge to all mankind, under whose roof we are here assembled. The provisions for the election of the Board of Regents and the choice of a Secretary, upon the singleness of whose integrity and the largeness of whose comprehension are concentrated the hope and confidence of his scientific brethren throughout the world, are a guaranty that the honor of the republic will be held sacred in the discharge of this high charity, and that it will not be diverted into *any local channel* from the enlarged intentions of its testator. Let us profit by the example of Smithson, and, instructed by the wisdom of this high-minded son of England, learn to confide in our own rulers. Let us be aroused to an earnest and harmonious effort to accomplish the plan proposed by our President at Albany, for the building up of an "*institution for science, supplementary to existing institutions, to guide public action in reference to scientific matters.*" With the details of the plan and the arguments in its favor you are familiar. You know how useful it would be as a protection from the wasteful expenditure upon abortive attempts to reverse the laws of nature. You know how much it is required to sustain the purity and independence of science, even within its

own proper domain. You know that in no age or country was there ever a more urgent call for a scientific society, in which scientific influence should predominate, where it should not be smothered by excess of patronage, and whence it should not be liable to banishment through any spirit or form of ostracism. If American genius is not fettered by the chains of necessity, and helplessly exposed to the assaults of envious mediocrity, but is generously nourished in the bosom of liberty, it will joyfully expand its free wings, and soar with the eagle to the conquest of the skies.











ASTRONOMICAL TABLES

ADAPTED TO THE

THEORY OF ASTRONOMY,

AS TAUGHT IN

NEW ENGLAND COLLEGES.

BY

JAMES H. COFFIN, A. M.

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## P R E F A C E.

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IN preparing text-books for American colleges it should not be overlooked that the design of college education is not so much to make adept practitioners in any particular science, as to give broad and comprehensive views of the whole field. Hence the principles of the several sciences should be thoroughly understood by the student ; but the application of them to practice *by mere rules* is foreign to the design of a collegiate course of study. On the question whether practical astronomy should be attended to at all in college it is not intended here to offer an opinion ; but if studied it should be in such a manner that its connection with the theory may be apparent, and that the two may mutually illustrate each other. Now how is the fact in respect to the treatises on astronomy used in our colleges ? Those in most general use give the theory merely, without attempting any practical application. Of this there is no reason to complain, for it is far better in its influence on the mind to leave it thus, than to give a mere mechanical application, as is done in all the treatises for colleges, so far as I know, in which the student is taught to make astronomical calculations. The tables in some of these works are very minute and accurate, and at the same time so constructed and arranged as to reduce the labor of calculation as much as possible ; but the student can see no connection between them and the motions and perturbations which occupy his attention in the study of the theory. In fact, one who has studied the theory with ever so much thoroughness, has here very little ad-



vantage over one who is entirely ignorant of it; each being guided wholly by rules which must appear entirely arbitrary.

The tables here offered to the public, are based, for the most part, on those of Delambre and Burg, with the more recent improvements by Professors Airy and Bessel; but the plan of construction has been varied so as to adapt them to the theory as taught in the treatises of Olmsted or Herschel. It is believed that any student, who has well studied one of those treatises, will readily understand the application of these tables, and will see that they only carry out to practical results the theory there taught.

The quantities in the tables are given, for the most part, in degrees and decimals, instead of signs, degrees, minutes and seconds, with a view to facilitate the labor of calculation, and to secure the same degree of accuracy with a less number of figures.

The introductory remarks and directions in this pamphlet are intended but for temporary use, the tables being designed for a larger treatise, not yet ready for the press, in which all the subjects connected with the calculation of eclipses, which should be thought interesting or profitable to the student will be introduced, and by the aid of necessary diagrams and plates, more fully explained than the limits of a small pamphlet allow.

Williams College, May 4, 1842.

## USE OF THE TABLES IN THE CALCULATION OF ECLIPSES.

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FROM the nature of eclipses, both solar and lunar, it is evident that they can occur only when the sun is near one of the moon's nodes, and consequently only in opposite seasons of the year. For example, if eclipses occur in January of any given year, others may be expected in July. But the particular month in which they can occur, depends on the longitude of the moon's node, and must therefore vary, occurring earlier every year than they did in the year preceding, since the nodes move westward about  $19^{\circ}$  each year.

The sun in its apparent annual course, leaves the Vernal Equinox, where its longitude is  $0^{\circ}$ , about the 21st of March, and moves eastward toward the moon's nodes about  $1^{\circ}$  each day. Consequently, it must arrive at either node in about as many days after the 21st of March, in any given year, as the longitude of the node on that year contains degrees. For example, the longitude of the ascending node in the year 1854, is  $64^{\circ}$ ; (see Table 2d, right hand column;) therefore eclipses may be expected about sixty four days after the 21st of March, which carries the time into May. The months therefore in 1854 when we may expect eclipses, are May and November.

The moon can eclipse the sun only when new, and can be itself eclipsed at no other time than at the full. It is not therefore on the precise day, found by the above process, that eclipses must necessarily happen; but on the next new or full moon before or after; more frequently the former.

The velocity of the motions of the sun and moon in their respective orbits, is variable; but it is more convenient in the calculations to regard it as uniform, and to make the necessary corrections afterward. Table 2d, gives the time of new moon in March of each year, and the longitude of the sun, moon, and moon's ascending node, all calculated on this supposition. It

also gives the mean anomalies of the sun and moon, i. e. the distance of each from its perigee. Table 3d, gives the motions, still supposed uniform, during any number of lunations from one to thirteen.

#### TO CALCULATE THE TIME OF AN ECLIPSE.

To explain the method of calculating the time of an eclipse, I will take as an example a solar eclipse that is to be expected in May, 1854.

Having found, in the manner described above, that eclipses may be expected in that month, I take from Table 2d, the time of mean new moon in March, 1854, together with the mean longitudes and anomalies of the sun and moon, and the longitude of the moon's ascending node, and write them out in the manner shown in the Example. I next enter Table 3d, and take out as many lunations, as when added to the time of new moon in March, will bring the time near to that at which the eclipse is to be expected, and write them down with the longitudes and anomalies under the corresponding quantities already taken from Table 2d. These I add together, (with the exception of the right hand column, where I subtract the lower number from the upper, because the motion of the node is retrograde,) and thus obtain the time of mean new moon in May with the mean longitudes and anomalies. Table 5th, shows me the month and day to which the number of days found by the foregoing addition corresponds.

At this stage of the calculation it is well to compare the longitude of the sun or moon with that of the ascending node, (or with that of the descending node, found by adding  $180^\circ$  to the longitude of the ascending node,) and if they do not differ more than  $20^\circ$  there *may* be an eclipse; though there will not *probably* be one if the difference is over  $16\frac{1}{2}^\circ$ . If the difference be too great, it shows that too many lunations were added or too few, and a correction must be made accordingly.

At the time of new moon the longitudes of the sun and moon must be equal; and according to our calculations they are so at the time of mean new moon in May, just found: But this is on the supposition that their motions were uniform. To find



whether or not their longitudes are truly equal, I next proceed to compute them, taking into account all the chief inequalities in their motions; and if they come out alike the time of new moon is correctly found, otherwise, I add or subtract such an amount of time, as with the relative velocities of the sun and moon at the time will render them equal.

The progressive motion of the moon's perigee, and the retrograde motion of its nodes, being both caused by the sun's attraction, are most rapid when the sun is in its perigee, and constantly grow slower and slower till the sun reaches its apogee, where the motion becomes the slowest. Consequently, as the sun leaves its perigee, the moon's perigee immediately gets before, and its nodes behind their mean place, and continue so till the sun reaches its apogee, when owing to the diminished rate of motion, their mean and true places again coincide. The contrary takes place when the sun is in the other half of its orbit. Hence it is apparent that the moon's anomaly, being reckoned from its perigee, must be less than the mean when the sun's anomaly is less than  $180^\circ$ , and greater when greater; showing that something must be subtracted from the moon's anomaly in the former case and added in the latter. The same must also be true of the longitude of the moon's nodes. These facts are indicated in Tables 6th and 7th, by the signs — and + placed at the head of the column containing the argument.

Entering Tables 6th and 7th, with the sun's anomaly as an argument, I take out the corrections which the foregoing considerations show to be necessary, and which are denominated Annual Equations of the Moon's Perigee and Node, (taking care to make a proper allowance for the odd degree and decimals of the anomaly, as the tables give the equation only for every two degrees,) and apply the former to the moon's anomaly, and the latter to the longitude of the node, according to the sign + or — at the head of the column of the argument. Observe in these, and most of the other tables, that the unit figure of the argument is placed at the top or bottom of the table, and the other figures at the right or left. When the latter is found at the left we must look for the former at the top, but when the latter is at the right, the former must be sought for at the bot-

tom. Opposite the latter, and in the same column with the former, the equation is found. Thus in the Example, the sun's anomaly being  $143^{\circ}.797$ , I look for the number 14 in the left hand column, and for 3 at the top. But since the latter number is not found, the equation being given in the table only for  $142^{\circ}$  and  $144^{\circ}$ , I note the difference between these equations, and take a proper proportion of it for the excess of the argument over  $142^{\circ}$ , viz.  $1^{\circ}.797$ . By this process I find the Annual Equation of the Perigee to be  $-0^{\circ}.221$ ; and of the Node  $-0^{\circ}.0875$ .

By the laws of elliptical motion, the sun and moon move quickest in perigee and slowest in apogee. Therefore by the same reasoning as in the preceding case, they must be in advance of their mean place when passing from perigee to apogee, i. e. when their anomaly is less than  $180^{\circ}$ , and behind it in the other half of their orbits; so that something must be added in the former case and subtracted in the latter. In the present example the sun's mean anomaly is  $143^{\circ}.797$ , and the moon's, as now corrected,  $145^{\circ}.078$ , showing that both are passing from perigee to apogee. I therefore enter Tables 8th and 9th, with these anomalies respectively as arguments, and take out the Equation of the Centre of the sun and of the moon, applying the former to the sun's longitude and the latter to the moon's, each with the sign  $+$  as shown at the head of the column of the argument.

The attraction of the sun upon the moon tends to draw it away from the earth, and thus dilate its orbit; and since the distance of the sun from the earth varies in different seasons of the year, its attraction also varies. This causes the moon's periodic time to be greater when the sun is in perigee than when it is in apogee. Hence a subtractive equation must be applied to the moon's longitude when the sun is passing from perigee to apogee, and an additive one when passing from apogee to perigee. I therefore enter Table 10th, with the sun's anomaly as an argument, and find the equation to be  $-.1118$ , which I apply both to the moon's longitude and anomaly,\* for it obviously affects

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\* The anomaly contains but three decimal places; hence in applying the corrections to it, the 3d figure is given according to its nearest value.

both alike. The same is true of most of the other corrections that remain to be applied.

The remarks appended to Table 11th, sufficiently explain it. With the year 1854 for an argument, I enter that table and find the equation to be .0009, which must evidently be additive.

From the theory of the perturbation of the moon's motion, known in astronomy by the name of Variation, it is evident that the equation must be additive when the moon is passing from syzygy to quadrature, and subtractive when it is passing from quadrature to syzygy; also that the quadrants (so termed) nearest the sun must each contain a little less than  $90^\circ$ , and the other two quadrants each a little more than  $90^\circ$ . Now the moon is passing from syzygy to quadrature, when the excess of its longitude over the sun's is from  $0^\circ$  to  $90^\circ$ , or from  $180^\circ$  to  $270^\circ$ ; but when this excess is from  $90^\circ$  to  $180^\circ$ , or from  $270^\circ$  to  $360^\circ$ , the moon is passing from quadrature to syzygy. I therefore subtract the sun's longitude from the moon's as thus far corrected, and with the remainder as an argument enter Table 12th, take out the equation and apply it according to its sign, if the argument is less than  $180^\circ$ ; but change the sign if the argument is over  $180^\circ$ . In the present case the argument is  $2^\circ.1880$ , which gives for the equation  $+.0445$ .

Variation being occasioned by the sun's disturbing influence, must be greater or less according as the sun is nearer to the earth or more remote from it; or in other words it must be subject to an annual equation. The method of taking the Annual Equation of Variation from Table 13th, is more difficult than the preceding. Two arguments are employed; viz. 1st, the argument just used for Variation, which is to be sought for at the top or bottom of the table, and 2d, the sun's anomaly at the right or left. If the former is found at the top, we look for the latter at the left; but if at the bottom, at the right. The equation is found opposite the latter, and in the same column with the former. Since one argument is given only for every  $5^\circ$  and the other for  $10^\circ$ , it is necessary to institute a kind of double proportion for the units and decimals. It is further to be noticed that if both arguments are found in the same gnomon, enclosed by the heavy lines about the table, the equation is to be applied



with its proper sign as found in the table ; but if one is found in the inner and the other in the outer gnomon, the sign before the equation is to be changed from + to -, or from - to +. In the present case the former argument is  $2^{\circ}.1880$ , which being between  $0^{\circ}$  and  $5^{\circ}$ , is to be considered as found in the inner gnomon at the top; and the latter is  $143^{\circ}.797$ , which is found in the outer gnomon at the left. Making a proper allowance for the units and decimals, the equation is  $+.0053$ ; but the arguments being found, one in the *inner* and the other in the *outer* gnomon, the sign must be changed, and the equation becomes  $-.0053$ . This I apply to the moon's longitude and anomaly.

The general theory of the equation denominated Evection, is stated at the foot of Table 14th; but to explain it fully, so as to show the propriety of the argument, would require diagrams and more space than can here be allotted to it. To obtain the argument I subtract the sun's corrected longitude from the moon's mean longitude, borrowing  $360^{\circ}$  if necessary, multiply the remainder by 2, and subtract the product from the moon's mean anomaly. Thus in the example

The moon's mean anomaly is,	-	-	-	145 <sup>o</sup> .0780
The moon's mean longitude is,			64 <sup>o</sup> .2329	
Subtract sun's corrected longitude,			65 .3494	
				<hr/>
				358 .8835 $\times 2 = 357 .7670$
				<hr/>
Argument of Evection,	-	-	-	147 .3110

Entering Table 14th, with this argument, I find the equation to be  $-.7156$ , which I apply as in the example.

Evection like Variation is subject to an annual equation, and for the same reason. I therefore enter Table 15th, with the argument of Evection at the top or bottom, and with the sun's anomaly at the side, and take out and apply the Annual Equation of Evection in the same manner as was directed for Table 13th.

The influence of the sun tending to dilate the moon's orbit must be greatest when the former is in the plane of the latter, i. e. when the sun is passing the nodes, and least when it is farthest from them. Hence the moon's periodic time must exceed the mean in the first case and fall short of it in the last;

and consequently the moon must be behind its mean place, while the sun is passing through the first quadrant after leaving either node, and in advance when the sun is in the other two quadrants; so that something must be subtracted from the moon's longitude when the sun is in the quadrants first named, and added when it is in the last. To find in which quadrant the sun is, I subtract the longitude of the node from the sun's longitude; and then with the remainder as an argument I enter Table 16th, and take out the equation. In the example the sun's longitude is  $65^{\circ}.3494$ , and that of the ascending node  $61^{\circ}.0008$ . This gives for the argument  $4^{\circ}.3486$ , and for the equation  $-.0026$ .

The moon's anomaly is now altered considerably, by reason of the various equations that have been applied, from what it was when I used it to take out the Equation of the Centre from Table 9th; and since this equation is a very important one, I now take it out again, and by whatever amount it differs from what it was as first taken out, I correct the moon's longitude. Thus in the example, the anomaly as used was  $145^{\circ}.078$ , which gave as an equation  $3^{\circ}.4154$ ; while now it is but  $144^{\circ}.298$ , which gives for the equation  $3^{\circ}.4834$ ; so that I did not add enough for the Equation of the Centre by  $0.680$ . I therefore now add this quantity to the moon's longitude.

The inclination of the plane of the moon's orbit to that of the ecliptic, causes longitudes reckoned upon it to be different from what they would be if reckoned on the ecliptic. I therefore enter Table 17th, with the moon's distance from the node as an argument, and take out an equation for reducing to the ecliptic the moon's longitude, which has thus far been reckoned on its own orbit. The equation I find to be  $-.0233$ , which applied to the moon's longitude makes it  $66^{\circ}.9131$ .

I have now obtained the true longitudes, both of the sun and moon, reckoned from the mean equinox, and find that the latter is greatest by  $1^{\circ}.5637$ , which shows that the moon has passed by the sun, and that the eclipse is over. I must now subtract such an amount from the time of new moon, as it must have taken the moon to gain this difference, and in order to do so I must know the relative velocities of the sun and moon in their orbits at the time.

Their motions are swiftest in perigee and grow slower as they recede from it; hence their anomalies are the proper arguments for determining their motions. I therefore enter Tables 20th and 21st, with the anomalies of the sun and moon respectively as arguments, and take out their hourly motions. The former I find to be .0401 and the latter .5018.

The other inequalities which I have mentioned, must likewise affect the moon's hourly motion, of which Variation and Evection are the most important. The effect of Variation, as is plain from the theory, is to increase the moon's velocity in syzygy, and diminish it in quadrature. Now in an eclipse the moon is always in syzygy, hence I add to its hourly motion the quantity mentioned in the margin of Table 21st. To correct the moon's hourly motion for Evection, I enter Table 22d, with the same argument that I used for that inequality in Table 14th, and in the middle column I find the equation, which I apply to the hourly motion according to its sign.

The following is the operation in the case before us.

Moon's hourly motion by Table 21st,	-	.5018
Add for Variation,	- - - - -	.0115
		<u>.5133</u>
Subtract for Evection by Table 22d,	- -	.0092
		<u>.5041</u>
Subtract sun's hourly motion,	- - -	.0401
		<u>.4640</u>
Hourly gain of the moon upon the sun,	-	.4640

Now by a simple proportion, I find how long it must have taken the moon to gain the difference in the longitudes of the sun and moon, viz.  $1^{\circ}.5637$ . Thus

.4640 : 1 hour ::  $1^{\circ}.5637$  : the time required, which is thus found to be 3 hours, 22 minutes, and 12 seconds. This subtracted from the time of mean new moon, as shown in the Example, leaves for the true time of new moon, May, 26d. 8h. 48m. 44sec.\*

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\* It is apparent that so great an alteration as I have just made in the time, must affect the arguments of all the equations I have introduced, so that my work may be inaccurate to the amount of two or three minutes. It would hence seem desirable to have obtained, if possible, some nearer approximation to the true time of new



The time thus found is Greenwich time, and to reduce it to that of any other place allowance must be made for the difference of longitude.

EXAMPLE.—*Showing the method of calculating the time of a solar eclipse.*

	Time.	Sun's Anom- aly.	Sun's Longi- tude.	Moon's Anom- aly.	Moon's Longi- tude.	Longi- tude of Node.
Mean new moon in March, 1854, . . . . .	d. h. m. s. 28 10 42 50	0 586	0 0194	0 93.665	0 6.0194	0 2153
Add two lunations, . . . . .	59 1 23 6	58.211	58 2135	51.634	58 2135	-3.1275
Mean new moon in May, . . . . .	26 12 10 56	143.797	64.2329	145.299	64.2329	61 0883
Ann'l equation moon's perigee and node,				- .221		-.0875
				145.078		61.0008
Equation of the centre, . . . . .			+1.1165		+3.4154	
			65.3494	145 078	67.6483	
Annual equation of moon's longitude,				- .112	- .1118	
				144.966	67.5365	
Secular equation of moon's longitude,				+ .001	+ .0009	
				144.967	67.5374	
Variation, . . . . .				+ .045	+ .0445	
				145.012	67.5819	
Annual equation of variation, . . . . .				- .005	- .0053	
				145.007	67.5766	
Evection, . . . . .				- .716	- .7156	
				144.291	66.8610	
Annual equation of evection, . . . . .				+ .010	+ .0100	
				144.301	66.8710	
Nodal equation of the moon's longitude,				- .003	- .0026	
				144.298	66.8684	
Correction of the equation of the centre,					+ .0680	
					66.9364	
Reduction to the ecliptic, . . . . .					- .0233	
					66.9131	
Subtract for diff. in lon. of sun and moon,	3 22 12	-.135	-.1351	-1 699	-1.6988	+ .0074
True time, and long. from mean equinox,	26 8 48 44	143.662	65.2143	142.599	65.2143	61 0032
Lunar nutation, . . . . .			-.0012		- .0042	-.0042
True longitudes, . . . . .			65.2101		65.2101	61.0040

The time of a lunar eclipse is calculated precisely in the same manner as that of the sun, only that the half lunation in Table 3d, is used in order to give the time of mean *full* moon, and the longitudes of the sun and moon, instead of being made to agree, are made to differ just  $180^\circ$ .

moon at the outset, which might have been done by applying to the time of mean new moon the 'preliminary equations' found in Tables 27th and 28th, and then taking from Table 4th the mean motions in longitude and anomaly during the time so applied. These motions applied to the mean longitudes and anomalies at the time of mean new moon, would have given the mean longitudes and anomalies at the corrected time, and I might then have proceeded to calculate the longitudes of the sun and moon at the corrected time of new moon, in the same manner that I have done for mean time. Had I done so, the longitudes would have come out so nearly alike, that the alteration in the time would not have amounted to more than a few minutes.

Since, however, my object is to give a clear view of the *theory* of the calculations, rather than to ensure the greatest possible accuracy, I preferred the course I have adopted.

## TO CALCULATE THE ELEMENTS OF AN ECLIPSE.

The following elements or data, are all that are needed for making any calculation that we may desire in regard to an eclipse, either solar or lunar; such as the place on the earth's surface where the sun will be centrally eclipsed at any given time, while the eclipse lasts; the portions of the earth where an eclipse will be visible, and the time when it will commence, become a maximum and terminate; or the size of the eclipse at any given place and time. The 12th is not needed in solar eclipses, nor the 2d, 3d, and 6th, in lunar.

1st. The time of new or full moon.

2d. The longitudes of the sun and moon.

3d. The obliquity of the ecliptic to the equator.

4th. The moon's latitude.

5th. The angle of the moon's visible path with the ecliptic.

6th. The sun's declination.

7th. The sun's hourly motion.

8th. The moon's do.

9th. The sun's apparent semi-diameter as seen from the earth.

10th. The moon's do.

11th. The apparent semi-diameter of the earth as seen from the moon, which is the same as the moon's horizontal parallax.

12th. The apparent semi-diameter of the earth's shadow where it eclipses the moon, as seen from the earth.

The method of obtaining the first has already been explained.

To obtain the 2d, I compute the motions of the sun and moon, during the correction applied to the time, (in the Example, 3 hours, 22 minutes, and 12 seconds,) which I can easily do after having found their hourly motions, as already explained, and add or subtract them from the previous longitudes and anomalies, according as the correction in the time was additive or subtractive. At the same time I correct the longitude of the node, by taking from Table 4th its motion, during the correction in the time, and applying it with the contrary sign from that of the other motions, because it moves in the opposite direction. All the longitudes must also be corrected for Nutation taken from Table 19. The operation is shown in the Example.

The 3d element is obtained from Table 23d, and needs no explanation, other than that given with the table.

To obtain the 4th element, I subtract the longitude of the node from that of the moon, and with the remainder enter Table 24th. It is plain that the moon's latitude must be north for the first  $180^\circ$  after it leaves the ascending node; and that it moves northerly, or *ascends* through the first quadrant, and southerly, or *descends* through the second. Also the reverse must be true in the other half of the orbit. These facts are indicated in the table by the capital letters at the head of the columns containing the argument.

Table 25th gives the fifth element. It is entered with two arguments; 1st, the difference between the hourly motions of the sun and moon already obtained, at the top; and 2d, the difference in the longitudes of the moon and its node, just found, at the right or left.

The sun's declination, which is the 6th element, evidently depends on its longitude, being north for the first  $180^\circ$  after it leaves the vernal equinox, and south through the rest of its course. It is obtained from Table 26th.

The 7th and 8th elements have already been obtained; but if much accuracy were required, they would have to be computed over again, because the anomalies have been changed.

The 9th element is obtained from the 1st column of Table 26th, where it is sufficiently explained.

Table 21st, columns 1st and 3d, give the 10th and 11th elements, so far as they depend on the elliptical form of the moon's orbit. But the effect of Variation is to throw the orbit into a kind of oval, with its shortest diameter lying in syzygy. From this cause, the distance of the moon from the earth is less when it is new or full than at other times, which must increase their apparent size as viewed from each other. Hence the corrections in the margin of Table 21st. Also Evection, by altering the shape of the moon's orbit, affects its apparent size and parallax, so that a farther correction becomes necessary from Table 22d. The following shows the method of obtaining these elements, for the eclipse of May, 1854.



	Semi-diameter.	Parallax.
Values taken from Table 21st, - - - -	.2482	.9097
Variation, - - - -	+ .0020	+ .0073
	.2502	.9170
Evection, - - - -	- .0023	- .0083
True semi-diameter and parallax, - - - -	.2479	.9087

The apparent semi-diameter of the earth's shadow, which is the 12th element, is always equal to the sum of the parallaxes of the sun and moon, diminished by the sun's apparent semi-diameter. The sun's parallax is always .0024, and the method of obtaining the other data for finding this element, has been already explained.

#### CALCULATION OF THE CENTRAL TRACK OF A SOLAR ECLIPSE.

In a solar eclipse there is a shadow of the moon projected upon the earth, and we may wish to know at what place upon the earth's surface the centre of the shadow will fall at any given time, for there the sun must be centrally eclipsed. Some slight calculations, aided by a terrestrial globe, are all that is required for this purpose.

We first find the time when the centre of the shadow first strikes\* the earth, and the moon's latitude at the time: also the moon's latitude and relative longitude† at the time when we wish to calculate the position of the centre of the shadow. (See Olmsted's Astronomy, Art. 260 and 262.) We then convert the latitude in both cases into degrees of the earth's circumference, measured from the terrestrial ecliptic upon a secondary to it, and call the first *m* and the second *m'*. The relative longitude we convert into degrees measured on the parallel to the terrestrial ecliptic which passes through the centre of the shadow, and reckoned from that secondary to the terrestrial ecliptic whose plane produced would pass through the sun.

Now take a terrestrial globe, elevate the north pole about  $66\frac{1}{2}^{\circ}$ , so that the wooden horizon may represent the ecliptic, and screw the graduated quadrant of altitude to the zenith. Find the sun's place on the wooden horizon and count from it to the left  $90^{\circ}$ ,

\* It is immaterial whether we take the time when the centre of the shadow strikes or leaves the earth, provided we make the other parts of the process to correspond.

† Difference in longitudes of sun and moon.

where place the foot of the quadrant of altitude. The quadrant will then represent the western edge of the earth's illumined hemisphere. Find where at the equator it will be just 6 o'clock in the morning, at the time the centre of the shadow first strikes the earth, and turn the globe on its axis till this point comes under the graduated quadrant. Count upward on the quadrant the number of degrees contained in  $m$ , and it will show the point on the earth's surface where the centre of the shadow first strikes. Now turn the globe forward on its axis the same number of degrees that the earth would turn in the interval of time between that at which the centre of the shadow first strikes the earth and that for which you wish to calculate; count from the sun's place on the wooden horizon, a number of degrees equal to  $n'$ , toward the left if before new moon, and toward the right if after, and there place the foot of the quadrant of altitude. Count upward on it a number of degrees equal to  $m'$ , and you have the place required.

In these directions the moon's latitude is supposed to be north, but the process will be just the same if it is south, only we must screw the quadrant of altitude to the nadir and count the degrees downward from the wooden horizon.

If greater accuracy be required, calculations may be substituted for the mechanical part of the process. The theory of the following method is the same as that just described.

First find, as before, the time when the centre of the shadow first strikes or leaves the earth, (I will here adopt the latter.) Find the sun's longitude at the time, and call it  $s'$ . Find the longitude of the sun, and the latitude and relative longitude of the moon, at the time to which the calculations refer, and call them respectively  $s$ ,  $l$  and  $d$ . Convert the time last mentioned into degrees, minutes and seconds, reckoning  $15^\circ$  for an hour; subtract therefrom  $90^\circ$ , (borrowing  $360^\circ$  if necessary,) and call the remainder  $t$ .

Let the moon's equatorial parallax, which may be regarded as constant during the eclipse,  $= p$ .

Let the obliquity of the ecliptic to the equator  $= m$ .

Let  $P$ ,  $P^1$ ,  $P^2$ , &c. = sundry arcs and angles obtained during the process of the calculations.

Let the required latitude of the centre of the shadow =  $x$ .

Let the required longitude reckoned westerly from the meridian of Greenwich, or of that place for which the time is given, =  $y$ .

$$\text{Then } \frac{l}{p} = \sin. P,$$

$$\frac{d}{p \times \cos. P} = \sin. P^1,$$

$$*s \pm P^1 = P^2,$$

$$\sin. m \times \cos. P^2 = P^3,$$

$$\tan. m \times \sin. P^2 = \tan. P^4,$$

$$\dagger \sin. (P \pm P^4) \times \cos. P^3 = \sin. x = \text{the latitude},$$

$$\frac{\cot. s'}{\cos. m} = \tan. P^5,$$

$$\frac{\tan. P^2}{\cos. m} = P^6,$$

$$\frac{\sin. P^3}{\cos. x} = P^7,$$

$$\sin. x \times \tan. P^7 = P^8,$$

$$\dagger P^5 - P^6 + \pm P^8 = y = \text{the longitude}.$$

The chief difficulty in applying these equations consists in knowing which of the four possible values to give to  $P, P^1, P^2$ , &c. The following statements will remove all doubt.

$P, P^1, P^3$  and  $P^4$  are each always less than  $90^\circ$ .

$P^5$  is always of the same affection as  $s'$  increased by  $90^\circ$ .

$P^6$  is always of the same affection as  $P^2$ .

$P^7$  is less or greater than  $90^\circ$ , according as  $P$  is less or greater than the complement of  $P^4$ ;—it never exceeds  $180^\circ$ .

$P^8$  is always of the same affection as  $P^7$ .

To apply the process to a particular case, let it be required to find the place where the solar eclipse which we have taken as an example will be central at 20 minutes and 51 seconds past 10, by

\* If after new moon +; if before it - .

† The sign before  $P^4$  is + if  $P^2$  is less than  $180^\circ$ ; but - if it is greater. And in the latter case if  $P^4$  is greater than  $P$ , the latitude of the place will be opposite in character to that of the moon; i. e. if the moon's latitude is north that of the place will be south, and the contrary.

‡ The sign before  $P^8$  is + if  $P^2$  is between  $0^\circ$  and  $90^\circ$ , or between  $270^\circ$  and  $360^\circ$ ; but - if  $P^2$  is between  $90^\circ$  and  $270^\circ$ .



Greenwich time. After the preparatory steps, we have the following data and results.

DATA.	RESULTS.
Time=10 <i>h.</i> 20 <i>m.</i> 51 <i>sec.</i>	$P = 28^{\circ} 58' 22''$
$s' = 65^{\circ} 16'$	$P^1 = 66 \quad 29 \quad 27$
$s = 65 \quad 16$	$P^2 = 131 \quad 46 \quad 1$
$l = 0.4394$	$P^3 = 15 \quad 22 \quad 46$
$d = 0.7278$	$P^4 = 17 \quad 56 \quad 12$
$t = 65^{\circ} 12' 45''$	$x = 44 \quad 45 \quad 28 = \text{the latitude.}$
$p = 0.9072$	$P^5 = 153 \quad 21 \quad 5$
$m = 23^{\circ} 27' 34''.5$	$P^6 = 129 \quad 19 \quad 34$
	$P^7 = 21 \quad 55 \quad 45$
	$P^8 = 15 \quad 49 \quad 38$
	$y = 73 \quad 24 \quad 38 = \text{the longitude.}$

Thus we find that the centre of the shadow at the time just mentioned is in lat.  $44^{\circ} 45' 28''$ , and lon.  $73^{\circ} 24' 38''$ , which is on the west shore of Lake Champlain, about four miles north of the village of Plattsburg.

It is difficult, without the aid of diagrams, to explain the method of finding how an eclipse of the sun will appear at any given place and time; and it is thought best to omit it till this work shall be published in a more enlarged form.

The method of calculating the size and appearance of lunar eclipses, is sufficiently explained in Olmsted's Astronomy, Articles 258-60.

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27. First Preliminary Equation.
28. Second Preliminary Equation.
29. Augmentation of the Moon's Semidiameter.
30. To convert minutes into decimals of a degree.
31. To convert seconds into decimals of a degree.

TABLE I.

*Elements of Orbits of Sun and Moon.*

	Sun.	Moon.
Mean longitude, Jan. 1, 1801, - - - -	280° 39' 13".17	118° 17' 8".3
Motion in 100 years or 36525 days, - - - -	36000 46 0.77	481267 52 41.6
Mean longitude of perigee, Jan. 1, 1801, - - - -	279 31 9.71	266 10 7.5
Motion of do. eastward in 100 years, - - - -	1 42 56.0	4069 2 46.6
Longitude of Moon's Node, Jan. 1, 1801, - - - -		13 53 17.7
Motion of do. westward, in 100 years, - - - -		1934 9 57.5

## TABLE II.

*Mean New Moon, &c. in March.*

Year	Mean New Moon in March.				Sun's Mean Anomaly.	Moon's Mean Anomaly.	Sun and Moon's Mean Longitude.	Longitude of Node.
	d.	h.	m.	sec.				
1800	25	0	18	49	83.208	127.960	2.7139	28.8207
1810	5	6	36	43	63.167	63.441	342.8436	196.4758
1820	14	1	38	40	72.232	24.740	352.0799	2.5671
1830	23	20	40	37	81.295	346.038	1.3163	168.6585
1840	3	2	58	31	61.254	281.530	341.4458	336.3135
1841	22	0	31	8	79.624	257.140	359.8335	315.9844
1842	11	9	19	42	68.888	206.943	349.1144	297.2190
1843	0	18	8	17	58.152	156.746	338.3954	278.4536
1844	18	15	40	54	76.522	132.366	356.7831	258.1245
1845	8	0	29	28	65.786	82.169	346.0640	233.3591
1846	26	22	2	6	84.156	57.789	4.4517	219.0300
1847	16	6	50	40	73.420	7.592	353.7326	200.2646
1848	4	15	39	15	62.684	317.335	343.0135	181.4991
1849	23	13	11	52	81.054	293.015	1.4012	161.1702
1850	12	22	0	27	70.318	242.819	350.6822	142.4048
1851	2	6	49	1	59.583	192.622	339.9631	123.6394
1852	20	4	21	38	77.952	168.242	358.3508	103.3103
1853	9	13	10	13	67.217	118.045	347.6317	84.5449
1854	28	10	42	50	85.586	93.665	6.0194	64.2158
1855	17	19	31	24	74.851	43.468	355.3003	45.4504
1856	7	4	19	59	64.115	353.271	344.5813	26.6851
1857	25	1	52	36	82.485	328.891	2.9689	6.3560
1858	14	10	41	11	71.749	278.694	352.2499	347.5906
1859	3	19	29	46	61.013	228.497	341.5338	328.8252
1860	21	17	2	24	79.383	204.117	359.9185	308.4961
1861	11	1	50	58	68.647	153.920	349.1994	289.7307
1862	0	10	39	33	57.911	103.723	338.4803	270.9653
1863	19	8	12	10	76.281	79.343	356.8680	250.6362
1864	7	17	0	45	65.545	29.146	346.1490	231.8708
1865	26	14	33	22	83.915	4.766	4.5366	211.5417
1866	15	23	21	57	73.179	314.569	353.8176	192.7763
1867	5	8	10	31	62.443	264.372	343.0985	174.0110
1868	23	5	43	9	80.813	239.992	1.4862	153.6819
1869	12	14	31	43	70.077	189.795	350.7671	134.9165
1870	1	23	20	18	59.341	139.599	340.0481	116.1511
1871	20	20	52	55	77.711	115.219	358.4357	95.8220
1872	9	5	41	29	66.976	65.022	347.7167	77.0567
1873	28	3	14	6	85.346	40.642	6.1043	56.7275
1874	17	12	2	41	74.610	350.445	355.3853	37.9622
1875	6	20	51	15	63.874	300.248	344.6662	19.1968
1876	24	18	23	53	82.244	275.868	3.0539	358.8677
1877	14	3	12	28	71.508	225.671	352.3348	340.1023
1878	3	12	1	3	60.772	175.474	341.6158	321.3370
1879	22	9	33	40	79.142	151.094	0.0034	301.0079
1880	10	18	22	15	68.406	100.897	349.2844	282.2425
1881	0	3	10	50	57.670	50.700	338.5653	263.4771
1882	19	0	43	27	76.040	26.320	356.9530	243.1480
1883	8	9	32	2	65.304	336.123	346.2339	224.3826
1884	26	7	4	39	83.674	311.743	4.6216	204.0535
1885	15	15	53	14	72.938	261.546	353.9025	185.2881
1886	5	0	41	49	62.202	211.349	343.1835	166.5228
1887	23	22	14	26	80.572	186.969	1.5712	146.1937
1888	12	7	3	0	69.836	136.772	350.8521	127.4283
1889	1	15	51	35	59.100	86.575	340.1330	108.6629
1890	20	13	24	12	77.470	62.196	358.5207	88.3337
1891	9	22	12	47	66.734	11.999	347.8016	69.5683
1892	27	19	45	24	85.104	347.619	6.1893	49.2332
1893	17	4	33	59	74.368	297.422	355.4703	30.4739
1894	6	13	22	33	63.632	247.225	344.7512	11.7085
1895	25	10	55	10	82.002	222.845	3.1339	351.3794
1896	13	19	43	45	71.266	172.648	352.4198	332.6140
1897	3	4	32	19	60.530	122.451	341.7007	313.8486
1898	22	2	4	56	78.900	98.071	0.0884	293.5195
1899	11	10	53	31	68.164	47.874	349.3694	274.7542
1900	0	19	42	6	57.428	357.677	338.6503	255.9888

This Table shows the time of New Moon in March, of each year, with the longitudes, anomalies, &c. of the Sun and Moon at that time, on the supposition that all the motions are performed with uniform angular velocity.



TABLE III.

*Mean Lunations.*

No. Lun.	Mean Lunations.			Sun's Mean Anomaly.	Moon's Mean Anomaly.	Sun and Moon's Mean Longitude.	Longitude of Node.
	<i>d.</i>	<i>h.</i>	<i>m.</i>	<i>sec.</i>			
1	29	12	44	3	29.105	29.817	29.1067
2	59	1	28	6	58.211	51.634	58.2135
3	88	14	12	9	87.316	77.451	87.3202
4	118	2	56	12	116.421	103.268	116.4270
5	147	15	40	14	145.527	129.085	145.5337
6	177	4	24	17	174.632	154.906	174.6405
7	206	17	8	20	203.738	180.718	203.7472
8	236	5	52	23	232.843	206.535	232.8530
9	265	18	36	26	261.948	232.352	261.9607
10	295	7	20	29	291.054	258.169	291.0674
11	324	20	4	32	320.159	283.986	320.1742
12	354	8	48	35	349.264	309.803	349.2309
13	383	21	32	37	378.370	335.620	378.3377
3	14	18	22	1	14.553	192.908	*14.5534

NOTE.—The true quantities for one lunation, from which these tables are calculated, are as follows, viz.

Length of a lunation,	-	-	-	29d.	12h.	44m.	2.88sec.
Sun's mean motion in Anomaly in one lunation,	-	-	-	-	-	29 <sup>o</sup> .	105.35761
Moon's do.	-	-	-	-	-	25	.81692410
Sun and Moon's mean motion in Longitude, do.	-	-	-	-	-	29	.10674457
Mean motion of the Node, do.	-	-	-	-	-	1	.56377989

TABLE IV.

*Mean Motions of the Sun and Moon.*

Days.	Sun's Anomaly.	Sun's Longitude.	Moon's Anomaly.	Moon's Longitude.	Longitude of Node.
1	0.9856	0.98565	13.0650	13.17640	.05295
2	1.9712	1.97129	26.1300	26.35279	.10591
3	2.9568	2.95694	39.1950	39.52919	.15886
4	3.9424	3.94259	52.2600	52.70559	.21182
5	4.9280	4.92824	65.3250	65.88198	.26477
6	5.9136	5.91388	78.3900	79.05838	.31773
7	6.8992	6.89953	91.4549	92.23477	.37068
8	7.8848	7.88518	104.5199	105.41117	.42364
9	8.8704	8.87083	117.5849	118.58757	.47659

Hours.	Sun's Anomaly.	Sun's Longitude.	Moon's Anomaly.	Moon's Longitude.	Longitude of Node.
1	.0411	.04107	0.5444	.54902	.00221
2	.0821	.08214	1.0887	1.09803	.00441
3	.1232	.12321	1.6331	1.64705	.00662
4	.1643	.16427	2.1775	2.19607	.00883
5	.2053	.20534	2.7219	2.74508	.01104
6	.2464	.24641	3.2662	3.29410	.01324
7	.2875	.28748	3.8106	3.84312	.01545
8	.3285	.32855	4.3550	4.39213	.01765
9	.3696	.36962	4.8994	4.94115	.01986

\* For the moon increase this by 180°.

TABLE NO. IV—CONTINUED.

Min- utes.	Sun's Lon. and Anom.	Moon's Anom.	Moon's Lon- gitude.	Lon. of Node.	Se- conds.	Sun's Lon. and Anom.	Moon's Lon. and Anom.
1	.00068	.0091	.00915	.00004	1	.00001	.00015
2	.00137	.0181	.01830	.00007	2	.00002	.00030
3	.00205	.0272	.02745	.00011	3	.00003	.00046
4	.00274	.0363	.03660	.00015	4	.00005	.00061
5	.00342	.0454	.04575	.00018	5	.00006	.00076
6	.00411	.0544	.05490	.00022	6	.00007	.00091
7	.00479	.0635	.06405	.00026	7	.00008	.00107
8	.00548	.0726	.07320	.00029	8	.00009	.00122
9	.00616	.0817	.08235	.00033	9	.00010	.00137

TABLE V.

*Days of the year reckoned from March.*

Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.
1	32	62	93	123	154	185	215	246	276	307	338
2	33	63	94	124	155	186	216	247	277	308	339
3	34	64	95	125	156	187	217	248	278	309	340
4	35	65	96	126	157	188	218	249	279	310	341
5	36	66	97	127	158	189	219	250	280	311	342
6	37	67	98	128	159	190	220	251	281	312	343
7	38	68	99	129	160	191	221	252	282	313	344
8	39	69	100	130	161	192	222	253	283	314	345
9	40	70	101	131	162	193	223	254	284	315	346
10	41	71	102	132	163	194	224	255	285	316	347
11	42	72	103	133	164	195	225	256	286	317	348
12	43	73	104	134	165	196	226	257	287	318	349
13	44	74	105	135	166	197	227	258	288	319	350
14	45	75	106	136	167	198	228	259	289	320	351
15	46	76	107	137	168	199	229	260	290	321	352
16	47	77	108	138	169	200	230	261	291	322	353
17	48	78	109	139	170	201	231	262	292	323	354
18	49	79	110	140	171	202	232	263	293	324	355
19	50	80	111	141	172	203	233	264	294	325	356
20	51	81	112	142	173	204	234	265	295	326	357
21	52	82	113	143	174	205	235	266	296	327	358
22	53	83	114	144	175	206	236	267	297	328	359
23	54	84	115	145	176	207	237	268	298	329	360
24	55	85	116	146	177	208	238	269	299	330	361
25	56	86	117	147	178	209	239	270	300	331	362
26	57	87	118	148	179	210	240	271	301	332	363
27	58	88	119	149	180	211	241	272	302	333	364
28	59	89	120	150	181	212	242	273	303	334	365
29	60	90	121	151	182	213	243	274	304	335	366
30	61	91	122	152	183	214	244	275	305	336	
31		92		153	184		245		306	337	

This Table shows the month and day to which any number of days in a year, reckoned from the 1st of March, corresponds.

TABLE VI.

*Annual Equation of the Moon's Perigee.*

ARGUMENT—Sun's Anomaly.

Arg.	0°	2°	4°	6°	8°	10°	Arg.
0	.000	.013	.026	.038	.051	.063	+
1	.063	.076	.088	.101	.113	.126	35
2	.126	.138	.149	.161	.172	.183	34
3	.183	.194	.205	.216	.226	.236	33
4	.236	.245	.255	.264	.273	.282	32
5	.282	.290	.298	.305	.312	.319	31
6	.319	.325	.331	.337	.342	.347	30
7	.347	.351	.355	.359	.362	.365	29
8	.365	.367	.369	.370	.371	.371	28
9	.371	.371	.371	.370	.369	.367	27
10	.367	.365	.362	.359	.355	.351	26
11	.351	.347	.342	.337	.331	.324	25
12	.324	.318	.311	.304	.296	.288	24
13	.288	.280	.271	.261	.252	.242	23
14	.242	.231	.221	.210	.199	.188	22
15	.188	.177	.165	.153	.141	.129	21
16	.129	.117	.104	.092	.079	.066	20
17	.066	.052	.039	.026	.013	.000	19
Arg.	10°	8°	6°	4°	2°	0°	Arg.

The sun's attraction causes a progressive motion in the line of the moon's apsides, which affects the place of the perigee; and since the distance of the sun varies in different seasons of the year, its attraction also varies. This causes the motion to be more rapid at some times than at others, occasioning inequalities in the moon's anomaly, for which this Table furnishes the correction.

TABLE VII.

*Annual Equation of the Moon's Node.*

ARGUMENT—Sun's Anomaly.

Arg.	0°	2°	4°	6°	8°	10°	Arg.
0	.0000	.0053	.0106	.0159	.0212	.0264	+
1	.0264	.0316	.0367	.0418	.0469	.0520	35
2	.0520	.0570	.0619	.0667	.0714	.0760	34
3	.0760	.0805	.0849	.0892	.0934	.0975	33
4	.0975	.1015	.1053	.1090	.1126	.1160	32
5	.1160	.1192	.1223	.1253	.1281	.1308	31
6	.1308	.1333	.1356	.1378	.1398	.1417	30
7	.1417	.1434	.1449	.1462	.1373	.1481	29
8	.1481	.1488	.1493	.1496	.1499	.1500	28
9	.1500	.1498	.1494	.1488	.1481	.1473	27
10	.1473	.1463	.1451	.1437	.1421	.1402	26
11	.1402	.1381	.1359	.1336	.1313	.1289	25
12	.1289	.1263	.1235	.1205	.1273	.1138	24
13	.1138	.1103	.1067	.1030	.0992	.0953	23
14	.0953	.0913	.0871	.0828	.0784	.0740	22
15	.0740	.0695	.0648	.0600	.0551	.0501	21
16	.0501	.0452	.0403	.0355	.0306	.0257	20
17	.0257	.0206	.0155	.0104	.0052	.0000	19
Arg.	10°	8°	6°	4°	2°	0°	Arg.

The retrograde motion of the moon's nodes is caused by the sun's attraction, and as the distance of that luminary varies in different seasons of the year, its attraction must also vary, producing inequalities in the motion of the nodes, for which this Table supplies the correction.



## TABLE VIII.

*Equation of the Sun's Centre.*

ARGUMENT—Sun's Anomaly.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
+												—
0	.0000	.0343	.0685	.1028	.1370	.1711	.2052	.2393	.2733	.3071	.3409	35
1	.3409	.3746	.4081	.4415	.4748	.5079	.5408	.5736	.6062	.6386	.6708	34
2	.6708	.7028	.7345	.7660	.7972	.8282	.8590	.8895	.9196	.9495	.9790	33
3	.9790	1.0083	1.0372	1.0658	1.0941	1.1220	1.1493	1.1767	1.2035	1.2299	1.2559	32
4	1.2559	1.2815	1.3067	1.3315	1.3559	1.3799	1.4034	1.4264	1.4491	1.4711	1.4928	31
5	1.4928	1.5140	1.5347	1.5549	1.5747	1.5939	1.6127	1.6309	1.6486	1.6658	1.6824	30
6	1.6824	1.6986	1.7142	1.7293	1.7439	1.7578	1.7712	1.7841	1.7965	1.8112	1.8194	29
7	1.8194	1.8301	1.8401	1.8496	1.8585	1.8669	1.8747	1.8819	1.8885	1.8945	1.9000	28
8	1.9000	1.9049	1.9091	1.9128	1.9159	1.9185	1.9204	1.9217	1.9225	1.9226	1.9223	27
9	1.9223	1.9213	1.9197	1.9175	1.9148	1.9115	1.9075	1.9030	1.8980	1.8924	1.8862	26
10	1.8862	1.8794	1.8721	1.8642	1.8557	1.8467	1.8372	1.8270	1.8164	1.8052	1.7935	25
11	1.7935	1.7812	1.7684	1.7551	1.7412	1.7269	1.7120	1.6966	1.6807	1.6644	1.6475	24
12	1.6475	1.6301	1.6123	1.5940	1.5752	1.5560	1.5362	1.5161	1.4955	1.4745	1.4530	23
13	1.4530	1.4311	1.4088	1.3861	1.3630	1.3395	1.3155	1.2915	1.2666	1.2415	1.2162	22
14	1.2162	1.1904	1.1643	1.1379	1.1111	1.0840	1.0566	1.0289	1.0009	.9726	.9441	21
15	.9441	.9152	.8861	.8567	.8271	.7973	.7672	.7369	.7064	.6757	.6448	20
16	.6448	.6137	.5825	.5510	.5194	.4877	.4558	.4238	.3917	.3594	.3271	19
17	.3271	.2946	.2621	.2296	.1969	.1641	.1314	.0985	.0657	.0329	.0000	18
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

Owing to the elliptical form of the sun's apparent orbit, it does not revolve with uniform angular velocity, and this Table shows the correction in its longitude, required to be made on that account.

The epoch of this Table is 1840.

## TABLE IX.

*Equation of the Moon's Centre.*

ARGUMENT—Moon's Anomaly.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
+												—
0	0.0000	0.1181	0.2362	0.3542	0.4721	0.5897	0.7072	0.8244	0.9412	1.0578	1.1738	35
1	1.1738	1.2896	1.4048	1.5223	1.6336	1.7471	1.8600	1.9721	2.0835	2.1944	2.3041	34
2	2.3041	2.4131	2.5212	2.6284	2.7346	2.8398	2.9439	3.0470	3.1490	3.2498	3.3494	33
3	3.3494	3.4478	3.5450	3.6409	3.7354	3.8287	3.9205	4.0109	4.0999	4.1874	4.2735	32
4	4.2735	4.3580	4.4410	4.5223	4.6022	4.6804	4.7569	4.8318	4.9050	4.9765	5.0463	31
5	5.0463	5.1143	5.1806	5.2451	5.3078	5.3687	5.4277	5.4849	5.5403	5.5938	5.6454	30
6	5.6454	5.6952	5.7480	5.7889	5.8330	5.8750	5.9152	5.9534	5.9897	6.0240	6.0564	29
7	6.0564	6.0868	6.1152	6.1417	6.1663	6.1889	6.2094	6.2281	6.2448	6.2595	6.2722	28
8	6.2722	6.2830	6.2919	6.2988	6.3038	6.3068	6.3079	6.3070	6.3043	6.2997	6.2931	27
9	6.2931	6.2847	6.2743	6.2621	6.2481	6.2322	6.2144	6.1948	6.1734	6.1502	6.1252	26
10	6.1252	6.0984	6.0699	6.0396	6.0076	5.9739	5.9384	5.9013	5.8624	5.8220	5.7799	25
11	5.7799	5.7362	5.6908	5.6439	5.5954	5.5453	5.4937	5.4406	5.3860	5.3300	5.2723	24
12	5.2723	5.2135	5.1531	5.0913	5.0281	4.9636	4.8978	4.8306	4.7622	4.6924	4.6215	23
13	4.6215	4.5493	4.4759	4.4013	4.3256	4.2487	4.1707	4.0915	4.0114	3.9302	3.8480	22
14	3.8480	3.7620	3.6806	3.5954	3.5093	3.4223	3.3344	3.2457	3.1562	3.0658	2.9747	21
15	2.9747	2.8828	2.7901	2.6968	2.6028	2.5081	2.4127	2.3168	2.2203	2.1232	2.0256	20
16	2.0256	1.9275	1.8289	1.7298	1.6303	1.5304	1.4301	1.3294	1.2285	1.1272	1.0256	19
17	1.0256	0.9238	0.8217	0.7194	0.6170	0.5144	0.4117	0.3088	0.2059	0.1030	0.0000	18
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

Owing to the elliptical form of the moon's orbit, it does not revolve with uniform angular velocity, and this Table shows the correction in its longitude required to be made on that account.

## TABLE X.

*Annual Equation of the Moon's Longitude.*

ARGUMENT—Sun's Mean Anomaly.

Arg.	0°	2°	4°	6°	8°	10°	Arg.
0	.0000	.0064	.0128	.0192	.0255	.0318	+
1	.0318	.0381	.0443	.0505	.0567	.0627	34
2	.0627	.0687	.0747	.0805	.0863	.0920	33
3	.0920	.0974	.1028	.1081	.1132	.1183	32
4	.1183	.1232	.1280	.1326	.1370	.1413	31
5	.1413	.1454	.1494	.1532	.1568	.1602	30
6	.1602	.1634	.1664	.1692	.1719	.1743	29
7	.1743	.1764	.1785	.1803	.1818	.1832	28
8	.1832	.1843	.1852	.1859	.1864	.1866	27
9	.1866	.1865	.1864	.1860	.1853	.1843	26
10	.1843	.1832	.1819	.1802	.1784	.1764	25
11	.1764	.1742	.1717	.1690	.1662	.1630	24
12	.1630	.1597	.1562	.1525	.1486	.1446	23
13	.1446	.1404	.1359	.1313	.1265	.1216	22
14	.1216	.1165	.1113	.1059	.1004	.0947	21
15	.0947	.0890	.0831	.0771	.0711	.0649	20
16	.0649	.0586	.0523	.0459	.0395	.0330	19
17	.0330	.0264	.0198	.0132	.0066	.0000	18
Arg.	10°	8°	6°	4°	2°	0°	Arg.

The attraction of the sun upon the moon tends to draw it away from the earth and thus dilate its orbit; and since the distance of the sun from the earth varies in different seasons of the year, its attraction also varies. This causes a variation in the size of the moon's orbit, as well as in its velocity, producing inequalities in its longitude, for which this Table gives the required corrections.

## TABLE XI.

*Secular Equation, showing the Acceleration of the Moon's Mean Motion.*

ARGUMENT—The date.

Arg.	0	5
181	00	01
182	01	02
183	03	04
184	05	06
185	07	09
186	11	13
187	15	17
188	19	21
189	24	27
190	30	33

The eccentricity of the earth's orbit is, and has been for ages, slowly diminishing, which renders the sun's disturbing influence on the moon less and less every year, thus allowing the orbit of the latter to contract, diminishing its periodic time. This Table contains the necessary corrections from this cause, at intervals of five years during the present century.

NOTE.—The numbers in this Table are the 3d and 4th places of decimals of a degree; therefore in using them, two cyphers must be prefixed as decimals. Thus for 33 read .0033.

TABLE XII.

*Variation.*

ARGUMENT—Moon's Longitude diminished by that of the Sun.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
0	+0000	+0204	+0407	+0610	+0812	+1013	+1212	+1410	+1607	+1801	+1993	35
1	+1993	+2183	+2370	+2553	+2734	+2911	+3084	+3254	+3419	+3580	+3736	34
2	+3736	+3887	+4034	+4175	+4312	+4443	+4568	+4687	+4801	+4908	+5009	33
3	+5009	+5104	+5192	+5274	+5349	+5418	+5479	+5534	+5581	+5622	+5656	32
4	+5656	+5682	+5701	+5713	+5718	+5716	+5706	+5689	+5666	+5634	+5597	31
5	+5597	+5551	+5499	+5440	+5374	+5302	+5222	+5137	+5044	+4946	+4841	30
6	+4841	+4730	+4613	+4490	+4361	+4227	+4088	+3944	+3794	+3640	+3481	29
7	+3481	+3317	+3149	+2978	+2802	+2623	+2440	+2254	+2065	+1874	+1680	28
8	+1680	+1483	+1281	+1084	+0882	+0679	+0475	+0270	+0064	-0142	-0348	27
9	-0348	-0554	-0760	-0965	-1170	-1373	-1575	-1775	-1973	-2170	-2364	26
10	-2364	-2555	-2744	-2929	-3112	-3291	-3466	-3638	-3805	-3969	-4127	25
11	-4127	-4281	-4430	-4575	-4714	-4847	-4975	-5097	-5214	-5324	-5429	24
12	-5429	-5526	-5618	-5703	-5781	-5853	-5918	-5976	-6026	-6070	-6107	23
13	-6107	-6137	-6159	-6174	-6182	-6183	-6176	-6162	-6140	-6111	-6076	22
14	-6076	-6033	-5982	-5925	-5860	-5789	-5711	-5626	-5534	-5435	-5330	21
15	-5330	-5220	-5101	-4977	-4847	-4712	-4571	-4424	-4271	-4114	-3952	20
16	-3952	-3785	-3614	-3437	-3257	-3073	-2885	-2694	-2500	-2302	-2102	19
17	-2102	-1899	-1694	-1487	-1278	-1067	-0855	-0642	-0429	-0215	-0000	18
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

Reverse the Signs.

Reverse the Signs.

Owing to the disturbing influence of the sun, the moon is alternately accelerated and retarded in the different quadrants, reckoning from syzygy. This Table contains the corrections in its longitude resulting from this cause.

TABLE XIII.

*Annual Equation of Variation.*

ARGUMENTS—The argument of Variation at the top and bottom, and the Sun's Mean Anomaly at the sides.

		90	95	100	105	110	115	120	125	130	135		
		270	275	280	285	290	295	300	305	310	315		
Arg.	Arg.	0	5	10	15	20	25	30	35	40	45		
		180	185	190	195	200	205	210	215	220	225		
0	180	-0000	-0064	-0125	-0183	-0233	-0281	-0314	-0342	-0358	-0364	180	360
1	190	+0003	-0053	-0114	-0172	-0225	-0267	-0306	-0333	-0353	-0358	170	350
2	200	+0019	-0042	-0100	-0156	-0211	-0250	-0286	-0317	-0333	-0342	160	340
3	210	+0028	-0038	-0093	-0133	-0181	-0225	-0261	-0286	-0306	-0314	150	330
4	220	+0033	-0044	-0091	-0130	-0165	-0192	-0225	-0250	-0267	-0281	140	320
5	230	+0042	-0050	-0099	-0131	-0161	-0188	-0211	-0230	-0246	-0259	130	310
6	240	+0047	-0057	-0109	-0141	-0169	-0195	-0218	-0237	-0252	-0264	120	300
7	250	+0050	-0062	-0117	-0151	-0181	-0208	-0231	-0250	-0265	-0276	110	290
8	260	+0053	-0066	-0123	-0159	-0189	-0216	-0239	-0258	-0273	-0284	100	280
9	270	+0053	-0065	-0125	-0163	-0195	-0223	-0247	-0266	-0281	-0291	90	270
10	280	+0053	-0064	-0125	-0165	-0198	-0227	-0252	-0271	-0286	-0296	80	260
11	290	+0050	-0072	-0139	-0183	-0219	-0248	-0273	-0292	-0307	-0317	70	250
12	300	+0047	-0078	-0149	-0200	-0239	-0269	-0294	-0313	-0328	-0338	60	240
13	310	+0042	-0081	-0159	-0215	-0257	-0289	-0314	-0333	-0348	-0358	50	230
14	320	+0033	-0083	-0169	-0231	-0277	-0311	-0336	-0355	-0370	-0379	40	220
15	330	+0028	-0084	-0178	-0245	-0295	-0330	-0355	-0374	-0389	-0398	30	210
16	340	+0019	-0078	-0193	-0265	-0319	-0355	-0380	-0399	-0414	-0423	20	200
17	350	+0003	-0072	-0213	-0291	-0349	-0387	-0412	-0431	-0446	-0455	10	190
		90	85	80	75	70	65	60	55	50	45	Arg.	Arg.
		270	265	260	255	250	245	240	235	230	225		
		180	175	170	165	160	155	150	145	140	135		
		360	355	350	345	340	335	330	325	320	315		

The inequality in the moon's motion, denominated Variation, and for which Table 12th furnishes the correction, being occasioned by the disturbing influence of the sun, must be greater or less according as the distance of the earth from that luminary varies. In that Table the earth is supposed to be at its mean distance; hence another correction becomes necessary, which this Table furnishes.



TABLE XIV.

*Evection.*

ARGUMENT—The Moon's Mean Anomaly diminished by twice the excess of the Moon's Mean Longitude over the True Longitude of the Sun.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
0	.0000	.0238	.0475	.0713	.0950	.1187	.1423	.1659	.1894	.2129	.2363	+
1	.2363	.2596	.2829	.3061	.3291	.3521	.3750	.3978	.4204	.4428	.4651	34
2	.4651	.4873	.5093	.5312	.5530	.5744	.5958	.6170	.6379	.6587	.6793	33
3	.6793	.6996	.7197	.7396	.7593	.7787	.7979	.8168	.8355	.8539	.8720	32
4	.8720	.8899	.9074	.9247	.9417	.9584	.9748	.9909	1.0067	1.0222	1.0374	31
5	1.0374	1.0522	1.0667	1.0808	1.0947	1.1082	1.1213	1.1341	1.1465	1.1586	1.1703	30
6	1.1703	1.1817	1.1926	1.2032	1.2135	1.2234	1.2323	1.2420	1.2507	1.2590	1.2670	29
7	1.2670	1.2745	1.2816	1.2884	1.2948	1.3007	1.3063	1.3114	1.3162	1.3206	1.3245	28
8	1.3245	1.3281	1.3312	1.3339	1.3362	1.3381	1.3396	1.3407	1.3414	1.3417	1.3415	27
9	1.3415	1.3410	1.3400	1.3386	1.3369	1.3347	1.3321	1.3291	1.3257	1.3220	1.3178	26
10	1.3178	1.3132	1.3082	1.3028	1.2971	1.2909	1.2844	1.2774	1.2701	1.2624	1.2543	25
11	1.2543	1.2459	1.2370	1.2278	1.2182	1.2083	1.1980	1.1872	1.1763	1.1650	1.1533	24
12	1.1533	1.1412	1.1288	1.1161	1.1031	1.0897	1.0760	1.0619	1.0476	1.0330	1.0180	23
13	1.0180	1.0027	.9872	.9713	.9552	.9388	.9221	.9051	.8879	.8704	.8526	22
14	.8526	.8346	.8164	.7979	.7792	.7603	.7411	.7217	.7021	.6822	.6622	21
15	.6622	.6420	.6217	.6011	.5803	.5594	.5383	.5171	.4957	.4742	.4525	20
16	.4525	.4307	.4087	.3867	.3646	.3423	.3199	.2975	.2749	.2523	.2296	19
17	.2296	.2068	.1840	.1611	.1382	.1152	.0922	.0692	.0462	.0231	.0000	18
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

The disturbing influence of the sun causes a variation in the eccentricity of the moon's orbit, and in the position of the major axis. This affects the moon's longitude, producing inequalities, for which this Table gives the required corrections.

TABLE XV.

*Annual Equation of Evection.*

ARGUMENTS—The argument for Evection at the top and bottom, and the Sun's Mean Anomaly at the sides.

Arg.	0 10 20 30 40 50 60 70 80 90											
	Arg.	180	190	200	210	220	230	240	250	260	270	
0	180	.0000	-.0031	-.0161	-.0236	-.0303	-.0364	-.0408	-.0444	-.0467	-.0472	360 180
10	190	+.0036	-.0047	-.0122	-.0200	-.0272	-.0331	-.0386	-.0425	-.0453	-.0467	350 170
20	200	+.0072	-.0005	-.0033	-.0158	-.0228	-.0294	-.0347	-.0394	-.0425	-.0444	340 160
30	210	+.0108	+.0033	-.0042	-.0111	-.0181	-.0244	-.0303	-.0347	-.0386	-.0408	330 150
40	220	+.0136	+.0072	+.0005	-.0064	-.0128	-.0189	-.0244	-.0294	-.0331	-.0364	320 140
50	230	+.0164	+.0108	+.0050	-.0011	-.0072	-.0128	-.0181	-.0228	-.0272	-.0303	310 130
60	240	+.0186	+.0142	+.0092	+.0042	-.0011	-.0064	-.0111	-.0158	-.0200	-.0236	300 120
70	250	+.0200	+.0175	+.0133	+.0092	+.0050	+.0005	-.0042	-.0083	-.0122	-.0161	290 110
80	260	+.0211	+.0192	+.0175	+.0142	+.0108	+.0072	+.0033	-.0005	-.0047	-.0081	280 100
90	270	+.0211	+.0211	+.0200	+.0186	+.0164	+.0136	+.0103	+.0072	+.0036	.0000	270 90
100	280	+.0211	+.0219	+.0228	+.0222	+.0214	+.0200	+.0175	+.0150	+.0117	+.0081	260 80
110	290	+.0200	+.0228	+.0242	+.0256	+.0258	+.0253	+.0241	+.0219	+.0195	+.0161	250 70
120	300	+.0156	+.0222	+.0256	+.0278	+.0294	+.0300	+.0297	+.0286	+.0264	+.0236	240 60
130	310	+.0164	+.0214	+.0258	+.0294	+.0320	+.0339	+.0344	+.0342	+.0328	+.0303	230 50
140	320	+.0136	+.0200	+.0253	+.0300	+.0339	+.0364	+.0383	+.0386	+.0381	+.0364	220 40
150	330	+.0108	+.0175	+.0241	+.0297	+.0344	+.0383	+.0405	+.0422	+.0422	+.0408	210 30
160	340	+.0072	+.0150	+.0219	+.0286	+.0342	+.0386	+.0422	+.0422	+.0410	+.0444	200 20
170	350	+.0036	+.0117	+.0195	+.0264	+.0328	+.0381	+.0422	+.0450	+.0464	+.0467	190 10
180	360	+.0000	+.0081	+.0161	+.0236	+.0303	+.0364	+.0408	+.0444	+.0467	+.0472	180 0
												Arg.
												Arg.

The inequality in the moon's motion denominated *Evection*, and for which Table 14th furnishes the correction, being occasioned by the disturbing influence of the sun, must be greater or less according as the distance of the earth from that luminary varies. In that Table the earth is supposed to be at its *mean* distance; hence another correction becomes necessary, which this Table furnishes.

NOTE.—The foregoing explanation is true so far as it goes; but the values given in the Table are partially referable to a variation in the argument for *Evection* occasioned by the *Annual Equation* of the Moon's Longitude. And we might on this principle add several other Tables, which should contain corrections required by the alteration of the arguments of previous ones; but this would not accord with the simplicity of our design.

TABLE XVI.

*Nodal Equation of the Moon's Longitude.*

ARGUMENT—The Sun's longitude diminished by that of the Moon's Node.

Arg.		0°	2°	4°	6°	8°	10°	Arg.	
0	18	.0000	.0012	.0024	.0036	.0048	.0059	17	35
1	19	.0059	.0071	.0082	.0092	.0102	.0112	16	34
2	20	.0112	.0121	.0129	.0137	.0144	.0150	15	33
3	21	.0150	.0156	.0161	.0165	.0169	.0171	14	32
4	22	.0171	.0173	.0174	.0174	.0173	.0171	13	31
5	23	.0171	.0169	.0165	.0161	.0156	.0150	12	30
6	24	.0150	.0144	.0137	.0129	.0121	.0112	11	29
7	25	.0112	.0102	.0092	.0082	.0071	.0059	10	28
8	26	.0059	.0048	.0036	.0024	.0012	.0000	9	27
Arg.		10°	8°	6°	4°	2°	0°	Arg.	

The influence of the sun tending to dilate the moon's orbit is greatest when the former is in the plane of the latter; i. e. when passing the nodes, and least when farthest from them, occasioning an inequality in the moon's motion, for which this Table furnishes the necessary correction.

TABLE XVII.

*Reduction.*

ARGUMENT—The Longitude of the Moon, diminished by that of its Node.

Arg.		0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.	
0	18	.0000	.0039	.0079	.0118	.0157	.0196	.0235	.0273	.0311	.0349	.0386	17	35
1	19	.0386	.0423	.0460	.0496	.0531	.0565	.0599	.0632	.0664	.0696	.0726	16	34
2	20	.0726	.0756	.0785	.0813	.0837	.0866	.0891	.0914	.0937	.0958	.0979	15	33
3	21	.0979	.0998	.1016	.1033	.1048	.1062	.1075	.1087	.1096	.1105	.1113	14	32
4	22	.1113	.1119	.1124	.1127	.1129	.1130	.1129	.1127	.1124	.1119	.1113	13	31
5	23	.1113	.1105	.1096	.1087	.1075	.1062	.1048	.1033	.1016	.0998	.0979	12	30
6	24	.0979	.0958	.0937	.0914	.0891	.0866	.0837	.0813	.0785	.0756	.0726	11	29
7	25	.0726	.0696	.0664	.0632	.0599	.0565	.0531	.0496	.0460	.0423	.0386	10	28
8	26	.0386	.0349	.0311	.0273	.0235	.0196	.0157	.0118	.0079	.0039	.0000	9	27
Arg.		10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.	

The preceding Tables give the Moon's Longitude in its orbit: this reduces it to the Ecliptic.

TABLE XVIII.

*Lunar or Menstrual Equation of the Sun's Longitude.*

ARGUMENT—The Longitude of the Moon diminished by that of the Sun.

Arg.		0°	5°	10°	Arg.	
+	18	.0000	.0002	.0004	+	35
1	19	.0004	.0005	.0007	16	34
2	20	.0007	.0009	.0010	15	33
3	21	.0010	.0012	.0013	14	32
4	22	.0013	.0015	.0016	13	31
5	23	.0016	.0017	.0018	12	30
6	24	.0018	.0019	.0020	11	29
7	25	.0020	.0020	.0021	10	28
8	26	.0021	.0021	.0021	9	27
Arg.		10°	5°	0°	Arg.	

The revolution of the earth around the common centre of gravity of the moon and earth, affects the sun's apparent place, causing a change in its longitude for which this Table furnishes the correction.

## TABLE XIX.

*Lunar Nutation in Longitude.*

ARGUMENT—Longitude of the Moon's Ascending Node.

Arg.	Arg.		Arg.	Arg.
0°	180°	.0000	180°	360°
10	170	.0009	190	350
20	160	.0017	200	340
30	150	.0024	210	330
40	140	.0031	220	320
50	130	.0037	230	310
60	120	.0042	240	300
70	110	.0046	250	290
80	100	.0047	260	280
90	90	.0048	270	270

The Precession of the Equinoxes, being occasioned in part by the attraction of the moon lying out of the plane of the earth's equator, must be more or less rapid according to the obliquity of the plane of its orbit to that of the equator, which depends on the longitude of the moon's nodes. The longitudes of all the heavenly bodies, being reckoned from the Vernal Equinox, must be affected by any change in the place of the point from which they are reckoned, and therefore need a correction from this cause, which this Table supplies.

## TABLE XX.

*Sun's Semi-diameter and Horary Motion.*

ARGUMENT—Sun's Anomaly.

Arg.	Semi-diameter.	Horary motion.	Arg.
0°	.2717	.0425	360°
10	.2716	.0424	350
20	.2714	.0423	340
30	.2710	.0422	330
40	.2705	.0421	320
50	.2700	.0419	310
60	.2694	.0417	300
70	.2687	.0415	290
80	.2679	.0413	280
90	.2671	.0411	270
100	.2663	.0409	260
110	.2655	.0407	250
120	.2648	.0405	240
130	.2642	.0403	230
140	.2637	.0401	220
150	.2632	.0400	210
160	.2628	.0399	200
170	.2626	.0398	190
180	.2625	.0397	180

The elliptical shape of the sun's apparent orbit causes it to vary, both as to apparent size and velocity. The values of these are given in this Table, at intervals of 10° throughout the entire orbit.



## TABLE XXI.

*Moon's Semi-diameter, Horary Motion and Equatorial Parallax.*

ARGUMENT—Moon's corrected Anomaly.

Argument.	Semi-diameter.	Horary motion.	Equat. Parallax.	Argument.
0°	.2743	.6133	1°.0051	360
5	.2742	.6131	1°.0049	355
10	.2740	.6129	1°.0042	350
15	.2737	.6106	1°.0030	345
20	.2733	.6086	1°.0014	340
25	.2727	.6061	.9993	335
30	.2719	.6031	.9968	330
35	.2712	.5994	.9939	325
40	.2703	.5956	.9907	320
45	.2693	.5911	.9871	315
50	.2682	.5864	.9833	310
55	.2671	.5816	.9792	305
60	.2659	.5767	.9749	300
65	.2647	.5714	.9705	295
70	.2635	.5661	.9659	290
75	.2623	.5608	.9613	285
80	.2611	.5556	.9567	280
85	.2599	.5503	.9521	275
90	.2586	.5450	.9475	270
95	.2574	.5397	.9429	265
100	.2562	.5350	.9384	260
105	.2550	.5303	.9342	255
110	.2539	.5256	.9302	250
115	.2528	.5214	.9263	245
120	.2518	.5173	.9227	240
125	.2509	.5136	.9194	235
130	.2500	.5100	.9162	230
135	.2492	.5069	.9133	225
140	.2485	.5039	.9108	220
145	.2479	.5014	.9086	215
150	.2474	.4992	.9066	210
155	.2469	.4972	.9048	205
160	.2465	.4958	.9034	200
165	.2462	.4944	.9023	195
170	.2460	.4936	.9016	190
175	.2459	.4932	.9011	185
180	.2458	.4930	.9009	180

The principle and construction of this Table is the same as that of Table 20th. At the time of new or full moon the quantities in this Table must be increased for the effect of Variation as follows, viz. 1st column, .0020; 2d do. .0115; 3d do. .0073.

## TABLE XXII.

*Moon's Semi-diameter, Hourly Motion, and Equatorial Parallax, as affected by Evection.*

ARGUMENT—The same as for Evecton, Table 14th.

Arg.	Semi-diameter.	Hourly motion.	Equatorial Parallax.	Arg.
0°	+.0029	+.0112	+.0105	360°
10	+.0028	+.0109	+.0103	350
20	+.0027	+.0103	+.0098	340
30	+.0025	+.0097	+.0091	330
40	+.0022	+.0086	+.0081	320
50	+.0019	+.0070	+.0068	310
60	+.0014	+.0055	+.0052	300
70	+.0009	+.0036	+.0035	290
80	+.0005	+.0019	+.0018	280
90	-.0000	-.0002	-.0001	270
100	-.0005	-.0020	-.0019	260
110	-.0010	-.0038	-.0037	250
120	-.0014	-.0055	-.0054	240
130	-.0019	-.0071	-.0068	230
140	-.0022	-.0083	-.0080	220
150	-.0024	-.0094	-.0089	210
160	-.0026	-.0103	-.0096	200
170	-.0027	-.0107	-.0100	190
180	-.0028	-.0109	-.0103	180

All the inequalities in the moon's longitude, for which the foregoing Tables give the corrections, must likewise affect its apparent size, hourly motion, and equatorial parallax. Variation and Evecton are the only ones that it is important to take into account, the former of which may be considered constant at the time of new or full moon, and this Table gives the requisite correction for the latter.

## TABLE XXIII.

*Obliquity of the Ecliptic to the Equator.*

ARGUMENT—The date.

Arg.	0	1	2	3	4	5	6	7	8	9
184	23 27 15.1	23 27 43.2	23 27 40.4	23 27 37.1	23 27 33.5	23 27 29.6	23 27 27.1	23 27 24.8	23 27 23.6	23 27 23.4
185	23 27 24.3	23 27 26.0	23 27 28.3	23 27 31.0	23 27 33.6	23 27 35.7	23 27 37.2	23 27 37.7	23 27 37.3	23 27 36.0
186	23 27 33.6	23 27 30.7	23 27 27.2	23 27 23.7	23 27 21.3	23 27 17.6	23 27 15.8	23 27 14.9	23 27 15.1	23 27 15.9
187	23 27 18.4	23 27 20.1	23 27 23.5	23 27 26.0	23 27 27.9	23 27 29.0	23 27 29.2	23 27 28.5	23 27 26.7	23 27 24.1
188	23 27 20.8	23 27 17.4	23 27 13.9	23 27 10.8	23 27 8.4	23 27 6.8	23 27 6.3	23 27 7.0	23 27 8.6	23 27 10.8
189	23 27 13.4	23 27 16.0	23 27 18.3	23 27 20.0	23 27 20.7	23 27 20.5	23 27 19.4	23 27 17.3	23 27 14.4	23 27 11.0

The obliquity of the ecliptic to the equator is slowly diminishing, owing to the attraction of the planets, and is also subject to an inequality whose period is about nineteen years, caused by the attraction of the moon, and called Nutation. This Table gives the obliquity on the 1st of January in each year, taking both these causes into account.

TABLE XXIV.

*Moon's Latitude in Eclipses.*

ARGUMENT—Moon's Longitude diminished by that of its Node.

Arg.	.0	.1	.2	.3	.4	.5	.6	.7	.8	.9	1 <sup>o</sup> .0	Arg.	
N.A.	S.D.											N.D.	S.A.
0	180	0.0000	0.0087	0.0175	0.0262	0.0350	0.0437	0.0524	0.0611	0.0698	0.0785	0.0873	179 359
1	181	0.0873	0.0960	0.1048	0.1135	0.1222	0.1310	0.1397	0.1484	0.1572	0.1659	0.1746	178 358
2	182	0.1746	0.1834	0.1921	0.2008	0.2095	0.2182	0.2269	0.2356	0.2443	0.2530	0.2617	177 357
3	183	0.2617	0.2704	0.2791	0.2878	0.2965	0.3052	0.3139	0.3226	0.3313	0.3399	0.3486	176 356
4	184	0.3486	0.3572	0.3659	0.3746	0.3832	0.3919	0.4005	0.4092	0.4180	0.4267	0.4353	175 355
5	185	0.4353	0.4440	0.4526	0.4613	0.4699	0.4786	0.4873	0.4960	0.5046	0.5133	0.5219	174 354
6	186	0.5219	0.5306	0.5392	0.5478	0.5565	0.5652	0.5739	0.5826	0.5912	0.5999	0.6085	173 353
7	187	0.6085	0.6173	0.6258	0.6343	0.6431	0.6517	0.6604	0.6690	0.6778	0.6864	0.6950	172 352
8	188	0.6950	0.7036	0.7122	0.7208	0.7295	0.7381	0.7467	0.7553	0.7639	0.7725	0.7811	171 351
9	189	0.7811	0.7897	0.7983	0.8069	0.8155	0.8241	0.8327	0.8413	0.8499	0.8585	0.8672	170 350
10	190	0.8672	0.8758	0.8844	0.8930	0.9015	0.9101	0.9187	0.9273	0.9358	0.9444	0.9529	169 349
11	191	0.9529	0.9614	0.9700	0.9785	0.9870	0.9955	1.0041	1.0126	1.0211	1.0296	1.0382	168 348
12	192	1.0382	1.0467	1.0552	1.0638	1.0723	1.0808	1.0893	1.0978	1.1063	1.1148	1.1233	167 347
13	193	1.1233	1.1318	1.1403	1.1488	1.1573	1.1658	1.1743	1.1828	1.1913	1.1998	1.2082	166 346
14	194	1.2082	1.2167	1.2251	1.2335	1.2420	1.2504	1.2588	1.2672	1.2756	1.2841	1.2925	165 345
15	195	1.2925	1.3009	1.3093	1.3177	1.3261	1.3345	1.3428	1.3512	1.3596	1.3680	1.3765	164 344
16	196	1.3765	1.3849	1.3933	1.4017	1.4101	1.4185	1.4269	1.4353	1.4437	1.4521	1.4605	163 343
Arg.	1 <sup>o</sup> .0	.9	.8	.7	.6	.5	.4	.3	.2	.1	.0	Arg.	

The moon has sometimes a north and sometimes a south latitude, owing to the obliquity of the plane of its orbit to that of the ecliptic. This Table gives the latitude for every tenth of a degree of longitude, reckoned 17° either way from each node. The capital letters at the head of the columns of the argument show whether the latitude is north or south, and whether it is ascending or descending.

TABLE XXV.

*Angle of the visible path of the Moon with the Ecliptic in Eclipses.*

ARGUMENTS—Horary motion of the Moon from the Sun at the top, and the Moon's distance from the Node at the right and left.

N. A.	S. D.	.44	.46	.48	.50	.52	.54	.56	.58	.60	N. D.	S. A.
0°	180°	5°47'	5°46'	5°45'	5°44'	5°43'	5°42'	5°41'	5°40'	5°39'	180°	360°
3	183	5 46	5 45	5 44	5 43	5 42	5 41	5 40	5 40	5 39	177	357
6	186	5 45	5 44	5 43	5 42	5 41	5 40	5 39	5 39	5 38	174	354
9	189	5 42	5 41	5 40	5 39	5 38	5 38	5 37	5 36	5 35	171	351
12	192	5 39	5 38	5 37	5 36	5 35	5 34	5 34	5 33	5 32	168	348
15	195	5 35	5 34	5 33	5 32	5 31	5 30	5 30	5 29	5 28	165	345

The angle of the moon's path with the ecliptic, which depends upon its distance from the node, is apparently increased by the earth's motion in the same direction. This Table gives the apparent angle, taking both these facts into consideration.



## TABLE XXVI.

*The Sun's Declination.*

ARGUMENT—Sun's Longitude.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
N. S.	0.0000	0.3980	0.7961	1.1939	1.5911	1.9883	2.3850	2.7809	3.1761	3.5703	3.9639	S. N.
0 18	0.0000	0.3980	0.7961	1.1939	1.5911	1.9883	2.3850	2.7809	3.1761	3.5703	3.9639	35 17
1 19	3.9639	4.3564	4.7477	5.1380	5.5267	5.9142	6.3000	6.6839	7.0664	7.4472	7.8259	34 16
2 20	7.8259	8.2025	8.5767	8.9489	9.3189	9.6858	10.0506	10.4128	10.7720	11.1284	11.4817	33 15
3 21	11.4817	11.8317	12.1789	12.5225	12.8628	13.1997	13.5328	13.8622	14.1877	14.5092	14.8270	32 14
4 22	14.8270	15.1402	15.4494	15.7539	16.0542	16.3500	16.6408	16.9273	17.2086	17.4850	17.7561	31 13
5 23	17.7561	18.0223	18.2833	18.5386	18.7886	19.0331	19.2717	19.5042	19.7314	19.9528	20.1681	30 12
6 24	20.1681	20.3770	20.5795	20.7761	20.9664	21.1500	21.3267	21.4975	21.6611	21.8186	21.9689	29 11
7 25	21.9689	22.1122	22.2486	22.3781	22.5003	22.6153	22.7234	22.8242	22.9178	23.0042	23.0831	28 10
8 26	23.0831	23.1544	23.2184	23.2750	23.3242	23.3658	23.3997	23.4261	23.4450	23.4564	23.4603	27 9
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

The plane of the ecliptic not coinciding with that of the equator, the sun is sometimes north of the equator and sometimes south. This is called its declination, and this Table shows its amount for every degree of longitude. The epoch of the Table is 1840.

## TABLE XXVII.

*1st Preliminary Equation.*

ARGUMENT—Moon's Anomaly.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	+
0 0 0	0 9 34	0 19 8	0 28 41	0 38 13	0 47 44	0 57 13	1 6 41	1 16 7	1 25 31	1 34 54	1 44 35	35
1 1 34 54	1 44 16	1 53 36	2 2 53	2 12 8	2 21 19	2 30 28	2 39 34	2 48 39	2 57 43	3 6 45	3 16 31	34
2 3 6 45	3 15 44	3 24 42	3 33 38	3 42 32	3 51 23	4 0 7	4 8 47	4 17 25	4 26 1	4 34 33	4 43 33	33
3 4 34 33	4 43 2	4 51 15	5 59 42	5 7 56	5 16 5	5 24 9	5 32 9	5 40 4	5 47 54	5 55 38	6 3 32	32
4 5 55 38	6 3 16	6 10 49	6 18 18	6 25 40	6 32 56	6 40 6	6 47 6	6 54 8	7 1 27	7 50 31	7 58 31	31
5 7 7 50	7 14 30	7 21 27	7 27 22	7 33 36	7 39 46	7 45 46	7 51 33	7 57 23	8 3 12	8 59 30	9 6 29	30
6 8 8 59	8 14 33	8 20 18	8 25 44	8 31 0	8 36 6	8 41 2	8 45 48	8 50 24	8 54 50	8 58 6	9 2	29
7 8 58 6	9 3 13	9 7 9	9 10 54	9 14 28	9 17 51	9 21 3	9 24 49	9 26 54	9 29 33	9 32 1	9 34 128	28
8 9 32 19	9 31 18	9 36 24	9 38 19	9 40 3	9 41 36	9 42 59	9 44 11	9 45 12	9 46 3	9 46 44	9 47 26	27
9 9 46 44	9 47 14	9 47 33	9 47 46	9 47 54	9 47 49	9 47 36	9 47 13	9 46 38	9 45 52	9 44 53	9 43 27	26
10 9 44 53	9 43 42	9 42 21	9 40 51	9 39 3	9 37 14	9 35 12	9 32 58	9 30 32	9 27 58	9 25 12	9 22 25	25
11 9 25 12	9 22 14	9 19 5	9 15 43	9 12 9	9 8 25	9 4 31	9 0 25	8 56 10	8 51 45	8 47 8	8 42 24	24
12 8 47 8	8 42 18	8 37 19	8 32 11	8 26 53	8 21 24	8 15 46	8 9 57	8 3 56	7 57 45	7 51 24	7 45 23	23
13 7 51 24	7 44 51	7 38 9	7 31 18	7 24 10	7 17 9	7 9 52	7 2 24	6 54 46	6 47 0	6 39 42	6 32 22	22
14 6 39 4	6 30 57	6 22 41	6 14 19	6 5 51	5 57 17	5 48 37	5 39 51	5 30 57	5 21 56	5 12 48	5 4 21	21
15 5 12 48	5 3 33	4 54 11	4 44 42	4 35 6	4 25 20	4 15 26	4 5 26	3 55 21	3 45 11	3 34 58	3 24 19	19
16 3 31 58	3 24 42	3 14 24	3 4 3	3 53 38	2 43 9	2 32 34	2 21 54	2 11 10	2 0 23	1 49 33	1 38 19	18
17 1 49 33	1 38 40	1 27 44	1 16 46	1 5 48	0 54 50	0 43 52	0 32 54	0 21 56	0 10 58	0 0 0	0 0 18	18
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

When the moon's anomaly is less than  $180^\circ$ , it is in advance of its mean place at time of new or full moon by reason of the Equation of the Centre, but behind it by Evection, (Tables 9 and 14,) yet on the whole it is in advance; consequently it will overtake the sun sooner than it would otherwise do, and something must be subtracted from the mean time. The contrary takes place when the anomaly is more than  $180^\circ$ . This Table shows the amount of time to be added or subtracted from these causes.

## TABLE XXVIII.

*2d Preliminary Equation.*

ARGUMENT—Sun's Anomaly.

Arg.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	Arg.
+	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	—
0	0 0 0	0 4 29	0 8 56	0 13 23	0 17 50	0 22 17	0 26 44	0 31 10	0 35 36	0 40 2	0 44 28	35
1	0 44 28	0 48 52	0 53 13	0 57 36	1 1 56	1 6 15	1 10 13	1 14 49	1 19 5	1 23 19	1 27 31	34
2	1 27 31	1 31 41	1 35 49	1 39 56	1 44 1	1 48 4	1 52 6	1 56 52	0 12 3	55 2	7 45 33	33
3	2 7 45	2 11 35	2 15 20	2 19 5	2 22 47	2 26 26	2 30 2	2 33 35	2 37 6	2 40 33	2 43 57	32
4	2 43 57	2 47 18	2 50 36	2 53 49	2 57 0	3 0 7	3 3 10	3 6 10	3 9 6	3 11 59	3 14 49	31
5	3 14 49	3 17 35	3 20 20	3 23 0	3 25 35	3 28 5	3 30 30	3 32 50	3 35 6	3 37 19	3 39 30	30
6	3 39 30	3 41 40	3 43 45	3 45 44	3 47 38	3 49 26	3 51 9	3 52 49	3 54 26	3 55 59	3 57 27	29
7	3 57 27	3 58 52	4 0 12	4 1 26	4 2 35	4 3 40	4 4 41	4 5 37	4 6 29	4 7 16	4 7 59	28
8	4 7 59	4 8 37	4 9 10	4 9 39	4 10 4	4 10 24	4 10 39	4 10 49	4 10 54	4 10 57	4 10 53	27
9	4 10 53	4 10 45	4 10 33	4 10 16	4 9 55	4 9 29	4 8 57	4 8 21	4 7 41	4 6 58	4 6 10	26
10	4 6 10	4 5 18	4 4 22	4 3 23	4 2 18	4 1 7	3 59 49	3 58 27	3 57 2	3 55 35	3 54 4	25
11	3 54 4	3 52 29	3 50 50	3 49 7	3 47 17	3 45 25	3 43 26	3 41 23	3 39 18	3 37 10	3 35 0	24
12	3 35 0	3 32 45	3 30 26	3 28 3	3 25 36	3 23 5	3 20 30	3 17 51	3 15 9	3 12 24	3 9 36	23
13	3 9 36	3 6 45	3 3 51	3 0 54	2 57 53	2 54 48	2 51 40	2 48 30	2 45 18	2 42 3	2 38 44	22
14	2 38 44	2 35 22	2 31 57	2 28 29	2 25 9	2 21 27	2 17 52	2 14 14	2 10 36	2 6 55	2 3 12	21
15	2 3 12	1 59 26	1 55 37	1 51 46	1 47 54	1 44 1	1 40 6	1 36 10	1 32 12	1 28 12	1 24 10	20
16	1 24 10	1 20 6	1 16 0	1 11 53	1 7 45	1 3 56	0 59 27	0 55 17	0 51 4	0 46 52	0 42 39	19
17	0 42 39	0 38 26	0 34 11	0 28 55	0 25 39	0 21 24	0 17 8	0 12 51	0 8 35	0 4 0	0 0 18	18
Arg.	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	Arg.

When the sun's anomaly is less than  $180^\circ$ , it is before and the moon behind the mean place, by reason of the Equation of the Centre (Table 8) of the former, and the Annual Equation of the Longitude (Table 10) of the latter. For both reasons, then, the moon will not overtake the sun so soon as it would otherwise do, and consequently something must be added to the mean time of New or Full Moon. The contrary takes place when the anomaly is more than  $180^\circ$ ; and this Table shows the amount of time to be added or subtracted from these causes.

## TABLE XXIX.

*Augmentation of the Moon's Semi-diameter.*

ARGUMENT—Distance of the place (as projected on the disc) from the earth's centre.

Arg.	
+	
0	.0045
10	.0045
20	.0044
30	.0043
40	.0041
50	.0038
60	.0035
70	.0031
80	.0024
90	.0015
100	.0000

Tables 21 and 22 show us the apparent semi-diameter of the moon as viewed from the centre of the earth; but the distance of the moon from any place on the earth's surface at which it is visible (save when it is in the horizon) is less than from the centre, which must cause it to subtend a greater angle. This Table shows the augmentation in the moon's apparent semi-diameter from this cause.

## TABLE XXX.

*To convert minutes into decimals of a degree.*

ARGUMENT—The number of minutes.

Arg.	0'	1'	2'	3'	4'	5'	6'	7'	8'	9'
0	.0000	.0167	.0333	.0500	.0667	.0833	.1000	.1167	.1333	.1500
1	.1667	.1833	.2000	.2167	.2333	.2500	.2667	.2833	.3000	.3167
2	.3333	.3500	.3667	.3833	.4000	.4167	.4333	.4500	.4667	.4833
3	.5000	.5167	.5333	.5500	.5667	.5833	.6000	.6167	.6333	.6500
4	.6667	.6833	.7000	.7167	.7333	.7500	.7667	.7833	.8000	.8167
5	.8333	.8500	.8667	.8833	.9000	.9167	.9333	.9500	.9667	.9833

## TABLE XXXI.

*To convert seconds into decimals of a degree.*

ARGUMENT—The number of seconds.

Arg.	0''	1''	2''	3''	4''	5''	6''	7''	8''	9''
0	.0000	.0003	.0006	.0008	.0011	.0014	.0017	.0019	.0022	.0025
1	.0028	.0031	.0033	.0036	.0039	.0042	.0044	.0047	.0050	.0053
2	.0056	.0058	.0061	.0064	.0067	.0069	.0072	.0075	.0078	.0081
3	.0083	.0086	.0089	.0092	.0094	.0097	.0100	.0103	.0106	.0108
4	.0111	.0114	.0117	.0119	.0122	.0125	.0128	.0131	.0133	.0136
5	.0139	.0142	.0144	.0147	.0150	.0153	.0156	.0158	.0161	.0164



AN

ESSAY

ON

INTESTINAL AUSCULTATION.

BY CHARLES HOOKER, M.D.

PROFESSOR OF ANATOMY AND PHYSIOLOGY IN YALE COLLEGE.

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READ AT THE ANNUAL NEW HAVEN COUNTY MEETING OF THE CONNECTICUT MEDICAL SOCIETY, APRIL 8, 1847.

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## AN ESSAY ON INTESTINAL AUSCULTATION.

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THE object of the following essay is to draw attention to an application of the art of auscultation hitherto neglected—the auscultation of the sounds produced in the intestinal canal. The cavity of the stomach and intestines, both in health and disease, contains, together with solid and liquid matters, a considerable quantity of aeriform substances. This is shown by examination after death, when air is invariably found in the intestinal canal, and may also be rendered evident, at any time during life, by percussion. These aeriform substances consist of common air, hydrogen and its different compounds, carbonic acid, and various other gases, in variable quantities and proportions in different subjects and in different conditions of the body.

The peristaltic action, which is constant in health and is commonly continued in disease, necessarily produces motions of the solid, liquid and gaseous contents of the intestines; and from the known laws of acoustics it might be philosophically inferred that these motions would be productive of sound. These sounds are sometimes audible at a distance from the body, and are noticed, under the term *borborygmi*, as a symptom in various diseases. As the quantity and proportions of the liquid and gaseous contents of the intestines are known to vary, and the peristaltic action to be variously modified, by the changes of disease, it might reasonably be presumed that the sounds produced within the intestines would be subject to corresponding variations; and it is not unphilosophical to suppose that these varieties of sound may afford valuable practical indications.

It is remarkable that a celebrated English philosopher, who was not a medical man, directed attention to this subject, many years before the discovery of the art of auscultation by Laennec. Hook, in his posthumous works, says, “There may be a possibility of discovering the internal motions and actions of bodies by the sound they make. Who knows but that, as in a watch we may hear the beating of the balance and the



running of the wheels, and the striking of the hammers, and the grating of the teeth, and multitudes of other noises ; who knows, I say, but that it may be possible to discover the motions of internal parts of bodies, whether animal, vegetable or mineral, by the sound they make ; that one may discover the works performed in the several offices and shops of a man's body, and thereby discover what engine is out of order, what works are going on at several times, and lie still at others, and the like ? ” “ I have this encouragement ”....“ from experience, that I have been able to hear very plainly the beating of a man's heart ; and *it is common to hear the motion of the wind to and fro in the guts* and other small vessels ; the stopping in the lungs is easily discovered by the wheezing.” The prediction of this philosopher, who, as Dr. Elliotson observes, seems almost to have prophesied the stethoscope, has been fully verified in reference to the thoracic viscera and the gravid uterus ; but to this time it has been strangely neglected in the investigation of the condition and action of the intestinal canal.

It is now more than twenty years since I have habitually attended to the sounds produced in the abdomen in various diseases ; and in the early stage of my investigations I indulged the hope, that in disorders of the intestinal canal auscultation might gain nearly the same distinctness and precision, that it had already acquired in relation to thoracic diseases. Though I long ago relinquished this sanguine expectation, continued observation has confirmed my opinion of the importance of the subject, and has enabled me to discover practical indications, which I regard as of great value.

When the ear is applied to the abdominal parietes of a healthy subject, there is heard an almost constant succession of sounds produced by the motion of the contents of the intestinal canal. These sounds are varied by many causes, such as the quickness, regularity, and other variations of the peristaltic action, the degree of fulness of the intestines, the proportions of the gaseous and other contents, the fluidity of the liquid contents, &c. The sounds, thus varying with the causes of their production, afford indication of these several causes ; and they thus become signs of actions and conditions of the intestines, a knowledge of which is of the utmost importance in investigating the diseases of these viscera. In most diseases of the intestinal canal the sounds do not afford definite diagnostic signs to characterize the different diseases, like the diagnostic signs disclosed by auscultation in thoracic diseases. They are chiefly signs of particular conditions or actions, which may occur in various intestinal diseases, rather than diagnostic signs to distinguish different dis-

eases. In some diseases, however, signs are thus obtained, which perhaps may be considered as truly diagnostic of the diseases in which they occur.

In the *Asiatic Cholera*, which prevailed in New Haven in 1832, this application of auscultation was attended with interesting results, which were noticed in an account of the cases which came under my observation, published in the Boston Medical and Surgical Journal for July, 1833. Writers generally noticed the loud borborygmi, audible at a distance from the patient, which occurred in that disease; and to the ear applied over the abdomen the sounds were so peculiar—at least so different from what I have observed in other diseases—that they seemed distinctly characteristic of that disease. These sounds manifested a rapid commotion of the whole intestinal canal, and might be compared to those produced by shaking together several flasks of various sizes partly filled with water. Frequently the sounds appeared to indicate that the rapid peristaltic motions were suddenly arrested and reversed by an anti-peristaltic action, which occurrence immediately preceded a paroxysm of vomiting. The large quantity of serum effused into the intestines, causing an extreme fluidity of their contents, with the rapid and irregular peristaltic and anti-peristaltic motions, would sufficiently account for this unusual variety of sounds.\*

The effects of various remedies upon the intestinal action, as indicated by the sounds, were carefully observed. Practitioners were generally disappointed, in that disease, to find the frequent vomiting and purging not checked by the administration of stimulants and astringents; and the sounds manifestly indicated that the common effect of these remedies was decidedly to increase the intestinal commotion. Such was the manifest effect of opium, unless given, in doses so large as to produce alarming prostration. On the contrary, frequent small doses of camphor, with a free administration of ice, appeared to have a soothing operation in moderating the rapid and irregular intestinal action. The comparative effects of large and small doses of calomel were strikingly interesting. Frequent small doses did not seem to diminish, but at least temporarily to increase, the disordered peristaltic and anti-peristaltic motions; while a single drachm dose almost invariably caused a total suspension of these motions. Calomel, in very large doses, thus seemed to be the

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\* It remains to be shown, whether these sounds are constant diagnostic signs of this disease, or whether, as I have observed in dysentery and other diseases, the varying epidemic type, in different seasons, will produce in cholera a variation of morbid intestinal action, with a corresponding variety of sounds.

appropriate remedy for the disease. It appeared to overpower the diseased intestinal action, arrested the vomiting and purging, and caused a total suspension of all intestinal motion, during which no sound was audible. An interval of perfect intestinal silence and repose now continued, ordinarily from eight to twelve hours, after which a natural peristaltic murmur indicated a gradual return of healthy action, which was in time succeeded by the grass-green evacuations, commonly regarded as evidence of a favorable crisis of the disease. Thus the large doses of calomel, instead of exhausting the system by an excessive cathartic operation, actually obviated exhaustion by arresting the profuse serous evacuations attending the disease.

Ordinarily the danger was considered as overcome, when the disordered intestinal action was suspended, and the stage of repose produced; and in this town few cases terminated fatally, when the practice was adopted of effecting this result by the large doses of calomel, before the system had been extremely exhausted by evacuations. In one case, however, that of a little girl, 10 years of age, who, without any premonitory symptoms, was most violently attacked with vomiting, purging and spasms, this treatment had the ordinary effect of promptly arresting the intestinal motions; but the system did not re-act, the pulse failed and became imperceptible within an hour from the attack, the coldness and lividity of the surface increased, and, without any return of peristaltic action, the patient died five hours from the attack.

*Cholera Morbus* is usually attended with intestinal sounds, which indicate a succession of quick and irregular peristaltic and anti-peristaltic motions. In some cases these motions continue until the contractile power of the intestines seems nearly exhausted, when a feeble, but more regular, peristaltic murmur indicates a gradual return of healthy action. The violent symptoms are not succeeded, as in Asiatic cholera, by a long interval of total inaction of the intestines; and the sounds are very different from those heard in that disease.

There is, however, a great diversity in cases commonly termed cholera morbus. Some cases commence with a violent diarrhœa, on the cessation of which occurs an obstinate vomiting, during which, as in colic, no intestinal sounds are heard, except those produced by anti-peristaltic action. Other cases commence with vomiting, without any downward motions, until at length the action is reversed, and the disease terminates with diarrhœa.

*Colic* is a disease which is variously divided by writers into several species. One of these, termed *C. rachialgia*, *C. pictonum*, &c., produced



by the poison of lead, has characteristics certainly sufficient to give it a specific distinction ; but the other divisions, I think, have reference to various exciting causes, or attendant circumstances, rather than to any proper specific characters. In the various forms of this disease auscultation affords results, which I regard as highly interesting, and of much practical value, and which may throw some light on the pathology of the disease.

*Common Colic* is characterized by “gripping pain in the bowels, chiefly about the navel, with vomiting and costiveness.” The exciting causes are various, as irritating indigested food, biliary derangement, habitual costiveness, hardened fæces, flatus, worms, exposure to cold, and—what I consider as much the most common cause—rheumatism affecting the intestines. With these various exciting causes, the general characters of the disease are similar ; the severe gripping pain, obstinate constipation and vomiting, constituting the prominent symptoms.

There is, however, an *incipient, forming, or latent stage*, which with strict observation I think may always be noticed, preceding the pain and other violent symptoms. The symptoms of this stage somewhat resemble those which precede the cold stage of intermittent fever. There is a general languor and lassitude, often a degree of moroseness or peevishness, and commonly a slight chilliness. The sensations in the abdomen are variously described by patients, as a numb, dead, heavy, or cold feeling. Many speak of a sensation, as of a cold weight, felt mostly between the region of the stomach and umbilicus. The physician is rarely consulted during this stage ; and the symptoms are so slight, that ordinarily they are not particularly noticed by patients unaccustomed to attacks of the disease ; while persons subject to frequent attacks learn to notice these sensations, as the invariable precursors of the more violent symptoms.

In this stage, which continues in different cases from half an hour to several hours, auscultation discovers a perfect stillness within the abdominal cavity. Sometimes there is an occasional rumbling in the course of the large intestines ; and, with a desire to relieve the unpleasant sensations, the patient, by a voluntary straining effort, produces an evacuation of fæces with a quantity of flatus. There is, however, no indication of the slightest motion in the small intestines. This forming or latent stage of colic, which is commonly overlooked both by patients and physicians, is deserving of particular attention ; because during this stage the peristaltic action is easily restored, and the violent symptoms thus prevented. In many cases this may be effected simply by the

application of heat to the surface, especially to the extremities. Friction to the abdomen, with a sort of kneading process, contributes also to this effect. Often a free draught of hot coffee, or of some aromatic infusion, is sufficient; in other cases, a small dose of rhubarb, or other mild cathartic, with some aromatic, is required. Commonly, a few drops of cajeput oil will promptly restore the peristaltic action. My usual remedy for this purpose is camphor, in frequent small doses; and I have instructed many persons to ward off habitual attacks of colic, by carrying constantly in the pocket a small piece of camphor, to be gradually dissolved in the mouth, and swallowed with the saliva, whenever these premonitory symptoms occur. This remedy is often more effectual, in exciting peristaltic action in such cases, than a brisk cathartic.

This forming stage, unless the peristaltic action is soon restored, is succeeded by the violent symptoms of the disease. With occasional short remissions, the pain becomes severe; the abdominal muscles are rigidly contracted, producing a knotted appearance of the surface, and there is occasional nausea and vomiting. The patient groans, and throws himself into various positions, with the vain hope of relieving his distress. In this, as well as in the forming stage of colic, the ear applied to the abdomen discovers no evidence of peristaltic action, but on the contrary a perfect stillness within the abdominal cavity.

This *cessation of peristaltic action*, I may confidently assert, is a chief *essential character of colic*; the motion being suspended before the occurrence of the violent symptoms, and not recurring until the disease is about to yield. Sometimes, during the violent contortions of the body, a momentary sound is heard, indicating a slight intestinal motion, which seems to be produced by the mechanical pressure of the abdominal parietes, rather than by a peristaltic action. Occasionally, too, there are sounds produced by anti-peristaltic motions, which motions either terminate at the stomach causing simply nausea, or extend into the stomach so as to excite vomiting. By these circumstances, and by the variety of sounds, anti-peristaltic motions can commonly be distinguished from a regular peristaltic action. This distinction is important, for as a cessation of peristaltic action is a main essential character of colic, so a return of this action indicates a favorable crisis of the disease. The sounds produced by anti-peristaltic motions are only occasional and transient, proceeding commonly from a limited portion of the intestinal canal; and they are usually succeeded, as before stated, by nausea or vomiting. Those attending a regular peristaltic action are produced throughout the whole course of the intestines, constituting an almost in-

cessant rumbling, heard distinctly at one moment directly under the ear, then gradually receding until it seems like a distant echo, and again returning in the course of the convolutions of the intestines. There is thus a union of near and distant sounds, indicating a general action throughout the intestinal canal. When this description of sounds is heard in colic, the patient may be considered as safe, even if the pain continues severe; on the contrary, a complete subsidence of the pain and other violent symptoms, unless attended by a return of healthy peristaltic murmur, affords no favorable indication, in any stage of the disease, and in an advanced stage, when the strength is exhausted by protracted suffering, it indicates extreme danger—a loss of the sensibility and excitability of the intestines, and a failing of the powers of life.

Commonly, a return of peristaltic motion is followed, almost immediately, with a relief of pain and other severe symptoms; but in protracted cases, when the bowels have become inflamed, and the soreness such that the least external pressure cannot be tolerated, this return of peristaltic motion causes a decided increase of pain. This circumstance is similar to what is often observed during the resolution of pneumonia, when a return of respiration to a portion of inflamed lung, which has previously been impermeable to air, produces the keenest pain. In such cases auscultation informs us that all is well, when the sensations of the patient would indicate an aggravation of the disease. The signs thus furnished, in colic and other diseases, will often direct the withholding of medication, when it is no longer required, and when its continuance might sometimes be injurious. Frequently they have enabled me to assure patients that the cause of difficulty was removed, and that my services were no longer required, some hours before the general symptoms showed signs of any mitigation.

Some eighteen years ago, I called one morning to see an eminent medical friend, who had been subject to frequent attacks of colic, and who was now thought to be dying, after a night of extreme suffering with this disease. Applying my ear to the abdomen, I immediately assured him that a regular peristaltic action was restored, and that the danger was over. He replied that he experienced no relief of symptoms, saw little reason for encouragement, and felt as though he could survive but a short time. The pain was now severe and increasing; and it was more than two hours from this time before the apprehensions of the patient, and of his friends generally, were at all relieved. In this case, as in many others that I have observed, it was full three hours, after my



confident assurance that the disease had made a favorable crisis, before there was any evacuation from the bowels.

In March, 1847, I was called at night to a man affected with colic. After the usual precursory symptoms, he had now, for about two hours, suffered severe pain, which had suddenly increased within the last few minutes, so that he could hardly be confined to the bed. The application of the ear discovered a regular active peristaltic motion. I concluded that this was a case, such as are sometimes observed, of a spontaneous restoration of peristaltic motion, and that this returning motion had caused the present sudden increase of pain. As the pain had not continued long enough to induce any considerable inflammation or soreness of the intestines, I did not hesitate to assure him, that if he would keep warm in bed, the pain would soon subside. I remained with him about thirty minutes, and left him in a quiet sleep—not taking to myself the credit of a cure, as I might have done, had not auscultation informed me that a spontaneous natural action had rendered medication unnecessary.

Pathologists entertain different opinions in reference to the immediate cause of colic. Some, with Cullen, consider the symptoms as owing to a spasmodic constriction of the intestines; while Abercrombie and others attribute the inaction of the intestines to torpor, or a loss of their muscular power. I am inclined to the latter opinion—to attribute the inaction of the intestines to a suspension of the motor nervous influence, and the supervening pain to a morbid excitement of the sensitive nerves. Such an association of paralysis of motion, with morbid sensibility, is not uncommon in other parts of the body. Paralysis of the limbs is often attended with paroxysms of severe pain: and in a painful sciatica, and in neuralgia of various parts of the system, the motor nervous influence is usually more or less diminished.

It is well known, however, that all the symptoms of colic are produced by any obstruction which mechanically arrests the motion through any portion of the intestines; as in strangulated hernia, involution of the intestines, and in cases of obstruction from impacted fæces, calculi, or any solid substances in the intestinal canal. It is remarkable, moreover, that the same results are caused by sympathy of the intestines with obstruction in other parts; as a foreign substance in the *cul-de-sac* of the appendix vermiformis, calculi in the ureters, gall-stones in the biliary ducts, and also in severe cases of dysmenorrhœa.

In all these affections, in some stages of the complaints at least, there is the same total suspension of peristaltic motion, which occurs in colic.

When the mechanical obstruction is obviated in these complaints, as in the relief of the strangulation in hernia, it is commonly observed that the relief is immediately succeeded by a rumbling sound in the intestines, which is usually followed by alvine evacuations.

It may be difficult to explain how these various mechanical obstructions should cause a suspension of the motor nervous influence in the intestines; but from my observations I may assert that there is commonly (I would not say invariably) the same numb, heavy sensation, preceding the pain and other violent symptoms, that I have described as constituting the forming stage of colic.

*Colica Rachialgia (Lead Colic)* may be considered as specifically different from common colic. Its cause, its symptoms, and its obstinate character, sufficiently distinguish it. Auscultation, also, in the course of the disease, discovers signs, which are distinctly characteristic. There is not, indeed, any particular sign, which, like the crepitation characteristic of pneumonia, the moment it is heard, decides the character of the disease; but the auscultator has to notice a succession of various signs, which are severally indefinite and insignificant, from the assemblage of which he will form his diagnosis.

The peristaltic action is wholly or partially suspended for a longer period, and is re-excited with more difficulty, than in common colic. Some cases I have closely watched, for two, three and even six days, without observing the least sound of peristaltic motion. More commonly, however, the stillness of the intestinal canal is occasionally broken, for a few moments, by a dull rumbling sound, indicating a slight and sluggish action in a limited portion of intestine. Often there is a transient blowing or sub-whistling sound, seemingly produced by wind crowded through a contracted portion of intestine. During the whole disease, all the intestinal motions appear weak, slow and sluggish; the sounds not having the suddenness and regular succession of healthy peristaltic action. Indeed, for weeks or months after convalescence, ordinarily the sedative influence of the lead appears to continue; the sounds indicating a torpid intestinal action, and regular evacuations being procured with difficulty.

This disease does not, like common colic, form a crisis by a sudden transition from total inaction to a general peristaltic motion, which terminates the disease. But in lead colic, sometimes for days before there is any decided improvement, an occasional rumbling is heard, indicating a considerable intestinal action. Again—perhaps within an hour, or on the succeeding day—we find all silent; and for several successive days, these intervals of inaction may alternate with efforts of the intestines to

re-establish peristaltic motion. From not being aware of this circumstance, in my early observations, I sometimes concluded that a favorable crisis had occurred, long before the disease was subdued.

In this disease, as in common colic, sounds frequently occur, produced by anti-peristaltic motions, which the practitioner should not mistake for regular peristaltic action.

*In dysentery* there are no characteristic sounds, attending the disordered intestinal action, sufficient to distinguish it from other diseases. The general character of this disease varies much in different seasons; and in particular cases, there are many variations of disordered action, in its different stages. These variations, however, are productive of signs, which, after a little observation of the epidemic tendencies, the auscultator may learn to improve, in watching the changes of symptoms, and in directing his treatment.

Though no constant rules can be given, for the sounds to be expected in the course of this disease, it is useful to watch by auscultation the changes which occur in different portions of the intestinal canal. Commonly, though the disease is seated chiefly in the large intestines, the small intestines are more or less affected, their peristaltic action being irregular—sometimes morbidly increased, but more commonly diminished, and sometimes wholly suspended. Attention to the signs furnished by auscultation will often enable a practitioner to avoid being taken by surprise, by the changes which frequently occur in the course of this disease.

*The proper management of cathartic medicines* is one of the most important, and often one of the most difficult, subjects in medical practice. The indications and contra-indications, for the use of this class of remedies, are often obscure; and in the course of almost every case of fever, and other dangerous disease, the practitioner will frequently on this subject find himself in doubt. It is obvious that a correct knowledge of the condition and action of the intestinal canal must essentially aid to render these indications plain. Hence every available means of acquiring such knowledge is calculated to aid the practitioner's decision in frequent cases of doubt and uncertainty. It is in this point of view, perhaps more than any other, that the exploration of the intestines by auscultation must become a valuable auxiliary to the healing art. Much information is thus afforded, in regard to the action and the contents of the intestines—circumstances which are obviously important in determining the indications for cathartics.

In many cases the practitioner is importuned by patients, or their



attendants, for the employment of cathartics, when the application of the ear would give assurance that spontaneous evacuations will soon occur. So when cathartics have been administered, we have a pretty sure criterion, in the signs furnished by auscultation, of the operation which they are likely to effect. The practitioner is thus guarded against over-dosing with cathartics, and thereby causing exhausting evacuations, which might be very injurious in a debilitated condition of the system.

It would be superfluous to attempt a description of the sounds which afford these indications, as a little observation renders obvious the sounds caused by a rapid peristaltic action briskly agitating the liquid contents of the intestines.

In cases of *diarrhœa* this method of exploration is valuable in forming an estimate of the severity and obstinacy of the complaint. In many cases, moreover, in the progress of fevers, and other diseases, the intestinal sounds will give the practitioner timely warning of the approach of this complaint, long before the occurrence of any evacuations; just as in pneumonia the sign of crepitation reveals the character of the disease, sometimes long before the appearance of the characteristic bloody sputa. The occurrence of a diarrhœa being thus anticipated, it may often be prevented by timely medication.

In other cases, when the occurrence of frequent copious evacuations might otherwise cause serious apprehension, we can by this means obtain immediate assurance that there is no danger. A single case is subjoined, as an instance of the satisfactory information frequently afforded me in such cases.

Some years since, I was called in haste, in the absence of the attending physician, to visit an aged woman, who in the course of a fever had been suddenly attacked with diarrhœa. The evacuations had caused extreme exhaustion, with faintness; and the patient and her friends were much alarmed. Applying the ear over the abdomen, I decided unhesitatingly that the diarrhœa would give no further trouble; but in reply I was told that for two hours the evacuations had been copious and frequent, the last only a few minutes previous; and it was urged that immediate remedies must be applied for arresting the complaint. I persisted in my decision, and did nothing except to quiet the alarm with my assurances that the diarrhœa was suspended. It was now early in the afternoon; and I afterwards learned, from the attending physician, that there was no subsequent evacuation until the next morning.

The method of exploration, recommended in this essay, may be practiced, either by the direct application of the ear, or through the medium

of the stethoscope. The stethoscope is advantageously used, when it is desired to discover the sounds originating in any definite region of the intestinal canal; but for most purposes the direct application of the ear is preferable.

It is an advantage of the latter method, that ordinarily it does not require the bed-clothing of the patient to be removed; as the sounds, which are the object of investigation, may commonly be heard, with sufficient clearness, through several thicknesses of clothing—the ear being applied, with moderate pressure, over the abdomen, while the patient is lying on the back. In no case, whether the immediate or mediate method is adopted, is it required to entirely uncover the abdomen.

The immediate method has another advantage, that it not only discovers the sounds originating directly under the ear, but at the same time notices those produced in more distant portions of the intestines. It thus takes a general observation of the condition and action of the intestinal canal, more fully than can be done with the stethoscope.

In conclusion, I would remark, that the purpose, of bringing to the notice of the medical profession the subject of intestinal auscultation, has been delayed for many years, with the hope of being able to give the subject a more satisfactory degree of precision and system. The importance of the subject, it will be understood, is claimed, not so much from its affording signs to characterize and distinguish different diseases, as from its giving indications of particular morbid conditions and actions of the intestines, which may occur in various diseases. The difficulty, which has been my chief source of discouragement and delay, has been the impossibility of giving a satisfactory description of the sounds affording these indications. To describe sounds, by language, is ordinarily a difficult matter. We recognize at once the voices of familiar acquaintances; but we should fail in any attempt to give a definite description of the sounds of these various voices. So the auscultator may discover variations of sound, produced within the intestinal canal, which a little observation will enable him to recognize, as signs affording clear and valuable indications; but these signs are to be learned by the practitioner's own experience, rather than from any description which the experience of others can furnish.













AN

A D D R E S S

IN COMMEMORATION OF

SEARS COOK WALKER,

DELIVERED BEFORE THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT  
OF SCIENCE,

APRIL 29, 1854.

BY

BENJAMIN APTHORP GOULD, Jr.

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# A D D R E S S .

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MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR  
THE ADVANCEMENT OF SCIENCE : —

THE melancholy duty which you have imposed upon me is one which permits of no hesitation in its fulfilment. And though it is to be wished that the task had devolved upon an abler pen and a more eloquent tongue, your request has carried with it the force of a command, and at your bidding lips already too familiar with the funeral strain are here to utter the voice of our common lamentation and our common eulogy.

It is now more than fifteen months since all that was mortal of Sears Cook Walker was borne to its long home. Twice has his own planet Neptune, fitting emblem, disappeared in that solar effulgence which at once illuminates it and forbids it to our vision ; — twice have the early flowers of spring blossomed above his honored grave. The places that knew him well know him no more, and the turmoil of life, like a great ocean, has rolled over the void that awed us by its depth and darkness. Those who called him their friend need no commemorative word, nor sound of eulogy.



But it is fitting that the votaries of science in America should unite, as such, to perpetuate the remembrance of his good works and pay to his memory the tribute of honor and gratitude which is its due.

No geographical division of our country may claim Walker as its own. Massachusetts was his birthplace; Pennsylvania long his home; this city the scene of his most brilliant achievements. The field of his investigations is bounded by Maine, Carolina, and Missouri, — and his ashes rest upon the banks of the Ohio. Where Science called him was his home; whom Science linked with him were his friends; for her he lived, and for her he died.

He was born on the 28th of March, 1805, in the little village of Wilmington, Mass., where his ancestors had lived and died for four successive generations. His father's mother was descended in a direct line from the celebrated Elder Brewster, who came out in the Mayflower. While yet an infant, he lost his father, but the care and devotion of an excellent mother guarded his childhood and youth, and directed his education with sound judgement and constant watchfulness. The activity and precocity of his intellect were extraordinary, and rendered him, at an early age, the wonder of the village. Few who knew him only in later years would have imagined that his constitution had in childhood been so feeble and delicate as to occasion the most anxious apprehension. Yet this was so, and for a time there was imminent danger that the slight tenement would prove too frail to sustain the restless activity which threatened to carry all before it. His judicious mother perceived the importance of providing first of all for his physical education, and of checking his intense application to books, but it was only with great difficulty that she succeeded. Still she was successful; she suppressed, at least in part, his devotion to study, and he joined for a time with all the zest of other children in the sports of his companions. Under this judicious guidance his health became gradually established, and after

completing the preparatory studies at the Academies of Andover, Tyngsborough, and Billerica, he entered Harvard College in 1821, and graduated, four years later, a classmate of Horatio Greenough, Augustus A. Gould, and Charles H. Davis. While in college he was as conspicuous for his classical as for his mathematical attainments, and here, as throughout his subsequent life, manifested the peculiar fondness and aptitude for the acquisition of languages, and that singularly retentive memory which formed so prominent a feature of his intellectual constitution. For two years after his graduation he remained in the vicinity of Boston as teacher, and in the autumn of 1827 removed to Philadelphia, which continued for nearly twenty years his home and the seat of his constant labors. His was a choice combination of buoyant spirits and cheerful manners with an earnest, absorbing devotion to science, and in the refined, intellectual, and social city of his adoption he found a most congenial atmosphere. In Philadelphia he led the life of a teacher for eight years, and subsequently, from 1836 to 1845, was actuary of the Pennsylvania Company for the Insurance of Lives and Granting Annuities, a position analogous to that held in Boston at the same time by Bowditch.

As a teacher he was singularly gifted in the didactic art, and most successful in imbuing his pupils with zeal for science and letters. Many an ardent lover of nature, many an earnest votary of science, many a refined and elegant scholar, looks back with gratitude, affection, and reverence to those days when it was his privilege to imbibe intellectual refreshment from Walker's never-failing well of thought, and catch the enthusiasm which glowed in the soul of his departed teacher and friend.

In Philadelphia, the engrossing duties of his school, and subsequently of his office, left him comparatively little time for original investigation, even had the condition of the community in America been such as to promote this. Still his ever active intellect was ceaselessly at work, and the relatively

less conspicuous efforts of his mind and pen — combined with that personal influence upon the public which every man of intellect is constantly exerting, whether voluntarily or not — were unquestionably of eminent service in paving the way for future progress, and in laying and cementing the basis on which we are striving to erect the structure of science in America. A physical frame singularly at variance with the restless activity of his intellect and with the fragile constitution of his early youth fettered his energies, and, for the period of uninterrupted prosperity and personal contentment, kept down the soarings of his genius and ambition. Still, even then, intellectual sloth was unknown to him. While engaged with his school he studied medicine, and went through the whole course requisite for the attainment of a degree. He devoted his leisure for a period to the study of Natural History, and was no mean proficient in Geology and Mineralogy, as well as in Physics and Chemistry. He was an active member of the Pennsylvania Geological Society, of the Committee of the Franklin Institute on Science and Art, and one of the most useful members of the American Philosophical Society. By frequent articles upon scientific topics in the various prints, by elaborate reports upon various subjects to the Franklin Institute, and by monthly announcements in its Journal of occultations and other important celestial phenomena, he kept awake the interest and sympathy of the community for studies of this character. Among other labors, he prepared, in 1834, an ingenious set of parallax tables, by which the time required for computing the phases of an occultation was reduced to less than half an hour. These were calculated for the latitude of Philadelphia, and it was his intention to publish them in a more general form adapted to different latitudes. But as this would have been a work requiring considerable time, he subsequently abandoned the project, believing that he could employ his leisure hours more usefully. He continued the computation of the occultations without interruption for six years, and



then induced our well-known colleague, Mr. Downes, to undertake the continuance of the work. It has been prosecuted to the present time, with what success we all know, and has of late years been published by the Smithsonian Institution and the *Astronomical Ephemeris*. Astronomy and Geography in America are much indebted to Mr. Walker for these labors, since many already in possession of the necessary means were stimulated by the periodical announcements, and by his personal exertions in still other ways, direct and indirect, to observe these phenomena. An extensive series of such observations was collected by Mr. Walker, and published in the *Proceedings of the American Philosophical Society*.\*

Though Walker's position at this period was rather that of an amateur than of a scientist, — professed investigators being then with few exceptions unknown, if not unheard of, among us *cis-Atlantic*s, — and though his love for Nature in all her varied manifestations had not yet been concentrated into an absorbing devotion to any one department of science, Astronomy was always his favorite pursuit. He had procured an astronomical clock, a twenty-inch transit-instrument, and a small Dollond telescope, and the use of these gradually became so attractive and acquired such fascination for him, that he surrendered little by little the study of the other natural sciences, and from about 1836, in which year he gave up his school to become Actuary of the Insurance Company, all the leisure hours at his disposal were devoted to astronomical observation and study. In 1837 he was invited to propose a plan for an Observatory in connection with the Philadelphia High School, an invitation which he accepted with eagerness. In accordance with his suggestion, the committee in charge of the school imported from Munich the excellent Fraunhofer equatorial and Ertel meridian-circle, which, in his hands and those of his accomplished brother, the present Director of the Observatory, have done so much for Astrono-

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\* Vol. I. New Series.

my in America, — not merely by the number of observations made with them, but also by the incentive which they afforded to the lovers of Astronomy in other parts of the country. It is unquestionable, that in several instances they induced successful efforts for the procurement of similar, and even superior, apparatus elsewhere. Professor Loomis's Observatory at Hudson, Ohio, was erected at about the same period, and somewhat earlier provided with instruments, and these two were the first institutions in America deserving the name of observatories. The Philadelphia instruments were the first ones imported into this country from Germany, and the ripe judgement manifested by Walker in selecting the artists to whom their construction should be confided has borne abundant and valuable fruit. The constancy and value of his labors at the High-School Observatory, while oppressed by a multitude of other duties, are abundantly attested by the numerous observations and investigations which he published in the Proceedings and Transactions of the American Philosophical Society, and various journals. Besides the articles already referred to, Walker published in 1841 a most remarkable and brilliant paper concerning the periodical meteors of August and November; probably the most valuable memoir which has yet appeared upon that subject. Up to this time he may be looked upon as occupying a prominent position among American amateurs in astronomy. This article on meteors may be considered as first placing him in the ranks of scientific investigators.

As we approach that eventful period in Walker's life which forms in fact the commencement of his real scientific career, in the higher sense, let me beg you to go back with me for about a dozen years, and consider the condition of science in the United States at that time, — say, for instance, in the year 1840.

The memoirs of our learned societies and academies, from the commencement of our national existence up to that epoch, furnished us with small authority for claiming a rank among the scientific nations of the world. Few, very few

investigations of value had been made by Americans in any department of natural science, and what little had been done consisted, with but few, though indeed some brilliant exceptions, either of purely local observations, or of speculations, unrebuked however wild, unappreciated however profound. Our geology, botany, physical constants, even our topography and geodesy, had been examined and studied chiefly by men of foreign birth and education. The learning of the other hemisphere was wrapped in a kind of sacred mystery, deemed impenetrable by a native of these Occidental wilds. The very name of European carried with it a mighty weight of authority, and the favorable mention of an American name in a European print was a passport to that Notoriety which far and wide usurped the influence of Fame. The first lesson of the American scholar was, not self-reliance, but self-distrust, and intellectual timidity took the place of modest diffidence. There were, it is true, exceptions to these remarks, yet such as to render the general truth more conspicuous. I have but to point to the bright names of Hare, Bache, Henry, and Bowditch, and to the earnest devotion of Silliman. Yet it is not long since we have seen the possibility of the labors of these and similar men being set at naught for a while even in their own land, and by their own countrymen, as soon as a little foreign authority was brought to bear against them. Many circumstances tended to promote and protract this state of things. The want of proper libraries, a corresponding disregard of the importance of foreign languages, and a consequent want of acquaintance with the science of continental Europe, except as presented in the garb of British translations and commentaries, all tended to delay the advent of an intellectual independence in any degree commensurate with that political independence which "our fathers fought for and bequeathed," two thirds of a century before. And all this dependence was largely aggravated by the deportment consistently maintained by the mother-country, which, owing in part to the still lingering



influences of colonial subjection and in part to the community of language, alone represented to us the science of the other hemisphere. The best efforts of the few devoted and self-sacrificing American scholars could obtain as their highest reward only some patronizing mention. Fortunate indeed was the inventor whose priority was acknowledged, — happy the experimenter or observer whose labors were noticed, though with disparagement. The combined action of these influences produced a reciprocal effect, which seemed almost to increase the evil already existing. Our ablest mathematician, whose genius would have rendered him a brilliant ornament of any scientific circle, was fully content that the crowning labor of his useful life should be a translation, enriched, it is true, with copious commentary, but a translation of the work of another. The most powerful and acute minds were content to exhaust their strength in the preparation of text-books. You, Mr. President and gentlemen, like all the votaries of science among us, are wont to contemplate with astonishment the remembrance of this period, at once so recent and so remote, — chronologically but as yesterday, and yet effaced from daily memory by the vast distance traversed since then in the onward progress of our nation. Most of the men whom we must thank for all this are still among us, full of honors, though but few of them, thank God, full of years. Walker has gone; fragrant be his memory.

Prominent among the producing causes which were silently but surely preparing the way for that vast step which was to transfer America from the rear to a prominent position in the army of nations combating for truth and the extension of the bounds of human knowledge, must be counted the noble enterprise of that good and venerable man who for so long a time and so ably has given a voice to American investigations in physics and natural history. The professedly scientific institutions of our country issued from time to time, though at considerable intervals, volumes of Transactions and Proceedings unquestionably not without their influence in

keeping alive the scarcely kindled flame, but whose contents, as might be expected, were for the most part rather in conformity with the then existing standard of excellence than in advance of it. Natural History in the United States was the mere sorting of genera and species; the highest requisite for distinction in any physical science was the knowledge of what European students had attained;—Astronomy was in general confined to observations, and those not of the most refined character, and its merely descriptive departments were estimated far more highly than the study of its laws. Astronomical computation had hardly risen above the ciphering out of eclipses and occultations. Indeed, I risk nothing in saying that Astronomy had lost ground in America since those Colonial times when men like Rittenhouse kept up a constant scientific communication with students of Astronomy beyond the seas. And I believe I may farther say, that a single instance of a man's devoting himself to science as the only earthly guide, aim, and object of his life, while unassured of a professor's chair or some analogous appointment, upon which he might depend for subsistence, was utterly unknown.

Such was the state of science in general. In Astronomy the expensive appliances requisite for all observations of the higher class were wanting, and there was not in the United States, with the exception of the Hudson Observatory, to which Professor Loomis devoted such hours as he could spare from his duties in the College, a single establishment provided with the means of making an absolute determination of the place of any celestial body, or even relative determinations at all commensurate in accuracy with the demands of the times. The only instrument that could be thought of for the purpose was the Yale-College telescope, which, although provided with a micrometer, was destitute of the means of identifying comparison-stars. A better idea of American astronomy a dozen years ago can hardly be obtained than by quoting from an article published at that time by the eminent geometer who now retires from the position of President of

this Association. He will forgive me the liberty, for the sake of the illustration. "The impossibility," said he, "of great national progress in Astronomy while the materials are for the most part imported can hardly need to be impressed upon the patrons of science in this country. . . . . And next to the support of observers is the establishment of observatories. Something has been done for this purpose in various parts of the country, and it is earnestly to be hoped that the intimations which we have heard regarding the intentions of government may prove to be well founded;—that we shall soon have a permanent national observatory equal in its appointments to the best furnished ones of Europe; and that American ships will ere long calculate their longitudes and latitudes from an American nautical almanac. That there is on this side of the Atlantic a sufficient capacity for celestial observations is amply attested by the success which has attended the efforts, necessarily humble, which have hitherto been made." \*

All good influences seemed to combine, at about this period, to inaugurate the new era. The discrimination and knowledge of Walker led him, in providing instruments for the High-School Observatory, to discard the class previously used in this country, and to equip the Philadelphia Observatory with the productions of German artists. This example was speedily followed, and within the next five years four noble Fraunhofer refractors were received and mounted in the United States. During these years the energy of our colleagues, Messrs. Bartlett, Gilliss, and Mitchel, equipped the fine Observatories of West Point, Washington, and Cincinnati; and the huge refractor at Cambridge was ordered within the same period. All things betokened the simultaneous devotion of earnest hearts and willing hands, and a concentration of their effort upon the work of advancing Astronomy in the United States; and it was precisely at this period, fortunately for us, and for the renown of our departed colleague, that a crisis in his life occurred.

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\* Peirce, Cambridge Miscellany, April, 1842, p. 25.



A series of unfortunate investments and commercial operations led to most disastrous results, and left Mr. Walker at the age of forty years utterly without means. It was a bitter cup for a man accustomed to the luxuries of life, and all the little comforts of opulence, which none knew how to prize better than he; but that Providence which had prepared for him so severe a trial

“Knew how to bring honey out of the lion.”

Often since that eventful time have I heard him bless the dispensation, which first made manifest to himself and to mankind the rare endowments of his genius and the exhaustless energies of his intellect.

Under these circumstances he was invited by the Secretary of the Navy, in 1845, to take part in the Washington Observatory; an invitation which, it is needless to say, he accepted with alacrity. Here he found himself for the first time in his life surrounded with all the facilities which he desired, and he applied himself with energy to the new field of duty open before him. But circumstances which it is not necessary to detail led him to resign his post at the Observatory to accept the direction of the Longitude department of the Coast-Survey;—an office which he filled until his last illness, with honor to himself and credit to the national service, as well as to American Astronomy.

During the eight years of Walker's residence in Washington, science in America was developed with almost electric rapidity. Scientific institutions and men sprang up, as it were by magic, in every section of the United States. The Coast-Survey under its present honored Chief entered upon new and enlarged activity. The Smithsonian Institution was organized. This Association was formed, and increased life infused into every department of research. It is our high privilege, Mr. President and gentlemen, that our lot has been cast in such a time, and that we have been permitted to act together in behalf of such a cause. It is unquestionable that

the latent energies which imparted this impulse to all scientific investigation, and were prominently manifested in Astronomy, aided to throw the important agency of Walker into bold relief. But it is no less true that he whom we here lament bore an important part in the great work, and that to his boundless enthusiasm, his genius, and his well-directed labors, the nation's warmest gratitude is due.

During these years of his residence in Washington, Mr. Walker made many investigations, and on many subjects; but the first, and that which brought him most prominently before the scientific world, was his series of researches connected with the planet Neptune. The remarkable combination of singular and imposing events attending the discovery of the planet and the determination of its orbit needs no repetition here. On the 2d of February, 1847, but little more than four months after the discovery of Neptune, Mr. Walker, without the aid of charts or telescopes, in the solitude of his chamber, made the discovery that a star observed by Lalande in May, 1795, must have been the same planet. The telescope of yonder Observatory, in the hands of our colleague, Professor Hubbard, confirmed the bold prediction on the first clear evening, and astronomers were thus furnished with an observation of Neptune made fifty-two years before, which afforded the means of a most accurate determination of the orbit. This discovery of Walker's was followed, at an interval of but few weeks, by an independent discovery in Europe of the same important fact by means of a totally different method;—viz. a laborious examination of the heavens through a zone  $6^{\circ}$  broad and  $45^{\circ}$  long, and a careful comparison of this region with the observations of Lalande.

Gentlemen, Astronomy is now also deploring the afflicting bereavement by which we have been deprived of the companionship and aid of that earnest, self-sacrificing, devoted man to whom this European discovery was due,—a man whose singleness of heart was only equaled by his truthfulness and zeal for science. Three months only have elapsed since the

death of Petersen. Forgive me if I pause to ask your tribute to his memory. He was a man whose warm affections and large soul were utterly unknowing of the petty rivalries and dishonoring jealousies which, alas! too often, in all countries and all times, furnish a painful evidence that Science alone, without the influence of some higher guide and aim, is insufficient to elevate her followers above the level of the world's turmoil and passions and cares. He was an ardent friend, a generous colleague, an enthusiastic scientist. Let me apply both to Walker and to Petersen the words which Hansen dedicated to the latter. "Hail to the man who can die as well as live for his vocation! A glorious death is his, and his name shall shine bright in never-dimming luster." Gentlemen, if all ages and realms have joined in echoing the "*Dulce et decorum est pro patria mori*," when that death is amid the maddening shouts of the battle-field, — when the effulgent hopes of victory and tokens of a nation's gratitude and honor blind the sight to the uncertain dangers in the way, — what should be said of the enthusiastic men who calmly and quietly, often, like Petersen and Walker, with all the self-devotion, though without the external encouragement, of a forlorn hope, lay down their lives for science, truth, and their country's fame?

The discovery of the identity of Neptune and the star of Lalande was confirmed in a remarkable manner by the examination of Lalande's manuscripts in Paris, and Walker devoted himself with ardor to the determination of the orbit. Meantime, Peirce had made his investigations of the theory of Neptune, and his discovery of the two solutions of the problem, and of that singular coincidence of direction between the computed position of the hypothetical planet at the time of discovery and the observed position of Neptune, — a coincidence doubtless the most wonderful in the history of astronomy, and not by any means less striking from the fact that the coincidence was not in place also as well as in direction. Walker's orbit furnished Peirce with the materials for



a still more thorough investigation of the theory and redetermination of the perturbations. Peiree's perturbations gave Walker the means of attaining an orbit yet more rigorously exact, which Peiree used again in his turn; and thus, by the joint labors of our two American astronomers, the theory of Neptune was placed, within eighteen months after the discovery of the planet, upon so sure and accurate a basis, that we risk nothing in asserting that the conformity between the predicted and observed places is far more close for Neptune than for any other planet in the heavens. This was of course a possibility only by reason of the minute eccentricity of the orbit, which alone rendered such an achievement feasible; but it is one of the most brilliant specimens of successful numerical computation on record. And two years later the planet was found so near to the predicted place, that, had another planet existed to mark the place predicted by Walker, it would have formed with Neptune a double star so close that few existing telescopes would have been able to recognize a dark line between.

I pass from this brilliant achievement to another equally brilliant, and which has done yet more to register him as "one of the few, the immortal names that were not born to die." I allude, of course, to the arrangement and development of the American method of observation.

It is impossible, Mr. President and Gentlemen, not to be aware that I am here treading upon dangerous ground. But I will not be false to the memory of my departed friend; and while I trust that the reluctance with which I enter on a subject from around which the clouds of contentious rivalry have not yet wholly cleared away, and that an earnest, faithful, and impartial study of the facts may shield me from any imputation of injustice, the memory of Walker demands, and shall receive, the honor which is its due.

Soon after the completion of the first line of telegraph in this country, — that between Washington and Baltimore, the means thus offered for the determination of the difference of

longitude of these two places was made use of by Captain Wilkes. The correction of a chronometer was determined at each end by transit-observations, and these were compared by telegraphic signals. This method, though far from possessing the precision requisite for geodetic purposes, was amply sufficient for the end desired at the time; and the experiment is historically interesting, as the first recorded telegraphic determination of a difference of longitude.

In the autumn of 1845, two or three months before Walker left Philadelphia for Washington, Professor Bache, who had but recently assumed the direction of the Coast-Survey, entered into communication with Mr. Walker regarding the telegraphic determination of longitude, and requested him to make arrangements in behalf of the Coast-Survey with the Telegraph Companies for the use of their lines. Negotiations for this purpose commenced in January, 1846, and, though temporarily interrupted by Walker's removal to Washington, and assumption of duties at the Observatory, in February, were only delayed for a time. Before his change of residence, the subject was thoroughly discussed between Messrs. Bache and Walker, and the important step of substituting transits of stars for arbitrary signals coinciding with the beats of a time-keeper, was determined on. Mr. Walker has informed me that this suggestion was due to the Superintendent of the Survey; but its practical application seems to have been a result in the elaboration of which the two bore an equal part. At least I may be permitted to state the still more honorable fact, that, in the very many conversations which it has been my privilege to hold with each of the two gentlemen separately upon this interesting question, their descriptions varied but in one salient point, namely, that each ascribed the chief merit to the other.

In a letter to Professor Bache, dated October 3, 1846, the mathematical theory of the subject was fully investigated, and lithographed circulars of instruction issued for the guidance of assistants; and on the 10th of the same month the

transit of a star was telegraphed to Philadelphia by Lieutenant Almy of the Navy, then attached to the Observatory and now to the Coast-Survey. "This," to use Walker's own words to this Association, in 1849, "was the first practical application of the method of star-signals, which is destined sooner or later to perfect the geography of the globe."

Sundry influences combining to embarrass farther proceedings, the work was postponed until the following year, when Mr. Walker entered the Coast-Survey service as a regular officer, and renewed the prosecution of these operations with vigor. On the 27th of July, 1847, he introduced another most important improvement by applying the method of coincidence of beats to the comparison of time-keepers at the two ends; the one indicating mean and the other sidereal time. These beats were signalized from one station to the other by taps of the observer upon the telegraph-key, as nearly as possible synchronous with the ticks of the clock, and forming a graduated register of time upon the running telegraph fillet; a "personal visible register," as Walker called it. The problem which next presented itself was to form a satisfactory "automatic visible register," that is to say, to connect an astronomical clock with a telegraphic circuit, in such a manner that the clock should give its own signals, and the error inseparable from the transference of the signals from the clock to the telegraph through the agency of personal perception and volition thus entirely avoided. This end had indeed been attained by apparatus suggested and used by Mr. Saxton for the measurement of short intervals of time. In 1843, at the launch of the frigate *Raritan*, he had employed an electrotome, consisting of a light tilt-hammer of platinum placed opposite the center of percussion of the pendulum, and struck by it at each oscillation,—and in 1846 had already suggested the use of a globule of quicksilver placed under the extremity of the pendulum in such a position as to form a contact at the instant of verticality. The entire avoidance of any method by which the rate of the clock could possibly be



affected was so indispensable, that it was at first deemed hazardous to employ either of these methods, since the supposed jar to the pendulum in the one case, and the passage of the current through the pendulum-rod in the other, exposed the apparatus to criticism on theoretical grounds. It is, however, but just to state, in this connection, that no practical inconvenience is found to result from either of these features, — a fact abundantly attested by the circumstance that the former of them is at this moment the only method used in the Coast-Survey and the Observatory at Cambridge, and the latter, with some ingenious modifications by Professor Keith, has been adopted at the Washington Observatory.

Mr. Walker's high standard of accuracy and precision led him to spare no exertion to obtain some apparatus which should be free even from theoretical objection; and during the subsequent summer he propounded the problem to Mr. Bond, at Cambridge, and to Professors Mitchel and Locke, at Cincinnati. Each of these gentlemen proposed a method, and constructed an apparatus for the purpose; that of our ingenious colleague, the astronomer of Cincinnati, having been put in operation and exhibited on the next day after the problem was submitted to him. This is not the appropriate occasion for any discussion of the merits of the methods proposed. To do less than assert that the mechanical problem, with a view to its astronomical and geodetic applications, was distinctly propounded by Walker, and by Walker alone, would be unpardonable. And although he might, as regards the public and tangible expression of a nation's gratitude, have written, like Virgil,

“Hos ego feci, — tulit alter honores,”

he may also share the triumph of the Mantuan, by the decisive, even if later, public vindication of his deserts.

The apparatus thus constructed furnished a graduated scale on which the commencement of every second was distinctly marked, so that, when connected with signal-keys, the instant

of the occurrence of any phenomenon might be recorded upon the same scale with sufficient precision to permit of the accurate measurement of time to within a few hundredths of a second.

It became manifest that the gain was not for the determination of longitudes alone, but for astronomical observations of every kind requiring minute precision in the determination of time. Estimate, gentlemen, for yourselves the importance of this step to Practical Astronomy. Walker immediately modified the transit-instrument to suit the new requisitions, and instead of five, seven, or, at the most, nine threads, he provided it with a number of tallies of five threads each. The skill of our accomplished artist, Mr. Wurdemann, has brought the construction of these diaphragms to a high degree of precision, and equipped with them many instruments of the Coast-Survey and of several observatories. No mental effort is requisite for an observation by the new method,—no careful attention to the clock, no counting of the seconds from the low ticks of the pendulum,—but all the attention and care of the observer are concentrated upon the accurate perception of the moment of passage across each thread, and as his fingers rest lightly on the key, their almost involuntary muscular contraction at the instant of transit gives the desired signal.

There remained apparently but one step more to complete the American method of observation, the mechanical attainment of a rotary motion, uniform, from second to second, within the limits of the unavoidable errors of our measurement. Unless this be attained, the measured space upon the record is not strictly proportionate to the elapsed time. This problem, although it has enlisted the attention of able minds, is not yet wholly solved. The ingenuity of Saxton, Bond, and Kerrison has been enlisted in the cause, and year after year, as our Association assembles in its annual round, we are gladdened by the announcement that one difficulty after another has been surmounted by the assiduity and genius of our

gifted colleague of Cincinnati. The improvements which Professor Mitchel announced at the Cleveland meeting have brought us very much nearer the desired goal; and the fact that his interest is still engaged in the enterprise is an earnest of future, and probably not distant, success. During Walker's residence in Cincinnati, soon after leaving Trenton, and but a short time before his death, he visited the Observatory, and Professor Mitchel exhibited the improvements he had already introduced in the apparatus, and explained those which he had in contemplation. Would that Walker might have witnessed their complete triumph,—the crowning glory of his achievement!

At a single meridian passage twenty-five transits are now observed with less fatigue than was formerly required for seven, and in the same time. Each of these transits is worth at least four by the old method, so that the relative efficiency of the two modes of observing is as 100 to 7. This is a low estimate, and derived from the experience of three or four years.

The telegraphic observations between Cambridge, New York, Philadelphia, and Washington furnished a singular and most unexpected result, and one which could not escape the acute perception of Mr. Walker. It appeared not only that the time required for the transmission of the electro-telegraphic signals was appreciable, but that it was — under the circumstances at least in which his longitude-observations had been made — less than a tenth part of the velocity of light in planetary space. This result was so extraordinary, and so greatly at variance with the previous ideas, that acquiescence in it was for some time refused, and it was not until the celebrated velocity experiments between St. Louis and Washington, that the reliability of his results was put beyond question. Some European physicists, indeed, still refuse their credence, and experiments made under different circumstances still yield apparently widely diverse results. The present activity of investigation will doubtless soon conduct us to a thorough



understanding of all the modifying influences. But Walker's claim to the brilliant discovery cannot be controverted ; nor can the fact, that it was himself who stimulated and encouraged the investigations of others. And with a nobleness of soul which those who knew him best can best understand, he aided and sympathized with such researches when they appeared to conflict with his own results, as cordially as when they furnished his theories with confirmation.

The proper limits of this address preclude me from entering more especially upon the description of the works of the great man whose loss we mourn. Let it not be thought, because I have specified these three more brilliant exploits alone, that these were all, or the chief part, of his honorable labors. Numerous able reports and accurate researches enrich the records of the Coast-Survey, and adorn the pages of scientific periodicals on both sides of the Atlantic.

There is one more of his achievements to which I must allude, even though briefly. It is well known to the astronomical portion of this Association, that in the English Nautical Almanac for 1856 appeared (in 1853) a profound, elaborate, and most valuable paper by Mr. Adams, in which this eminent mathematician and astronomer discusses with his characteristic ability the amount of the lunar parallax. The tables of Burekhardt have, until recently, been the standard ones, but Adams shows in this paper that, besides a constant correction, the value of which he has deduced with care, there are a number of important terms to be added to Burekhardt's value, these combined errors sometimes amounting to as large a quantity as 6". In concluding these masterly researches, Adams alludes to the important effect which such errors in the lunar parallax may exert upon the determination of longitudes from occultations, and expresses the opinion, that a great part of the observed difference in values derived from different occultations should be attributed to the use of an erroneous value for the parallax.

Gentlemen, you will in the course of this session learn from

the Superintendent of the Coast-Survey, that our Walker, in April, 1848, presented a report on longitudes, in which, after showing that an error must exist in these longitude-determinations of nearly the same order in seconds of time as the error of the lunar parallax in seconds of arc, he proved that such an error did actually exist, and proceeded to discuss the tables of Burekhardt. In this discussion (presented to the House of Representatives, December 18, 1848, and printed with Exee. Doc. No. 13, XXX. Congr. 2d Session) Walker points out the chief errors in the tables, and shows that the resulting error in previous determinations of the chief points of this hemisphere, as counted from a prime meridian in the other, amounted to two seconds of time. His estimate of the constant error of Burekhardt amounts to nearly the same as Adams's new computation; and he has given four out of the five principal terms with a precision truly astonishing. The sum of the coefficients of the terms indicated by Walker is two thirds of the sum of those of all the terms given by Adams, and our lamented countryman anticipated the distinguished astronomer of England by more than four years.

The additions to the world's science which I have described might either of them justly claim the world's homage and gratitude to his memory, and it may be pardonable if we Americans claim a peculiar share in his renown, not merely because he was our own, but because his most valuable labors were in the national service, as an officer of the United States Coast-Survey. Quoting his own words to this Association, when speaking of the new, American method of observation, "Since this operation is likely to come into general use in geography, geodesy, and hydrography, the origin of the method must be a subject of historical interest. . . . . As respects the service in which we are engaged, this origin is a matter of importance. It is a subject of just pride to those engaged in it, that the first conception of the method, and the first practical operation with it, are peculiarly its own. . . . . I deem it, therefore, a duty to declare, that, with the

single exception of the experiment between Baltimore and Washington, by Captain Wilkes, in 1844, I know of no telegraphic operation for longitude, and of no step in the improvement or perfectionment of the art, in Europe or America, which has not been the work of the officers proper of the Coast-Survey, or of commissioned officers and civilians acting temporarily as assistants. . . . . I will not here allude to the respective claims of Americans for priority or superior excellence of inventions and suggestions, believing that it will be becoming for all of us to look to the great work that has been accomplished by our united efforts, rather than to the single share of each."

That last sentence, Mr. President and Gentlemen, would alone be a monument to his memory.

It is an interesting fact, too, that Walker's labors concerning Neptune were not only stimulated, and in a measure prompted, by the institution within whose walls we are assembled, but that they were, if I mistake not, the first of its publications.

It is not only a token of vitiated taste, but, more than that, it is unworthy of seekers after truth, that the narrow boundaries of terrestrial geography and the small peculiarities of nationality should, as a general thing, enter prominently into our judgement, or even be inseparably associated in our minds with those great discoveries or advances which are the property of our race. It is debasing to those who aspire to the lofty calling of priests in that great catholic temple of Science, where the embodied thoughts of the Most High are revealed in all their majesty and glory to mortal man, that they should descend to unworthy rivalries and discordant claims, as to who first heard the Voice, or who first invoked the sacred Presence. Far from us be all such impulses banished.

But the most earnest patriotism is by no means inconsistent with the most disinterested devotion to Science. And while we deprecate the unworthiness of contests for mere priority, or that national boastfulness, which would extol itself at



the expense of allies in the great crusade, we may be permitted to gather hopefulness and courage from past success, and to cultivate that peaceful competition which springs from mutual emulation in a noble cause. Walker's condemnation of the spirit which would permit an American in the present condition of our science to send his researches across the Atlantic for publication, when the means for their publication here was at hand, was constant and deep. And no enterprise which even in the most indirect way would aid in the development of scientific taste or culture, or the elevation of the standard of excellence, wanted his hearty and constant aid, not in words merely, but in solid, effectual deeds. It is thus that Walker was patriotic; it is thus that I invoke your pride as Americans. He was one of the foremost champions in the great struggle in which we are all now enlisted for the advancement of science by means of its advancement in America; and when, from failing health or exhausted energies, all other motives had spent their force, the thought of his country's honor has nerved him again to the task. I have seen his eye flash, and his wearied features light up, at the thought that his aching head and overstrained nerves were suffering for his country's glory; and when prostrate on a bed of pain, his was the enthusiasm the wounded patriot feels in the moment of victory.

I have said that the crisis which fully developed Walker's character and powers was opportune for him and for us. Just at the moment when the feeble science of America most needed willing hands and able minds and earnest hearts, to protect, to guide and impel, to battle with her most insidious foe, that avid tribe which not only claims her kindred without title, but would fain make her, as Schiller says, their cow and not their goddess, — just as, in the progress of our national civilization, institutions were founding in all quarters of the land, which, in the absence of proper guidance, may easily prove as fatal as they were designed to be propitious, — just as the efforts of good men were combining in all quarters of

the land for the great cause they had so deeply at heart, — Walker came forward, and labored for his country, for truth, and for mankind. To him belongs a prominent place among the founders of Science in America. And if I lay stress on the rank which his labors have aided our country to assume, it is because I would claim just ground for pride in America's recent progress and her present tendency, rather than for the aggregate of her contributions thus far to the science of the world.

It was Walker's custom, during the last five years of his life, to escape from the heat of Washington, and pass the summer months in Cambridge. But neither the temptations of social intercourse with his numerous friends of kindred taste residing there, nor the close vicinage of the ocean he so much loved, nor yet the charms of the scenes of his youth, could wean him from his constant, unceasing mental application. Here it was that, in August, 1851, he was visited with a slight attack of paralysis, which for a few days deprived him of the use of one hand. Still he would not leave his investigations. The warnings of friends, the entreaties of relatives, the appeals of the physician, were all in vain; he could not abandon his studies and cherished pursuits. The dawn surprised him at his labors, and the eastern sunlight scarcely touched his pillow before he was again at work.

Yet the attack and its effects appeared to pass away, and during the same autumn Mr. Walker took charge of the telegraphic expedition at Bangor, Maine, for determining the differences of longitude between Halifax, Bangor, and Cambridge, — an expedition which was hardly terminated before the beginning of the year 1852. He had been but two days in Washington, on his return, before symptoms of mental alienation appeared, the true character of which was but too evident. The energy of the imprisoned intellect was disproportionate to the walls of flesh, and even the malady still illustrated the wondrous activity and power of his mind, and the wide scope of his learning. He remained a short time in

the asylum at Mount Hope, near Baltimore, and thence, in April, 1852, was transferred to Trenton, New Jersey, where in the skilful and judicious Superintendent, Dr. Buttolph, he found at once a wise counsellor and a sympathizing friend. Those who loved Walker owe to this gifted and true-hearted man a debt of gratitude and respect which time cannot efface.

In the beautiful seclusion of Trenton, Walker's mind reassumed by degrees its normal condition. Frequent visits of friends and the companionship of a devoted sister were permitted him; he corresponded with friends at a distance, who kept him informed of the progress of astronomical researches and discoveries, and the earnest, touching appeals for his books and papers were gradually complied with. Here he computed the ephemeris of Neptune published in the American Astronomical Ephemeris for 1855, and in the autumn of the same year left the institution with apparently sound mind and sound body; though of course greatly debilitated by the effects of his illness, and physically hardly the shadow of his former self.

He immediately visited his brother, Hon. Timothy Walker, in Cincinnati, with the intention of remaining there until the early spring. His letters from that city show him the same that he was of old,—the enthusiastic scientist, the warm-hearted friend. He resumed his labors for the Coast-Survey, and arranged for the renewal of his former sphere of activity, re-engaging his apartments, and fixing the day for his journey. But, alas! he was never again to look upon the blue waters of Potomac, nor the proud dome of the Capitol. An attack of fever, at first considered merely as a slight indisposition, proved too much for his enfeebled frame to withstand. Other maladies followed, and on the 30th of January, 1853, Walker died,—but in the midst of his friends and kindred. His ashes rest in the cemetery of Spring Grove, some four miles north of Cincinnati, on the banks of a beautiful tributary of the Ohio, called by the Indians *Mahkatewa*.



Mr. President and Gentlemen, you know how the Science of America has mourned for our departed Walker. Of her deep bereavement it is needless for me to speak. She was his only love. To her he had given all the devotion of a great and earnest heart, and he had experienced the ennobling influences of such devotion.

“ His spirit, ere our fatal loss  
Did ever rise from high to higher ;  
As mounts the heavenward altar-fire,  
As flies the lighter through the gross.”

The most conspicuous attribute of Walker's mental character was his intellectual activity. In despite of the serious obstacle offered by his physical constitution, — an obstacle which he felt most keenly, and which, indeed, prevented him from obtaining the bodily exercise imperative for the intellectual as well as corporeal health of the student, his ever vigorous mind was ceaselessly at work. Intellectual labor was the atmosphere he breathed, — he only lived in work. Never idle, the love of science was the moving spring of every deed and thought and aspiration, and the approval of those whose judgement was entitled to confidence was the highest reward he asked. Impulsive to a fault, his impulses were ever for some noble cause. And the liberality with which his mind was always open to conviction, the magnanimity with which he was at all times ready to acknowledge an error, and a peculiar readiness to overlook even injustice and moral error in others, were among the most prominent of his traits. That he was free from faults, no one would claim ; — who is ? Honor to the man whose failings are the offspring of generous impulses, whose weaknesses testify to the generosity of his heart, and of whom we may be sure, even if he err, that he has erred in order that, in his opinion, good might come. The transparency and childlike simplicity of his character misled those to whom such simplicity was foreign, and after seeking in vain to detect some hidden motive, they attributed to him a profound tact and a shrewdness utterly at variance

with his nature. A child of Genius, he followed whither his nature led, and all the outward bustle of the world had for him a less than secondary importance. Neither fitted nor inclined for a life of active participation in the world's affairs, yet gifted pre-eminently with the spirit of social communion and joyous companionship, he was a cheerful, yes, a happy man, even under depressing influences; and when the clouds of earth obscured and darkened his way, he could soar above them to the pure expanse of the intellectual empyrean.

“He whom thou never leavest, Genius,  
Feels no dread arise within his heart  
At the tempest or the rain.  
Thou wilt place him on thy fleecy pinion  
When he sleepeth on the rock;—  
Thou wilt shelter with thy guardian wing,  
In the forest's midnight hour.”

I should do wrong did I not allude to the classical attainments of Mr. Walker, which were great and varied. They were the solace of his weariness, and an unfailing well of constant pleasure. He possessed a singularly retentive memory for poetry. He could remember, and was wont to repeat, long passages from the Greek, Latin, and Italian poets. Tasso, his especial favorite, he knew by heart. Although his tastes, his intellect, his very affections, were devoted to the study of the exact sciences, he was no stranger to the refining and genial influences of classic culture. None were more earnest than he in combating the hazardous theory which is unfortunately so widely disseminated in the United States, that classical studies have hitherto occupied an undue prominence in the education of youth. His first advice to young men was to acquire a thorough knowledge of ancient and modern languages. When, some nine years ago, it was first my privilege to know Mr. Walker, I went to him, just after leaving college, to ask his advice as to future studies, and the best means of attaining scientific usefulness. “Study foreign languages,” was his advice; “for thus alone can you

keep pace with the progress of modern science. Read Laplace, Lagrange, Gauss, Bessel, Plana, in their own tongues, and accustom yourself to think with them in their own words." It was thus that Walker attained his pre-eminent position, it is thus that those who aspire to emulate his bright example would do well to direct their energies.

Among the number, happily not small, of the great men whose joy it has been to extend the guiding, helping, and protecting hand to young men aspiring to pursue a life of scientific usefulness, none surpassed Walker in this noble trait. Who was so ready to aid? who so eager to assist? in doubt and obscurity, who could guide so well as he?

He is gone. That helping, welcoming hand is palsied in the grave. That earnest voice is heard no longer in our midst. That powerful and restless intellect gives token of itself no longer. The eye that tracked the distant Neptune through the wilderness of space is closed for ever. The strong will that subdued all the forces of Nature to his own loved science is broken. And the quick thought that outstripped the electric messenger, and measured his pace, is stilled for evermore. He is gone. His memory alone remains, his noble memory.

"He was our own. How social, yet how great  
 Seemed in the light of day his noble mind!  
 How was his nature pleasing yet sedate!  
 Now for glad converse joyously inclined,  
 Then, swiftly changing, spirit-fraught, elate  
 Heaven's plan with deep-felt meaning it designed.  
 Fruitful alike in counsel and in deed,  
 This have we felt, this tasted, in our need.

"And many a soul that strove with him in fight,  
 And his great merit grudged to recognize,  
 Now feels the impress of his wondrous might,  
 And in his magic fetters gladly lies.  
 E'en to the highest has he winged his flight,  
 In close communion held with all we prize.  
 Extol him then! What mortals while they live  
 But half receive, posterity shall give."



AN  
HISTORICAL EULOGY  
OF  
M. LE MARQUIS DE LAPLACE,

PRONOUNCED IN

THE PUBLIC SESSION OF THE ROYAL ACADEMY OF SCIENCES,

AT PARIS, JUNE 15, 1829.

BY M. LE BARON FOURIER,  
PERPETUAL SECRETARY.

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TRANSLATED FROM THE FRENCH,

BY R. W. HASKINS,  
OF BUFFALO, N. Y.

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*Gentlemen:*—The name of LAPLACE has resounded in all parts of the world where the sciences are honored; but his memory has not received a more exalted homage, than the unanimous tribute of admiration and regret of that illustrious body, of which he has shared the labors and the renown. He consecrated his life to the investigation of the most noble objects upon which it is possible to occupy the human mind. The wonders of the heavens, the most exalted questions in natural philosophy, the ingenious and profound combinations of analytical mathematics; in short, all the laws of the universe, were present to his mind for almost sixty years, and his efforts were crowned by immortal discoveries.

It was remarked, from his earliest studies, that he was endowed with a remarkable memory, and that all the exercises of the mind were easy to him. He readily acquired a knowledge, sufficiently extensive, of the ancient languages, and cultivated divers branches of literature. All interested the rising genius, and he could compass all. His first successes were in theological studies, and he treated with talent and extraordinary sagacity, the most difficult points of controversy. We know not what happy event directed Laplace from scholastic studies to the depths of geometry. This last science, which admits of but little division, attracted and fixed his attention. From that moment he abandoned himself, without reserve, to the promptings of his genius, and felt, most vividly, that a residence in

the capital had become indispensable to him. D'Alembert was then enjoying the full measure of his renown. It was he who had advised the court of Turin that its Academy Royal possessed a geometrician of the first order, namely, Lagrange, who, in default of this friendship, might long have remained unknown. D'Alembert had announced to the king of Prussia, that but one single man in Europe was able to replace, at Berlin, the illustrious Euler, who, recalled by the government of Russia, had consented to return to St. Petersburg. I find, in the unpublished letters in possession of the Institute of France, the details of that glorious negotiation which fixed Lagrange in a residence at Berlin.

It was about the same period that Laplace commenced that long career, which he was destined soon to render so illustrious.

He presented himself at the house of D'Alembert, preceded by numerous recommendations, which he believed to be both powerful and necessary. But the proffer of them was useless: he was not even introduced. He then addressed to him of whom he had solicited the friendship, a most remarkable letter, upon the general principles of mechanics, and of which Laplace himself has oftentimes quoted passages. It was impossible that so great a geometrician as D'Alembert should not be struck with the extraordinary profundity of this production. The same day he applied to the author, and said to him—*these are his very words*—"Sir, you see that I make little enough account of recommendations; but you had no need of one. You have made *yourself* well known, which satisfies me: my friendship is due to you." A few days after this, Laplace was named professor of mathematics in the military school of Paris. From this moment, devoting himself unreservedly to the science he had chosen, he gave to all his efforts one fixed direction, from which he never after swerved; for the unshaken constancy of his views was ever the principal trait of his genius.

He had reached, already, the utmost known limits of mathematical analysis: he possessed all which this science then embraced, that was most ingenious or profound, and no man was more capable than himself to augment its domain. He had resolved one important question in theoretical astronomy. He formed the resolution to consecrate his efforts to that sublime science: he was destined to perfect it; and he was able to embrace it in all its extent. He profoundly contemplated his glorious design, and he devoted all his life to its accomplishment, with a perseverance of which the history of the sciences affords no other example.

The immensity of the subject flattered the just pride of his genius. He undertook to compose the *Almagest* of his age: it is the monument which he has left us, under the name of *Mécanique Céleste*; and this immortal work as far surpasses that of Ptolemy, as modern analytical science excels the elements of Euclid.

Time, which alone dispenses, with justice, literary glory; which delivers over to oblivion all cotemporaneous mediocrity, perpetuates the remembrance of great works. These only, convey to posterity the character of each age. Thus the name of Laplace will live through all future time. But, and I hasten to declare it, lucid and faithful history will not separate his memory from that of other successors of Newton. It will unite the illustrious names of D'Alembert, Clairault, Euler, Lagrange and Laplace. I limit myself to cite here, those great geometricians whom the sciences have lost, and the researches of whom have had for one common aim the perfection of physical astronomy. To give a just idea of their works, it is necessary to compare them; but the bounds assigned to this discourse oblige me to reserve that part of this disquisition for the collection of our memoirs.\*

After Euler, Lagrange most contributed to establish mathematical analysis. It has become, in the writings of these two great geometricians, a distinct science; the only one of mathematical theories of which it can be said that it is completely and rigorously demonstrated. Alone among all these theories, it is all-sufficient to itself, and it elucidates all the others; it is indispensable to them, and, deprived of its aid, they would not be able to endure, except very imperfectly.

Lagrange was born to invent and to aggrandize all the sciences of the calculus. In whatever condition fortune had placed him, whether as shepherd or prince, he had still been a great geometrician. He would have become this necessarily, and without an effort, which cannot be said of all those who have excelled, even in the first ranks, in this science.

If Lagrange had been contemporary with *Archimedes*, and with *Conon*, he had divided the glory of the most memorable discoveries: at Alexandria he had been a rival of *Diophantus*. The distinctive trait of his genius consists in the unity and the grandeur of his views. He confined himself, in all things, to one simple thought, elevated

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\* Memoirs of the Royal Academy of Paris.—Translator's Note.



and just. His principal work, the *Mécanique Analytique*, should be named *Mécanique Philosophique*; for it reduces all the laws of equilibrium, and of movement, to one common principle; and, which is not the least admirable, it subjects them all to one single method of calculation, of which Lagrange is himself the inventor. All his mathematical compositions are remarkable for one singular elegance, namely, the symmetry of the forms and the generality of the methods; and, if we may speak thus, for the perfection of the analytic style.

Lagrange was no less a philosopher than a great geometrician. He proved this, through all the course of his life, by the moderation of his desires, his immovable attachment to the general interests of humanity, by the noble simplicity of his manners, and the elevation of character; and, finally, by the exactness and profundity of his scientific labors.

Laplace had received from nature all the force of genius which an immense enterprise can demand. Not only has he concentrated in his *Almagest of the eighteenth century*, all that the mathematical and physical sciences had already invented, and which serve as the foundation of astronomy, but he has added to these sciences valuable discoveries which are strictly his own, and which had escaped all his predecessors. He has resolved, either by methods purely his own, or by those of which Euler and Lagrange had indicated the principles, questions the most important, and certainly the most difficult of all those which others had considered before him. His constancy has triumphed over all obstacles. Whenever his first attempts have not been successful, he has renewed them under the most ingenious and diversified forms.

Thus, there had been observed, in the movements of the moon, an acceleration of which no one had been able to discover the cause. It had been thought that this proceeded from the resistance of an ethereal medium to the movement of the celestial bodies. Now, if this were so, the same cause, affecting the course of the planets, would tend, more and more, to derange their primitive order. They would be constantly disturbed in their course, and eventually conclude by precipitating themselves upon the mass of the sun. It would become necessary that the creative power interfere to prevent or to repair the immense disorder which the lapse of time should have caused!

This cosmographical question is certainly one of the most sublime upon which the human mind can be employed: *it is solved in our day*. The first researches of Laplace upon the invariability of the dimensions of the solar system, and his explanation of the secular equation of the moon, have conducted to its solution. He had first examined if it were possible to explain the acceleration of the movement of the moon, by supposing that the action of gravity is not instantaneous, but subject to successive transmission, like light. By this process he was not able to discover the true cause. A new investigation was crowned with complete success. He gave, on the 19th of March, 1787, to the Academy of Sciences, a clear and most unexpected solution of this difficult problem. In this he proved distinctly, that the observed acceleration is a necessary effect of the law of universal gravitation.

This important discovery has subsequently elucidated some of the most essential points of the system of the world. It makes known to us, that if the action of gravitation upon the planets is not instantaneous, we must suppose that it moves more than fifty millions of times faster than light, of which the velocity is known to be about two hundred thousand miles per second. He concluded also, from his theory of the lunar motions, that the medium in which the stars move does not oppose to the course of the planets a perceptible resistance, for its effect would be most visible upon the movements of the moon, and yet no such result has appeared.

The discussion of the movements of this planet is prolific in remarkable consequences. From it the conclusion follows, that the rotary motion of the earth upon its axis, for instance, is invariable. The length of the day has not changed so much as the one hundredth part of a second in two thousand years! An astronomer, remarkable as it may appear, need not leave his observatory to measure the distance between the earth and the sun. He has but to observe assiduously the variations of the lunar movements, and from these he may determine that distance with certainty. A circumstance still more remarkable, is that which refers to the figure of the earth; for the very form of the terrestrial globe is disclosed by certain inequalities in the course of the moon. These inequalities would not exist, if the earth was perfectly spherical. We are able therefore to determine the earth's ellipticity, by the observation alone of these lunar movements, and the results deducible therefrom accord with the actual admeasurements performed through geo-

dasical voyages, undertaken for that purpose, to the equator, to the regions of the north, to India, and to various other countries.

It is to Laplace, above all others, that we are indebted for the amazing perfection of modern theories. I am unable here to refer in order to his works, or to the discoveries that have resulted from them. Their bare enumeration, however rapid, would exceed the limits to which I ought to confine myself. Besides his researches upon the secular equation of the moon, and the discovery, neither easy or unimportant, of the cause of the great inequalities of Jupiter and Saturn, I should have to refer to the admirable theorems upon the libration of the satellites of Jupiter. It would be necessary also to call to mind his analytical labors upon the flux and reflux of the sea, and to show the immense extent which he has given to that question.

There is no important point of physical astronomy which did not become to him an object of study, and of profound investigation : he has submitted to calculation most of the physical conditions which his predecessors have omitted. In the question, already so complex, of the form and rotary motion of the earth, he has considered the effect of the presence of water distributed among the continents ; the compression of the interior strata of the earth, and the secular diminution of the dimensions of our globe.

In this multiplicity of investigations we should particularly regard those which are connected with the stability of the grand phenomena ; than which nothing is more worthy of the meditations of philosophers. Thus, it is known that the causes, whether fortuitous or constant, which disturb the equilibrium of the seas, are subject to limits which they can never exceed. The specific gravity of water is much less than that of solid earth, and it follows that the oscillations of the ocean are always confined within very narrow limits, which would not be the case if the liquid spread over the globe were much heavier than it is. In general, nature holds in reserve the conservative forces, always present, which instantly interpose when the derangement commences, and always with power sufficient for its correction, by which the accustomed order is restored. We find, in all parts of the universe, this conservative power. The form of the planetary orbits, and their inclinations, vary and alter in the course of ages, but these changes are all limited. The primitive dimensions continue, and that immense assemblage of celestial bodies oscillates about a mean point, towards which they always return. All is disposed for order, perpetuity and harmony.



In the primitive and liquid state of the terrestrial globe, the heaviest particles of matter were congregated at the center ; and this has determined the stability of the seas.

Whatever was the physical cause of the formation of the planets, that cause has given, to all these bodies, a projectile movement, in the same direction around the sun, by which the solar system is rendered stable. The same effect is produced in the system of the satellites, and of Saturn's ring. Order is there maintained by the attractive force of the central mass. There is not, then, as Newton himself, and Euler have suspected, an additional power which must one day repair the derangement that time has caused. It is the law of universal gravitation itself, which is equal to every emergency, that regulates all, and maintains the variety and the order of the universe. The primitive emanation of supreme wisdom has presided over creation from the beginning of time, and rendered all disorder impossible. Newton and Euler knew not, therefore, *all* the perfections of the universe.

Usually, in all cases where doubts of the accuracy of the Newtonian law have arisen, and where, to explain its apparent defects, the accession of foreign causes has been sought, profound investigation has subsequently verified the supremacy of this primordial law. It has, at the present day, explained all known phenomena ; and the more accurately observations have been made, the more are their results found to conform to its theory. Laplace has, of all geometers, penetrated most profoundly into these sublime questions : he has indeed fathomed them, if the expression will be allowed me.

We cannot aver that it was given to Laplace to create an entire new science, as Archimedes and Galileo had done before him ; to give to the mathematical doctrines original principles of immense extension, as has Descartes, Newton and Leibnitz ; or, like Newton to transplant to the regions of the planets, and to extend to all the universe, the terrestrial dynamics of Galileo ; but Laplace was born to perfect all, to fathom all ; to transcend all former limits, and to resolve questions previously deemed incapable of solution. He would have perfected the science of the celestial world, had that science been capable of receiving such perfection at human hands.

The same characteristic is discernible in his researches upon the analysis of probabilities—a science wholly modern, yet immense ; the object of which being often mistaken, has given rise to inter-

pretations the most erroneous, but the applications of which will eventually embrace the whole field of human knowledge; forming a happy supplement to the imperfections of our nature.

This science, primitively the offspring of the clear and profound genius of Pascal, had been cultivated from its origin by Fermat and Huygens. James Bernouilli, a geometrician and philosopher, was its principal founder. The singularly happy discovery of Sterling, the researches of Euler, and above all, the ingenious and important application for which we are indebted to Lagrange, have perfected this doctrine. It has been illustrated by the very objections of D'Alembert, and by the philosophical views of Condorcet: Laplace has concentrated the several parts, and fixed the principles of the doctrine. It has become, then, a new science; and it is one subjected to one single rule of analysis. In addition to its constant usefulness in many of the ordinary purposes of life, it will yet shed the vivid light of day upon all the branches of natural philosophy. If we may be permitted here to express an individual opinion, we will add that the solution of one of the principal questions, namely, that of which the illustrious author has treated in the tenth chapter of his work, appears to us not strictly correct; yet, considered collectively, this work\* is one of the most precious monuments of his genius.

After having cited these brilliant discoveries it is scarcely necessary to add that Laplace belonged to all the first academies of Europe.

I might, and perhaps I should, here cite the high political distinctions with which he was clothed; but such enumeration cannot directly pertain to the object of this discourse. It is the great geometrician whose memory we celebrate. We have separated the immortal author of the *Mécanique Céleste* from all fortuitous circumstances, not strictly connected either with his glory or his genius. In short, gentlemen, what interest has posterity—who have already so many other details to forget,—in knowing or not that Laplace was, for a brief period, minister of a great state! What will interest them are the eternal truths he has discovered: the immutable laws that ensure the stability of the universe, and not the station he may have occupied some years in the conservative senate. What is important, gentlemen, and perhaps even more so than these discoveries, is the

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\* *Théorie Analytique des Probabilités.*—*Translator.*

example which he leaves to all those by whom the sciences are cherished, in the recollection of that incomparable perseverance which so effectually sustained, directed, and crowned his glorious efforts.

I omit, then, the fortuitous or accidental circumstances, so to speak, and such particulars as have no necessary connexion with the perfection of his works; but I will add, that in the first body of the nation the memory of Laplace was celebrated by a voice friendly and eloquent, which, by the important services it had rendered to historical science, to literature and to the state, had long since become illustrious.\*

I may refer, also, to the literary solemnity which attracted the attention of the capital. The French Academy, in uniting its voice to the acclamations of the country acquired additional renown by crowning political virtue and the triumphs of eloquence.† At the same time that learned body selected, as the successor of Laplace, an illustrious academician, who happily concentrates in himself all, in literature, in history, and in public administration, that most pointedly denotes superiority.‡

Laplace enjoyed one advantage which fortune does not always accord to great men. From his earliest youth he was deservedly appreciated by some illustrious friends. We have before us manuscript letters that describe the zeal which D'Alembert manifested to introduce him into the military school of France; and to prepare for him, if that were necessary, a still more valuable situation at Berlin. *Le president Bochart de Sarou* first printed his early works. All the evidences of friendship which have been given him forcibly recall his herculean labors and giant discoveries; but nothing so ably contributed to the progress of all physical knowledge as his relations with the illustrious Lavoisier, whose name, consecrated by the history of the sciences, has become an object of eternal respect and lamentation.

These two extraordinary men united their efforts. They attempted and achieved a very extended course of experiments to measure one of the most important elements§ of the physical theory of heat. They made, also, about the same time, a long series of experiments upon the dilation of solid bodies. The works of Newton sufficiently

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\* M. le Marquis de Pastoret. † M. Royer-Collard. ‡ M. le Comte Daru.

§ Algebraical elements.—*Translator.*



show the high value this great geometrician attached to the study of the physical sciences. Laplace has, of all his successors, made the most extensive and general application of the experimental method. He was almost as great a philosopher as geometrician. His researches upon refraction, upon capillary effects, barometrical admeasurements, statistical principles of electricity, velocity of sound, molecular action, the properties of gas, &c. show that nothing, in the investigations of nature, escaped his examination. He particularly desired perfection in instruments; and he had constructed, at his own cost, by a celebrated artist, a most valuable astronomical instrument, which he presented to the observatory of France.

With all the variety of phenomena he was intimately acquainted. He was closely allied, by long friendship, with two celebrated philosophers, whose discoveries have illumined alike the arts and the theories of chemistry. History will unite the names of Berthollet and Chaptal to that of Laplace. With these he was pleased to join, and their mutual discoveries ever had for their object the increase of knowledge the most important, and the most difficult to acquire.

The gardens of Berthollet, at his mansion *d'Arcueil*, were not separated from those of Laplace. Pleasing recollections and the most lively regrets unite to render illustrious this enclosure. It was there that Laplace received celebrated strangers, and all prominent men of whom science had received or expected benefits; but above all, such as sincere zeal attracted to the sanctuary of the sciences; whether they were those just commencing or such as were about to close an illustrious career. He entertained all with extreme politeness; and so unpretending was his demeanor that those not fully acquainted with the extent of his genius might be left to infer that he himself was sometimes able to draw instruction from the interview.

In citing the mathematical works of Laplace we ought, most particularly to notice the profundity of his research, and the importance of his discoveries. His works are distinguished by another characteristic, which all readers have appreciated. I refer to the literary merit of his compositions. His *Système du Monde* is remarkable for elegant simplicity of style and the purity of its language. There was not, as yet, an example of this species of writing; but we should entertain an erroneous idea if we suppose one would be able to acquire a knowledge of the phenomena of the heavens from sim-

ilar works. The suppression of the signs proper to the language of the calculus neither illustrates the text nor facilitates the reading. The work is a perfectly regular exposition of the results of the most profound investigations: an ingenious summary of principal discoveries. The precision of style, the choice of method, and the grandeur of the subject give a singular interest to this vast picture; but its real utility is to recall to geometers the theorems of which the demonstrations are already known to them. It is, properly speaking, a table of contents to a mathematical treatise.

The purely historical works of Laplace have another object. He there presents in a masterly manner, to geometers, the progress of the human mind in the inventions of the sciences.

The most abstract theories have, in fact, a beauty of expression which is peculiarly their own: as may be observed in several treatises of Descartes, in some pages of Galileo, in Newton and Lagrange. The novelty of our author's views, the elevation of his thoughts and their close affinity with the grand objects of nature, attract and fill the mind. It was sufficient that his style was pure and of a noble simplicity: it is this species of literature which Laplace has chosen, and it is certain that he occupies the first rank among authors of that class. His history of great astronomical discoveries is a model of elegance and precision. No prominent trait of his subject escapes him, and his expression is never either ambitious or obscure. All that he dignifies as great, is really so; while all he omits deserves not to be cited.

Laplace retained in very advanced age, that extraordinary memory for which he was remarkable from youth: that precious gift, not of genius, but the ability to acquire and to retain. The fine arts he did not cultivate, though he appreciated them. He was fond of Italian music: he admired the verses of Racine, and often amused himself with repeating, from memory, various passages of that great poet. The productions of Raphael's pencil adorned his apartments, by the side of the portraits of Descartes, Francis Viète, Newton, Galileo and Euler.

Laplace ever confined himself rigidly to light food; and of this, he reduced the quantity, from time to time, even to excess. His sight originally delicate, required continually the greatest precautions; yet he preserved it unimpaired through life. His great care of himself appeared to have but one single object, namely, to prolong life and preserve his powers solely for intellectual exertion. He lived for the sciences alone, and they have rendered his memory eternal.

He had established the habit of excessive daily mental effort, so detrimental to health, so necessary to profound investigations; yet notwithstanding this, he experienced no diminution of his accustomed vigor until within the two last years of his life. At the commencement of the malady to which he fell a victim, his friends recognized with anxiety and alarm, delirium, as one of its features. During the short period of his alienation, the sciences alone engrossed his attention. He spoke, with more than his accustomed ardor, of the planetary movements; and subsequently of a physical experiment which he declared to be most important: and he announced to persons whom he imagined to be present, that he should soon entertain the academy upon these subjects. He was now sinking rapidly. His physician,\* who merited by his superior talents, and by the care and solicitude which friendship alone had inspired, all the confidence of his patient, watched unremittingly at his bedside; and M. Bouvard, his co-laborer and friend, quitted him not, even for one moment. He was surrounded by a family to which he was devoted; under the eye of a consort whose tenderness had aided him to support those pains inseparable from life, and whose affections and accomplishments had taught him the value of domestic happiness; and he received from *M. le marquis de Laplace*, his son, marked evidences of the most touching filial affection.

He showed himself impressed with the recollection of the reiterated marks of interest that had been showed him by the king, and by Monsieur le Dauphin.

Those who attended him in his last moments, recalled to his mind the titles of his renown, and his most illustrious discoveries: his only reply was—"The knowledge we have of things is, indeed, small, while that of which we are still ignorant is immense." This is, at least as far as it was possible to seize it, the meaning of his last words, which were scarcely articulated. The same thought, and nearly in the same language, we have often before heard him express. He expired quietly, and without pain.

His last hour having arrived, the mighty genius which had so long animated him separated itself from its mortal envelop and returned to heaven.

The name of Laplace honors one of our provinces already so fruitful in great men, namely, ancient Normandy. He was born on

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\* M. Magendie.



the twenty-third of March, 1749, and he expired in the 78th year of his age, the fifth of May, 1827, at nine o'clock in the morning.

Need I recall to your recollection, gentlemen, the solemnity which diffused itself like a cloud over this mansion, when the fatal intelligence was announced? It was upon the very day and hour of your accustomed sessions. Each of you observed a mournful silence; for each felt most deeply, the shock this blow had given to the sciences. All faces were turned in silence to that seat which he had so long occupied among you. One single thought alone was present: all other meditation was impossible. The session was dissolved by your unanimous voice; and your body, on that day, for the first time was interrupted in its accustomed labors.

It is doubtless just—it is dignified and glorious, in a great nation, to decree illustrious honors to the memory of celebrated men. In the land of Newton the rulers of the state resolved that the mortal remains of that great man might be solemnly deposited among the royal tombs. France and Europe have offered to the memory of Laplace, an expression of their regrets, undoubtedly less fastidious, but perhaps more touching and more true.

He has received unaccustomed homage: but it was alone the learned circle in which he moved that could fully appreciate all his genius. The weeping voice of the sciences is heard in all parts of the world where philosophy has penetrated. We have examined numerous communications from all parts of the Germanic Confederation; from England, Italy, New Holland, the English possessions in India, and both Americas; and in all these we find the same sentiments of admiration, and expressions of the same regrets. Certainly the universal lamentation of the sciences, thus generously and voluntarily expressed, has not less of truth and lustre than the sepulchral pomp of Westminster.

I may be permitted, before closing this discourse, to repeat a reflection which presented itself when I recounted in this hall, the grand discoveries of Herschel, and which is still more applicable to those of Laplace than to his.

Your successors, gentlemen, will see verified the grand phenomena of which he has discovered the laws. They will behold those changes in the lunar movements which he has predicted, and of which he, alone, has been able to assign the cause. Continual observations of the satellites of Jupiter will perpetuate the memory of the inventor of the theorem which regulates them in their course;

and the great inequalities of Jupiter and Saturn, pursuing their long periods, and giving continually to these planets, new situations, will perpetually recall to recollection one of his most noble discoveries. These are the titles of true glory, which nothing can annihilate or efface. The appearances of the heavens will be changed; but even to these remote periods the glory of the discoverer will forever endure: the traces of his genius bear the seal of immortality.

I have presented to you, gentlemen, some traits of an illustrious life, which was consecrated to the glory of the sciences: your recollections will supply what I have omitted. Behold the voice of the country—of the human family, indeed, elevated to celebrate the benefactor of nations: the only dignified homage to those who, like Laplace, have been able to enlarge the domain of thought; and to verify to man the dignity of his being, by unveiling to his view all the majesty of the heavens!

R E P O R T  
ON THE  
HISTORY OF THE DISCOVERY  
OF  
N E P T U N E.

BY  
BENJAMIN APTHORP GOULD, JR.

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## R E P O R T.

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IN a general account of the discovery of the planet Neptune, and of the remarkable circumstances which preceded and attended this discovery, it cannot at this late period be expected that any new views should be brought forward, or any material facts cited, which have escaped the notice of the eminent astronomers who have already written upon this subject. The facts are before the world. The numerical data are in all the books. The history is too strange to be forgotten by any one who has once studied it. In the following Report, therefore, it can scarcely be hoped that the facts should be more clearly arranged or more concisely presented than has already been done by Airy, Biot, and Herschel, or that any thing, bearing upon the history prior to 1847, be narrated, which cannot be found elsewhere.

The strange series of wonderful occurrences of which I am to speak is utterly unparalleled in the whole history of science ; — the brilliant analysis which was the direct occasion of the search for a trans-Uranian planet, — the actual detection of an exterior planet in almost precisely the direction indicated, — the immediate and most unexpected claim to an equal share of merit in the investigation, made in behalf of a mathematician till then unknown to the scientific world, — and finally the startling discovery, that, in spite of all this, the orbit of the new planet was totally irreconcilable with those computations which had led immediately to its detection, and that, although found in the direction predicted, it was by no means in the predicted

place, nor yet moving in the predicted orbit. This series of events, together with the since developed theory of Neptune, constitute the subject of my Report.

The correctness of the prophecy, made<sup>1</sup> by a British writer within a few weeks of the discovery of the planet, that the future historian of astronomy would find it necessary to change his pen more than once while discussing this subject, will hardly now be called in question by the strongest partisans of any of those illustrious scientists, who have occupied themselves with the theories of Uranus and Neptune;—although the most important point at issue is a question very different from the one then anticipated.

There has never been a more complicated case presented for the sober judgment of the impartial historian; and the temptations to dwell upon the romance and poetry of the subject are extremely strong. I purpose, however, in the following history of the circumstances which led to, and have been connected with, the discovery of Neptune, to present the subject as simply as possible; leaving to others the philosophical considerations and poetic fancies which it suggests, and aiming only at clearness and impartiality. I shall arrange the Report as nearly as possible in the chronological order.

The planet Uranus was discovered by Sir William Herschel on the 13th of March, 1781, and, although at first supposed to be a comet, was before the end of the year recognized<sup>2</sup> as one of the primary planets of our solar system. Circular elements were first computed<sup>3</sup> during the summer of 1781, by Lexell, of St.-Petersburg, at that time in London; and others were soon after published in Russia,<sup>4</sup> France,<sup>5</sup> and Germany.<sup>6</sup> The computation

<sup>1</sup> *London Athenæum*, Nov. 21, p. 1191.

<sup>2</sup> *Mémoires de l'Acad. des Sciences, Paris*, 1779, p. 526; *Berliner Astronomisches Jahrbuch*, 1784, p. 215.

<sup>3</sup> *Acta Acad. Petrop.*, 1780, *Mem.*, p. 307.

<sup>4</sup> *Nova Acta Acad. Petrop.*, I., *Hist.*, pp. 72, 76, 81; *Acta Acad. Petrop.*, 1780, *Mem.*, p. 312.

<sup>5</sup> Lalande, *Mém. Acad. Sc. Paris*, 1779, p. 526.

<sup>6</sup> Klögel, of Helmstadt, *Berl. Ast. Jahrb.*, 1785, p. 193. Hennert, *ibid.*, p. 205.

of a planetary orbit was at that time a most laborious and troublesome process, by no means to be compared with the easy methods in use since Gauss gave<sup>1</sup> to the world the elegant and simple formulas of the "*Theoria Motus*." No elliptic elements were computed, therefore, until the year 1783, during which year elliptic orbits differing but slightly from each other were published by Méchain,<sup>2</sup> Laplace,<sup>3</sup> Caluso,<sup>4</sup> and Hennert;<sup>5</sup> and in the French and German astronomical Ephemerides for 1787 (published in 1784) were tables of the new planet. The name Uranus, originally proposed<sup>6</sup> by Bode, had at that time become almost universal upon the Continent, although in England the names "Herschel"<sup>7</sup> and "Georgium Sidus"<sup>8</sup> (or simply "The Georgian") were generally used until within a very few years, — the planet being still designated by the latter name in the British Nautical Almanac. The symbol adopted<sup>9</sup> with the name Uranus was that of platinum ( $\text{⌘}$ ), but in England and France the symbol  $\text{♅}$ , formed from the discoverer's initial, is generally used.

In the mean time Bode, the Astronomer Royal of Prussia, had suggested<sup>10</sup> that Uranus might have been observed by astronomers before the discovery of its planetary nature, and consequently that earlier observations might be found by a proper search in the catalogues of fixed stars. This happy idea prompted him to study over the old star-catalogues, and his search was crowned with abundant success.<sup>11</sup> In August, 1781,

<sup>1</sup> March, 1809.

<sup>2</sup> *Hist. Acad. Berl.*, 1782, pp. 41, 49.

<sup>3</sup> *Mém. de Bruxelles*, T. 5, *Hist.*, p. xlix., *Mém.*, p. 43; *Journal de Paris*, May 31, 1783; *Berl. Ast. Jahrb.*, 1786, p. 247; *Conn. des Temps*, 1786, p. 3.

<sup>4</sup> *Ephem. Astron. Mediol.*, 1784, p. 199.

<sup>5</sup> *Berl. Ast. Jahrb.*, 1786, p. 223.

<sup>6</sup> *Berl. Gesellschaft Naturforschender Freunde*, March 12, 1782, III. p. 350.

<sup>7</sup> Proposed by Lalande, Dec. 22, 1781; v. *Mém. Acad. Paris*, 1779, p. 526; *Hist. Acad. Berlin*, 1782, p. 39.

<sup>8</sup> Proposed by Herschel, *Roy. Soc. Philos. Trans.*, 1783, p. 1.

<sup>9</sup> Proposed by Koehler, of Dresden; *Berl. Ast. Jahrb.*, 1785, p. 191; *Nova Acta Petrop.*, I. 69.

<sup>10</sup> *Berl. Ast. Jahrb.*, 1784, p. 218.

<sup>11</sup> See also Bode, *Vom neuentdeckten Planeten*, Berl., 1784.



he discovered<sup>1</sup> that a star<sup>2</sup> (No. 964) in Tobias Mayer's Catalogue, which had been observed<sup>3</sup> September 25, 1756, was not to be found in the place indicated, and that it had not been mentioned on various occasions when all the other stars of equal magnitude in the same vicinity had been observed. Uranus must have been nearly in that place at the same time, according to the orbits of Laplace and Méchain,<sup>4</sup> and the presumption became thus quite strong, that this supposed fixed star of Mayer was really the planet Uranus. In the same way Bode and Fixlmillner of Kremsmünster found,<sup>5</sup> in 1784, that a star observed<sup>6</sup> by Flamsteed, December  $\frac{2}{1}\frac{2}{3}$ , 1690, and called by him 34 Tauri, was in all probability also Uranus. The same discovery seems to have been made and verified<sup>7</sup> independently in France by Lemonnier and Montaigne. Observations of the planet were thus obtained, which embraced an interval of more than an entire revolution, and from these two old observations in 1690 and 1756, and the two oppositions of 1781 and 1783, Fixlmillner computed elliptic elements,<sup>8</sup> which not only fully satisfied all the four places upon which they were based, but all the observations known. They were as follows: —

*Epoch, Jan. 1st, 1784.*

	$^{\circ}$	$'$	$''$		days.
Mean anomaly,	297	9	25	Tropical period,	30587.37
Long. perihelion,	167	31	33	Mean distance,	19.18254
Long. asc. node,	72	50	50	Eccentricity,	0.0461183
Inclination,	0	46	20	Mean daily trop. motion,	42".3704

which, as the event has proved, were very near the truth.

<sup>1</sup> *Berl. Ast. Jahrb.*, 1784, p. 219; 1785, p. 189; Wurm, *Geschichte des Uranus*, p. 35.

<sup>2</sup> *Reduced Conn. des Temps*, 1778, p. 195; Fixlmillner, *Berl. Acad.*, 1783, *Hist.*, p. 15; Bessel, *Fundamenta Astronomiæ*, p. 283.

<sup>3</sup> Mayer, *Opera Inedita*, ed. Lichtenberg, I. p. 72.

<sup>4</sup> *Hist. Acad. Berl.*, 1782, p. 40.

<sup>5</sup> *Hist. Acad. Berl.*, 1783, p. 15; *Berl. Astr. Jahrb.*, 1787, pp. 243, 247.

<sup>6</sup> *Hist. Cælestis Britann.*, II. p. 86, 2d ed.

<sup>7</sup> *Mém. Acad. Roy. des Sciences*, 1784, p. 353.

<sup>8</sup> *Hist. Acad. Berl.*, 1783, p. 19; *Berl. Astr. Jahrb.*, 1787, p. 249.

Wurm, of Nürtingen, computed,<sup>1</sup> at nearly the same time,<sup>2</sup> elements<sup>3</sup> very similar; and Fixlmillner,<sup>4</sup> von Zach,<sup>5</sup> and others,<sup>6</sup> constructed tables. Those by the former are in the Berlin Astronomical Almanac for 1789 (published 1786). These tables, as above remarked, satisfied all the observations, and continued to represent the planet's course most satisfactorily until 1788, when a discrepancy between theory and observation became very apparent;<sup>7</sup> and Fixlmillner was compelled to disregard Flamsteed's observation, and to calculate new elements and tables<sup>8</sup> from the oppositions since Herschel's discovery, and the single observation by Mayer. But at a later period, after Gerstner,<sup>9</sup> Lalande,<sup>10</sup> Oriani,<sup>11</sup> and Duval<sup>12</sup> had determined the perturbations by Saturn and Jupiter, and Delambre had published<sup>13</sup> his tables of Uranus, the discrepancies vanished, and the same elements were made to represent perfectly all the modern observations, and the two former ones of Mayer and Flamsteed. In 1788, Lemonnier discovered<sup>14</sup> that he had also observed<sup>15</sup> Uranus as a fixed star in 1764 and 1769. These observations of Uranus, made prior to Herschel's discovery of its planetary character, are called, by way of distinction, "ancient observations." Others have since been found, so that we now have twenty in all, viz.:—

<sup>1</sup> *Geschichte des Planeten Uranus*, p. 37; *Berl. Astr. Jahrb.*, 1788, p. 193.

<sup>2</sup> Summer of 1784.

<sup>3</sup> See Reggio, *Ephem. Astr. Mediol.*, 1784, p. 199.

<sup>4</sup> *Berl. Astr. Jahrb.*, 1789, p. 113.

<sup>5</sup> *Ibid.*, 1788, p. 217.

<sup>6</sup> Caluso, *Mém. de Turin*, 1787, pp. 113, 132, 137; Dom Nouet, *Conn. des Temps*, 1787, p. 176; Robison, *Trans. R. Soc. Edinb.*, 1788, Vol. I. p. 305.

<sup>7</sup> Wurm, *Gesch. des Uranus*, p. 38.

<sup>8</sup> *Berl. Astr. Jahrb.*, 1792, p. 159.

<sup>9</sup> *Ibid.*, 1792, pp. 214, 219.

<sup>10</sup> *Mém. de l'Acad. Sc. Paris*, 1787, p. 182.

<sup>11</sup> *Ephem. Astr. Mediol.*, 1790, 1791.

<sup>12</sup> *Mém. Acad. Berl.*, 1789; *Berl. Astr. Jahrb.*, 1793, 115.

<sup>13</sup> Computed 1789, crowned 1790. Wurm, *Geschichte des Uranus*, p. 89; Lalande, *Astronomie*, ed. 3<sup>me</sup>, Vol. I.

<sup>14</sup> See Lalande, *Acad. Sc. Paris*, 1789, p. 204; von Zach, *Comm. Soc. Reg. Gotting.*, 1789, p. 91.

<sup>15</sup> *Conn. des Temps*, 1821, p. 339.

- One<sup>1</sup> in 1690, by Flamsteed, Dec.  $\frac{2}{3}$ .  
 One<sup>2</sup> " 1712, " Flamsteed,  $\frac{\text{Apr. } 2}{\text{Mar. } 22}$ .  
 Four<sup>3</sup> " 1715, " Flamsteed,  $\frac{\text{March } 4, 5, 10, \text{ Apr. } \frac{29}{18}}{\text{Feb. } 21, 22, 27}$ .  
 Two<sup>4</sup> " 1750, " Lemonnier, Oct. 14, Dec. 3.  
 One<sup>5</sup> " 1753, " Bradley, Dec. 3.  
 One<sup>6</sup> " 1756, " Mayer, Sept. 25.  
 One<sup>7</sup> " 1764, " Lemonnier, Jan. 15.  
 Two<sup>8</sup> " 1768, " Lemonnier, Dec. 27, 30.  
 Six<sup>9</sup> " 1769, " Lemonnier, Jan. 15, 16, 20, 21, 22, 23.  
 One<sup>10</sup> " 1771, " Lemonnier, Dec. 18.

Mr. Le Verrier has, however, found reason<sup>11</sup> to suspect the accuracy of Flamsteed's observation of  $\frac{\text{March } 5}{\text{Feb. } 22}$ , 1715, so that there are really but nineteen available ones.

The best tables of Uranus which existed before the masterly and accurate researches<sup>12</sup> of Le Verrier, in 1845 and 1846, were those<sup>13</sup> computed by Bouvard in 1821. Bouvard was acquainted with all the ancient observations which we know, excepting three by Flamsteed in 1715. In the introduction to his tables, he announced<sup>14</sup> that he had been utterly unable to find any elliptic orbit, which, combined with the perturbations by Jupiter and Saturn, would represent both the ancient and the modern observations. The best tables which he could obtain by the

<sup>1</sup> *Hist. Cæl. Brit.*, II. 86, 2d ed.; *Hist. Acad. Berl.*, 1783, p. 16.

<sup>2</sup> *Hist. Cæl. Brit.*, II. p. 537.

<sup>3</sup> *Ibid.*, pp. 549, 551; *Conn. des Temps*, 1820, pp. 409, 410.

<sup>4</sup> *Conn. des Temps*, 1821, p. 339.

<sup>5</sup> Bradley, *Astron. Obs.*, I. p. 155; Bessel, *Fund. Astr.*, p. 283; *Greenwich Plan. Reduct.*, I. p. 300.

<sup>6</sup> *Hist. Acad. Berl.*, 1783, p. 8; Bessel, *Fund. Astron.*, p. 284; *Conn. des Temps*, 1778, p. 195.

<sup>7</sup> Bouvard, *Conn. des Temps*, 1821, pp. 341, 342.

<sup>8</sup> *Ibid.*, pp. 341 - 343.

<sup>9</sup> *Ibid.*, pp. 341, 343 - 347.

<sup>10</sup> *Ibid.*, pp. 341, 347.

<sup>11</sup> *App. Conn. des Temps*, 1849, p. 125.

<sup>12</sup> *Comptes Rendus de l'Acad. des Sc.*, XXI. p. 1050; XXII. p. 907.

<sup>13</sup> *Tables Astronomiques*, Paris, 1821.

<sup>14</sup> p. ii.

use of both represented neither of them, in any way at all satisfactory. On the other hand, by using modern observations only, he was enabled to find elements which, although they gave errors amounting sometimes to 74" for the ancient observations, still satisfied all the modern ones comparatively well, — never differing more than 10" from theory, and generally much less.

"Such," said he,<sup>1</sup> "is the alternative which the formation of tables of the planet Uranus presents; — if we combine the ancient observations with the modern ones, the first will be passably represented, while the second will not be represented with the precision which they require; — but if we reject the former, and retain the latter only, the resultant tables will have all desirable precision for the modern observations, but will be incapable of properly satisfying the ancient ones. We must choose between two alternatives. I have thought it proper to abide by the second, as being that which combines the most probabilities in favor of the truth, and I leave it to the future to make known whether the difficulty of reconciling the two systems result from the inaccuracy of the ancient observations, or whether it depend upon some extraneous and unknown influence, which has acted on the planet."

He therefore summarily rejected the former observations, and founded his tables upon the latter alone, adducing arguments against the accuracy of the ancient observations, and forgetting how well they harmonized with one another, and had harmonized with the elements obtained soon after the discovery of the planet.

But a very few years after the publication of Bouvard's tables, important differences between theory and observation became<sup>2</sup> again manifest, and attracted the attention of astronomers.

Airy alluded,<sup>3</sup> in 1832, to these discrepancies, in his Report

<sup>1</sup> p. xiv.

<sup>2</sup> See *Ast. Nachr.*, IV. p. 56; VI. pp. 195, 209; *Königsberg Observ.*, 1826; *Cambridge (Eng.) Observ.*, 1826, 1830.

<sup>3</sup> *Reports of British Association*, I. p. 154.



to the British Association on the Progress of Astronomy, and mentioned that the tables, constructed only eleven years previously, were in error nearly half a minute of arc.

It is an easy thing to censure Bouvard for the readiness with which he abandoned the ancient observations, — now that we know that the discrepancies were caused by the action of an exterior planet, and that the maximum of error in the ancient observations amounted<sup>1</sup> only to nine seconds. But the illustrious Bessel, who, had his priceless life been spared a very little longer, would have seen his suspicions most fully confirmed, spoke<sup>2</sup> as follows, in a public lecture,<sup>3</sup> in the beginning of the year 1840, six years and a half before the discovery of Neptune: —

“In my opinion, Bouvard made much too light of the matter; inasmuch as, after he had found himself unable to reconcile the theory both with the ancient observations and the forty years’ series of modern ones, he contented himself with the remark, that the former were not so accurate as the latter. I have myself subjected them to a more careful investigation, and new calculation, and have thereby attained the full conviction, that the existing differences, which, in some cases, exceed a whole minute, are by no means to be attributed to the observations.”

Bessel had already<sup>4</sup> pointed out one error in Bouvard’s tables, in the equation depending on the mean longitude of Saturn minus twice that of Uranus. This error was small in comparison with the discordances to be accounted for, — but we thus see how early he was engaged upon the investigation.

The last sentence of Bouvard’s introduction, just quoted, which was, in fact, the first published suggestion that the discordance between the theory and observations of Uranus might be due to the influence of an unknown planet, is hardly definite enough to be viewed historically in that light. But the follow-

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<sup>1</sup> *Proc. Amer. Acad.*, I. p. 333.

<sup>2</sup> Bessel, *Populäre Vorlesungen*, p. 448.

<sup>3</sup> In Königsberg, Feb. 28th, 1840.

<sup>4</sup> *Astron. Nachrichten*, No. 48, II. p. 441.

ing extract from a letter,<sup>1</sup> written November 17, 1834, by the Rev. Dr. T. J. Hussey, of Hayes, to Prof. Airy, — now English Astronomer Royal and then Director of the Observatory of Cambridge, England, — although first published<sup>2</sup> since the discovery of Neptune, is the earliest written allusion to the subject with which I am acquainted.

“The apparently inexplicable discrepancies between the ancient and modern observations suggested to me the possibility of some disturbing body beyond Uranus, not taken into account, because unknown. . . . Subsequently, in conversation with Bouvard, I inquired if the above might not be the case: his answer was, that, as might have been expected, it had occurred to him, and some correspondence had taken place between Hansen and himself respecting it. Hansen’s opinion was, that one disturbing body would not satisfy the phenomena; but that he conjectured there were two planets beyond Uranus. Upon my speaking of obtaining the places empirically, and then sweeping closely for the bodies, he fully acquiesced in the propriety of it, intimating that the previous calculations would be more laborious than difficult; that, if he had leisure, he would undertake them and transmit the results to me, as the basis of a very close and accurate sweep. . . . I may be wrong, but I am disposed to think, that, such is the perfection of my equatorial’s object-glass, I could distinguish, almost at once, the difference of light between a small planet and a star. . . . If the whole matter do not appear to you a chimera, which, until my conversation with Bouvard, I was afraid it might, I shall be very glad of any sort of hint respecting it.”

As regards the opinion attributed in this letter to Prof. Hansen, I have the authority\* of that eminent astronomer himself

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\* Prof. Hansen informs me, in a letter received since this account was written, that Mr. von Lindenau has been engaged upon a history of the discovery of Neptune. To his inquiries upon this subject, Prof. Hansen had replied in the following words, which he has also authorized me to make public:—

“Die mich betreffende Aeusserung in Airy’s Aufsätze (*Astr. Nachr.*, No. 585,

<sup>1</sup> *Notices R. Ast. Soc.*, VII. p. 123.

<sup>2</sup> Nov. 13, 1846.

for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief.

In the *Notices* of the Royal Astronomical Society for November 13th, 1846, is a most important publication<sup>1</sup> by the Astronomer Royal, entitled, "An Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus." In this paper is a series of extracts from letters, before unpublished, which furnish the testimony to a great part of the history anterior to the actual discovery. From the first letter<sup>2</sup> in the series, the preceding quotation was made.

Mr. Eugene Bouvard, nephew of the author of the Tables, wrote as follows<sup>3</sup> on the 6th October, 1837, from Paris, to the Astronomer Royal of England: — "My uncle has given me the tables of Uranus to reconstruct. In consulting the comparisons

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s. 136, und *R. Ast. Soc.*, VII. p. 123) ist jedenfalls unrichtig. Ich besitze freilich keine Copien von meinen Briefen an Bouvard, aber ich finde in seinen Briefen an mich, die ich wieder durchlas, als mir Airy's Aufsatz bekannt wurde, keine Spur, dass ich gegen ihn die von Hussey mir in den Mund gelegte Aeusserung gemacht hätte. Auch war dies unmöglich, da ich meine darauf bezüglichen Arbeiten nicht so weit fortgesetzt habe, um darüber eine bestimmte Ansicht haben zu können. Ich kann möglicher Weise geschrieben haben, dass *vielleicht* die bis dahin in der Bewegung des Uranus nicht erklärten Abweichungen von der Theorie nicht von einem, sondern von mehreren auf ihn einwirkenden, unbekannten Planeten herrührten; aber dass *Ein* störender Körper die Abweichung *nicht* erklären könne, habe ich auf keinen Fall behauptet. Airy, dem ich kurz nach dem Erscheinen seines Aufsatzes darüber schrieb, antwortete, dass er in seinem nächsten Artikel über Neptun davon Gebrauch machen wollte. Es wäre mir lieb, wenn dieses auch in dem Ihrigen geschähe. Immer ist es meine feste Ansicht gewesen, dass die Anomalien in der Bewegung des Uranus nichts anders sind, als die Wirkung eines oder mehrerer oberhalb befindlichen Planeten, und ich befand mich darüber in Opposition mit Bessel und Nicolai, welche beide dieses für unmöglich hielten. Ersterer ist indess in seinen letzten Lebensjahren von dieser Ansicht abgegangen, und hat sich ernstlich mit der Aufsuchung dieses Planeten durch Rechnung beschäftigt. Ich hatte in den zwanziger Jahren angefangen mich mit dieser Aufgabe zu beschäftigen, — gab die Arbeit aber wieder auf, und habe sie seitdem wegen anderer Untersuchungen ganz aus dem Gesicht verloren. Eine Stelle, wo LeVerrier sagt dass die Uranusbeobachtungen der letzten 20 Jahren nothwendig waren, um ein sicheres Resultat zu erlangen, lässt es mich nicht bereuen, damals meine Arbeit liegen gelassen zu haben."

<sup>1</sup> *Notices R. Astron. Soc.*, VII. p. 121.

<sup>2</sup> *Ibid.*, p. 123.

<sup>3</sup> *Ibid*, p. 125.

which you have made between observations of this planet and the calculations in the tables, it will be seen that the differences in latitude are very large, and are continually becoming larger. Does this indicate an unknown perturbation exercised upon the motions of this star by a body situated beyond? \* I do not know, but this is at least my uncle's idea."

Prof. Airy remarked in his reply,<sup>1</sup> that the error in latitude was very small; — that it was the errors in the longitude which were increasing with so fearful rapidity. And, a few months after, he showed<sup>2</sup> that the tabular radius-vector of Uranus was much too small. This result of observations at the quadratures was one to which Prof. Airy, both at that time and uniformly since, attached great importance.

It is from this period, that the definite belief of most astronomers in the existence of a trans-Uranian planet appears to date. Numerous mathematicians subsequently conceived the purpose of entering earnestly into laborious and precise calculations, in order to decide whether the assumption of an exterior cause of disturbance were absolutely necessary, and, if so, to determine from the known perturbations their unknown cause. The Astronomer Royal most justly expresses<sup>3</sup> himself "confident, that it will be found that the discovery is a consequence of what may properly be called a movement of the age; that it has been urged by the feeling of the scientific world in general, and has been nearly perfected by the collateral, but independent, labors of various persons possessing the talents or powers best suited to the different parts of the researches."

The problem became, from this time forth, one of the most important questions of Physical Astronomy. Astronomers in various countries busied themselves with it, and spoke of it without reserve.

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\* "Cela tient-il à une perturbation inconnue apportée dans les mouvements de cet astre par un corps situé au-delà?"

<sup>1</sup> *Notices R. Astron. Soc.*, VII. p. 126.    <sup>3</sup> *Notices R. Astr. Soc.*, VII. p. 122.

<sup>2</sup> *Astr. Nachr.*, No. 349, XV. p. 217.



The first decided opinion, publicly expressed, was, I believe, that of Bessel, in the lecture <sup>1</sup> delivered at Königsberg on the 28th February, 1840 ; from which I have already quoted. He had, at that time, already repeated the calculations of the elder Bouvard, and convinced himself that the ancient and modern observations could not be reconciled by any modification of the elements ; and that the differences could not be attributed to inaccuracy of instruments, or to methods of observation.

“ From all these investigations,” said he, <sup>2</sup> “ I have arrived at the full conviction, that we have in Uranus a case to which Laplace’s assertion \* is not applicable. . . . We have here to do with discordances, whose explanation can only be found in a new physical discovery. . . . Farther attempts to explain them must be based upon the endeavor to discover an orbit and a mass for some unknown planet, of such a nature, that the resulting perturbations of Uranus may reconcile the present want of harmony in the observations. If the motions of Uranus can actually be explained in this way, the lapse of time will raise this explanation to the rank of evidence, in the same degree in which it will exhibit the influence of this new power. And this, too, must exert influences upon the motion of Saturn, which are indeed smaller, but will, nevertheless, hardly be able to escape a special investigation, and will thus afford an independent confirmation of the existence of the new planet.”

In continuing his lecture, Bessel dwelt long upon the labor and difficulty of the problem. He spoke of the painful amount of preparatory labor to be performed, alluded <sup>3</sup> to the patience and care with which his young friend and pupil, Flemming, had already reduced all the observations of Uranus, and, with fatherly kindness, he encouraged him in his work.

The labors of the talented and enthusiastic Flemming were

\* *i. e.*, that the theory of gravitation entirely explained all motions observed in our solar system.

<sup>1</sup> Bessel, *Popul. Vorles.*, p. 408.

<sup>3</sup> *Ibid.*, p. 452.

<sup>2</sup> *Ibid.*, pp. 449, 450.

interrupted by his early death. Bessel himself was upon the point of commencing the investigation which he had proposed, when he sank beneath the weight of his labors ; and “ the prize toward which he had stretched forth his hand was wrested from him ”<sup>1</sup> by the lingering and painful disease which closed the long series of his brilliant investigations, and removed him from the world.

Sir John Herschel stated publicly,<sup>2</sup> soon after the discovery of Neptune, that Bessel, while in England, in 1842, conversed with him concerning the probability that the motions of Uranus were disturbed by an exterior planet, and later in the same year, in a letter from Königsberg, intimated that he was engaged in researches relative to these perturbations. Professor Schumacher<sup>3</sup> has promised to publish Flemming’s reduction of the Uranus-observations in the *Astronomische Nachrichten*.

Meantime Mr. Eugene Bouvard, in Paris, had been engaged upon the same labors. In May, 1844, he wrote again<sup>4</sup> to Mr. Airy, that he had not only reduced the observations of Uranus anew, but that he had reconstructed tables, and succeeded in satisfying the modern observations so nearly that the extreme of discordance amounted only to fifteen seconds, while the tables of his uncle gave places nearly two minutes out of the way.

But it must be observed, on the other hand, that the ancient observations were not represented with any degree of accuracy ; and, as regards the modern ones, fifteen seconds, although but a small angle, — a single second of time, — is nevertheless five times the amount of error which we are warranted in assuming.<sup>5</sup>

Thus not only the investigations of Bessel previous to 1840, but the entirely independent ones of Bouvard, between 1837 and 1844, showed the impossibility of explaining all the observed motions of Uranus by known causes. The Royal Society of

<sup>1</sup> Schumacher, *Introduction to Bessel’s Popular Lectures*, p. iv.

<sup>2</sup> *Letter to London Athenæum*, Oct. 3, 1846.

<sup>3</sup> *Introduction to Bessel’s Lectures*, p. iv.

<sup>4</sup> Airy’s *Account*, letter No. 5, *Notices Astr. Soc.*, VII. p. 127.

<sup>5</sup> *Proceedings Amer. Acad.*, I. p. 333.

Sciences of Göttingen had already proposed, as a prize-question,<sup>1</sup> in 1842, the full discussion of the theory of the motions of Uranus, with special reference to the cause of the large and increasing errors of Bouvard's tables.

The death of Flemming and the feebleness and illness of Bessel put a stop to their researches in Germany; and from this period we know of but two mathematicians, Messrs. Adams, of Cambridge University, in England, and Le Verrier, in Paris, who busied themselves with the problem. Mr. Adams states<sup>2</sup> that his attention was first called to the subject by reading, in the summer of 1841, Mr. Airy's Report on the Progress of Astronomy. Mr. Le Verrier undertook<sup>3</sup> the investigation at the request of Arago, the Director of the Observatory of Paris. That eminent scientist had doubtless been impressed by the computations of Bouvard with the necessity of the research; and the Astronomer Royal expresses<sup>4</sup> his conviction, that the knowledge of Bouvard's labors "tended greatly to impress upon astronomers, both French and English, the absolute necessity of seeking some external cause of disturbance."

Three months before the date of the last-cited letter of Bouvard, Mr. Adams had informed Mr. Airy, through Prof. Challis, that he was engaged in researches of a precisely similar nature. The letter<sup>5</sup> of Prof. Challis is dated February, 1844, and was written to obtain the reductions of the Uranus-observations which had been made at Greenwich. Airy immediately<sup>6</sup> forwarded the complete series of heliocentric errors of the Uranus-tables, — both in longitude and latitude, — for all the Greenwich observations from 1754 to 1830.

During the summer of 1845, Mr. Arago represented, as above stated, to Mr. Le Verrier, then colleague of the venerable

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<sup>1</sup> *Abhandl. d. Königl. Gesellschaft*, II. p. x.

<sup>2</sup> Adams's "*Explanation*," p. 3, *Naut. Alm.*, 1851.

<sup>3</sup> Le Verrier's "*Recherches*," p. 4, *Conn. des Temps*, 1849.

<sup>4</sup> "*Account*," *Ast. Soc. Notices*, VII. p. 127.

<sup>5</sup> "*Account*," letter No. 6, p. 128.

<sup>6</sup> *Ibid.*, letter No. 7, p. 128.

and illustrious Biot, the transcendent importance of the question, and urgently pressed him to enter into a full discussion of the subject. Le Verrier did this, and presented his first paper <sup>1</sup> to the Academy of Sciences on the 10th November, 1845.

A fortnight previous, Mr. Adams had communicated <sup>2</sup> to Mr. Airy, that, according to his calculations, the observed inequalities of Uranus might be explained by supposing an exterior planet with a mass and orbit as follows : —

Mean distance (assumed nearly in accordance with Bode's law), . . . . .	38.4
Mean sidereal motion in 365.25 days, . . . . .	1° 30' 9"
Mean longitude, October 1st, 1845, . . . . .	323° 34'
Longitude of the perihelion, . . . . .	315° 55'
Eccentricity, . . . . .	0.1610
Mass, that of the sun being unity, . . . . .	0.0001656

With these elements Mr. Adams gave a table of remaining errors of longitude for every three years of the modern observations, none of which exceeded two and a quarter seconds. The ancient observations were satisfied within 12'', excepting Flamsteed's in 1690, which differed by 44".4, not having been used in the equations of condition. It was, however, probable, he thought, that a small change in the mean motion of the hypothetical planet would reduce this discordance materially.

Prof. Airy, in replying, desired <sup>3</sup> to know whether this assumed perturbation would explain the error of the radius-vector, as he considered that this trial would be an *experimentum crucis*. For some reason, no answer was received. This is deeply to be regretted, as, had an affirmative answer been given, Airy would undoubtedly <sup>4</sup> have procured the publication of Mr. Adams's results, and the painful discussion concerning priority would have been spared. As the case now stands, the question of priority depends upon another question, — whether Adams's communication of his results to the Astronomer Royal can be considered as a publication of them.

<sup>1</sup> *Comptes Rendus*, XXI. p. 1050.

<sup>3</sup> "Account," letter No. 12, p. 130.

<sup>2</sup> Airy's *Account*, letter No. 11, p. 129.

<sup>4</sup> *Ibid.*, p. 131.



The next letter of Mr. Adams, which has been printed,<sup>1</sup> is dated September 2, 1846. Le Verrier had, in the mean time, not only published<sup>2</sup> the memoir already alluded to, in which the perturbations of Uranus by Jupiter and Saturn are fully developed, calculated, and discussed, — but had communicated to the Academy two other most important papers. In one,<sup>3</sup> presented on June 1st, 1846, he proved<sup>4</sup> that the motions of Uranus could not be accounted for, except by introducing the perturbative influence of an unknown planet, for which he assigned an approximate place. In the other,<sup>5</sup> he found an orbit, a mass, and a more precise position for the disturbing planet. This was presented on the 31st August.

Mr. Airy mentions,<sup>6</sup> that on the 29th June, at a meeting of the Board of Visitors of the Greenwich Observatory, at which Sir John Herschel and Prof. Challis were present, he spoke of the extreme probability that another planet would be discovered within a short time; and assigned, as a reason for this belief, the coincidence between Mr. Le Verrier's results and those of Mr. Adams. He had addressed<sup>7</sup> a letter to Mr. Le Verrier, similar to that sent a year previously to Mr. Adams, to make inquiries about the radius-vector. Mr. Le Verrier answered<sup>8</sup> under date of June 28, stating that the errors of radius-vector must be accounted for, inasmuch as the equations of condition depended on observations at the quadratures as well as at the oppositions. Concerning the correctness of this inference, however, there appears room for discussion. Le Verrier called Airy's attention to the fact, that the position in quadrature in 1844, deduced by means of his formulas from the two oppositions which comprised it, only differed  $0''.6$  from the observed position, which proved, he said, that the error of radius-vector had entirely disappeared. This he considered as one of the strongest arguments in favor of the truth of his results. For,

<sup>1</sup> "Account," letter No. 20, p. 137.

<sup>2</sup> *Comptes Rendus*, XXI. p. 1050.

<sup>3</sup> *Ibid.*, XXII. p. 907.

<sup>4</sup> *Ibid.*, p. 911.

<sup>5</sup> *Comptes Rendus*, XXIII. p. 428.

<sup>6</sup> "Account," p. 133.

<sup>7</sup> Letter No. 13, p. 132.

<sup>8</sup> Letter No. 14, p. 133.

while in his first researches he only made use of oppositions, the quadratures were represented with all precision. "Le rayon vecteur," said <sup>1</sup> he, "s'est trouvé rectifié de lui-même sans que l'on l'eut pris en considération d'une manière directe. Excusez-moi, Monsieur, d'insister sur ce point. C'est une suite du desir que j'ai d'obtenir votre suffrage."

At Airy's suggestion,<sup>2</sup> Professor Challis had already commenced<sup>3</sup> a search for the planet on the 29th July, using a modification of a plan which Mr. Airy had drawn up. The date of the letter suggesting this search was July 9; that of the general plan was July 13. Le Verrier's memoir,<sup>4</sup> which assigned  $325^{\circ}$  as the probable longitude of the planet, was presented to the French Institute, as we have seen, on June 1st. Still, it does not appear that any search whatever had been instituted in the intervening time in any part of Europe or America;—indeed, there is no account of any search having been made excepting by Professor Challis, before the night of September 23.

It must, indeed, be confessed that astronomers in general did not seem to consider the theoretical results, published by Mr. Le Verrier, as necessarily indicating the *physical* existence and true position of such an exterior planet. Professor Challis alone—the only astronomer who entered into a systematic search for the planet, and the only one excepting Dr. Galle, the assistant at the Royal Observatory of Berlin, whom we know to have even looked for it—has assigned,<sup>5</sup> as a reason which deterred him from an earlier search, that it was "so novel a thing to undertake observations in reliance upon merely theoretical deductions; and that, while much labor was certain, success appeared very doubtful." But is there any practical astronomer, in a latitude permitting the search, who was not deterred by the same considerations. Even in Paris, that focus of science, with its many and powerful telescopes, with its numerous

<sup>1</sup> "Account," letter No. 14, p. 134.

<sup>2</sup> Ibid., pp. 135, 136.

<sup>3</sup> *Ast. Soc. Notices*, VII. p. 145.

<sup>4</sup> *Comptes Rendus*, XXII. p. 917.

<sup>5</sup> *Ast. Soc. Notices*, VII. p. 145.

eminent astronomers, where Mr. Le Verrier was known and his brilliant genius appreciated, — not to allude to the American observatories, furnished with some of the finest and most powerful telescopes which have left the *ateliers* of Munich, — we have no information that any attempts\* were made to test the physical accuracy of Le Verrier's results, or that the planet was even looked for on one single evening.

A strange contrast to this apathy on the part of other astronomers is furnished by the demeanor of Le Verrier himself. Having fairly arrived at his results, he looked upon them as conclusive. His computations had been an earnest work. He had employed all his analytical powers, — and employed them, too, most successfully, — to refine the methods which he used, and to narrow the field of his inquiry; all his powers of application and numerical research, to insure precision; — and his indomitable perseverance, in carrying out his computations with full vigor, permitted him to omit no possible test of their accuracy. He proved that the observations of Uranus made it necessary to assume the existence of some unknown disturbing body. For the observations which he adopted as the basis of his calculations, he had assigned, *à priori*, the limits of error allowable; and he found that all the observations could be satisfied within these predetermined limits by the assumption of an exterior planet, moving in a given orbit, and having a given mass. The correctness of his results was dependent upon no empirical assumption. He gave them, therefore, fearlessly to the world, and staked his reputation upon their accuracy. This forms by no means the least part of his claims to the respect and admiration of scientists throughout the world. Had the planet not been found in the predicted place, Le Verrier would alone have borne the mortification. Neptune was discovered in almost precisely the direction assigned, and Le Verrier receives the admiration so justly due him.

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\* It is, perhaps, due to the Naval Observatory at Washington, to make some exception in its favor. *Astr. Nachr.*, XXVI. p. 65.

The mass and orbit given in the memoir of August 31st are<sup>1</sup> as follows:—

Semimajor axis, . . . . .	36.1539
Sidereal period, . . . . .	217 <sup> yrs.</sup> .387
Eccentricity, . . . . .	0.10761
Equation of the center, . . . . .	7° 44' 44"
Longitude of perihelion, January 1, 1800, . . . . .	284 5 48
Mean longitude, . . . . .	240 17 41
Precession in 47 years, . . . . .	0 39 20
Mean sidereal motion in 47 years, . . . . .	77 50 3
Mean anomaly, 1847, January 1, . . . . .	34 1 56
Mean longitude, . . . . .	318 47 4
Mass, . . . . .	9322

The geocentric longitude, resulting from this orbit, for the end of September, 1846, was 325°. Le Verrier, in acknowledging the receipt of a memoir, made use of the opportunity thus afforded,<sup>2</sup> to request Dr. Galle to look for the planet. The letter reached Berlin<sup>3</sup> on the 23d September, and Galle, in complying with this request, found, on the same evening, a new planet in longitude 325° 53', or within 55' of the geocentric place<sup>4</sup> assigned by Mr. Le Verrier.

The remembrance of the enthusiasm excited by this discovery, of the amazement with which the tidings were received, not only by astronomers, but by almost all classes of the community, and of the homage paid to the genius of Le Verrier, is still fresh in the memory of all. Nations vied with one another in expressions of their admiration.<sup>5</sup> Arago, to whom the right of conferring a name upon the new planet was delegated<sup>6</sup> by Le Verrier, gave<sup>7</sup> to the planet the name of that geometer, with the symbol  $\mathbb{E}$ , deduced from the initials of the same. This

<sup>1</sup> *Recherches*, pp. 234–236; *Comptes Rendus*, XXIII. p. 432.

<sup>2</sup> *Astr. Nachr.*, XXV. p. 51.

<sup>3</sup> *Berl. Vossische Zeitung*, Sept. 25th, 1846.

<sup>4</sup> 324° 58'.

<sup>5</sup> *Comptes Rendus*, XXIII. pp. 959–960, etc.

<sup>6</sup> *Ibid.*, p. 662.

<sup>7</sup> *Astr. Nachr.*, XXV. p. 81.



name and symbol have not, however, generally prevailed,\* as they are at variance with the received nomenclature, in accordance with which the names of Roman deities have been uniformly selected; and the name *Neptune*, which, with its appropriate symbol ( $\Psi$ ), a trident, was originally proposed<sup>1</sup> by the *Bureau des Longitudes*, and immediately adopted<sup>2</sup> by Gauss, Struve, Encke, and Airy, has become almost universal.

The first public announcement of Mr. Adams's labors was in a London newspaper,<sup>3</sup> the *Athenæum*. In this journal, under date of October 1st, 1846, Sir John Herschel, commenting on the actual detection at Berlin of the long-expected planet, spoke, as before quoted, of Bessel's conversation with him, and subsequent letter in 1842, and alluded to the fact that Mr. Adams<sup>4</sup> had been engaged in an investigation similar to that of Mr. Le Verrier, and with similar results.

On the 13th November, the Astronomer Royal presented to the Astronomical Society of London the extremely valuable and important Account which has been so often quoted in my Report, and which must ever remain an integral part of this singular history. Professor Challis presented at the same time an ac-

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\* It seems quite desirable that astronomers in different countries should be unanimous in the adoption of fixed names and symbols. For each of the two planets of which this Report speaks, two names and two symbols are in use. The great preponderance of authority seems, however, decidedly in favor of the mythological names used in this Report, with their corresponding symbols,  $\odot$  and  $\Psi$ . Le Verrier, who in 1846 announced<sup>5</sup> his fixed determination to call Uranus by the name of its discoverer, according to Lalande's proposition, has now happily abandoned<sup>6</sup> this unpopular idea; and even the distinguished Arago, who bestowed the name of the French geometer upon the new planet discovered in consequence of his computations, and publicly declared<sup>7</sup> his determination never to call it by any other name than Le Verrier, has yielded to the general usage of astronomers.

<sup>1</sup> Le Verrier, letter to Galle, Oct. 1, 1846, *Ast. Nachr.*, XXV. p. 194.

<sup>2</sup> Gauss, letter to Encke, Oct. 7, 1846, *Ibid.* Struve, *Bulletin Imp. Acad. Petersburg*, Dec. 27, 1846. Challis, *Astr. Nachr.*, XXV. p. 313.

<sup>3</sup> *London Athenæum*, Oct. 3, 1846, p. 1019.

<sup>4</sup> See also Challis's letter of Oct. 15, in *London Athenæum* of Oct. 17, 1846.

<sup>5</sup> *Recherches*, p. 3, note.

<sup>6</sup> *Comptes Rendus*, XXVII. pp. 209, 273, et al.

<sup>7</sup> *Ibid.*, XXIII. p. 662.

count<sup>1</sup> of his search since the end of July; and Mr. Adams, a brief notice<sup>2</sup> of his computations. These papers are printed together in the Monthly Notices<sup>3</sup> of that Society.

During the month of November, 1846, Le Verrier published a complete account of all his investigations, giving in detail the processes by which he had arrived at the results previously made known in the *Comptes Rendus*. This memoir may justly claim a place among the most remarkable mathematical works of the age. It is entitled *Recherches sur les Mouvements de la Planète Herschel, dite Uranus*, and is published as an *Addition* to the *Connaissance des Temps* for 1849, occupying 256 pages. Mr. Adams gave in December a similar abstract of his computations, as a supplement of 31 pages to the Nautical Almanac for 1851.

It is not easy for those who are not versed in the study of Physical Astronomy, to form any adequate idea of the difficulty of the problem which Messrs. Le Verrier and Adams proposed to themselves. The difficulties in the development of the proper methods were exceedingly great, as any one might infer from the manner in which even Airy was accustomed<sup>4</sup> to speak of the problem. An investigation must indeed be eminently difficult and complicated, which that distinguished mathematician would regard as unfeasible. Not only the orbit and mass of the suspected planet, but the elements of Uranus also, were to be regarded as unknown quantities. The limits of error of the ancient observations were also undetermined, but must yet exercise an important influence on the result.

Le Verrier's memoir consists of three parts. The first of these<sup>5</sup> contains a complete investigation of the theory of Uranus, and corresponds to the first paper of the series in the *Comptes Rendus*. It is an investigation of the highest importance, apart from its relation to the problem of a disturbing planet, and is conducted with extraordinary ability. The theory of Uranus may, as Airy has said,<sup>6</sup> be considered as placed now for the first

<sup>1</sup> *Ast. Soc. Notices*, VII. p. 145.

<sup>2</sup> *Ibid.*, p. 149.

<sup>3</sup> Vol. VII. No. 9.

<sup>4</sup> Airy's "Account," *passim*.

<sup>5</sup> *Recherches*, § 2, p. 6.

<sup>6</sup> *Account*, p. 131.

time upon a satisfactory foundation. The methods used, though essentially those of Laplace, Lagrange, and Poisson, have been modified in many respects, and it may with safety be asserted, that almost all of the modifications are improvements.

After a brief consideration of the general formulas of perturbations, Le Verrier proceeds<sup>1</sup> to what he calls the simultaneous determination of all the inequalities. The method adopted is such, that the inequalities are all obtained at the same time, and are so mutually dependent, that an error in any one part of the calculation vitiates the entire result. If, then, after the numerical computation is completed, any one part of the result is found to be exact, it may fairly be concluded that the remainder is exact also. By this method of "simultaneous determination," Le Verrier determined<sup>2</sup> all the perturbations by Saturn, carrying them out to the very smallest sensible terms;—in many cases even to terms whose coefficients amounted only to 0".01. The planetary elements<sup>3</sup> on which his researches were based, were taken from Bouvard's tables.

The whole computation of the perturbations by Saturn having been completed, Le Verrier proceeded to compute<sup>4</sup> a part of the same quantities by the method of Lagrange, in order to test the accuracy of his previous results. The actual steps of this second and most laborious calculation are not given, but the agreement<sup>5</sup> between the amount of the inequalities as obtained by the two different processes is wonderfully close, and reflects no small honor on the author's powers of numerical computation, and on the precision with which all the small terms were taken into account. The agreement of the parts thus doubly computed is a test of the accuracy of the whole work.

The perturbations by Jupiter, proportional to the first power of the mass, are determined<sup>6</sup> according to the ordinary methods; and, although not doubly computed, as in the case of Saturn, are carried out with great rigor.

<sup>1</sup> *Recherches*, § 5, p. 9.

<sup>2</sup> *Ibid.*, § 15, p. 16.

<sup>3</sup> *Ibid.*, pp. 16, 52.

<sup>4</sup> *Ibid.*, § 23, p. 32.

<sup>5</sup> *Ibid.*, §§ 26–28, pp. 37–41, etc.

<sup>6</sup> *Ibid.*, § 34, p. 51.

In the investigation<sup>1</sup> of the changes proportional to the square of the disturbing force, Le Verrier has neatly availed himself of the method used<sup>2</sup> by Laplace. The parts of these, which are not strictly secular, can without hesitation be rejected, and he has calculated<sup>3</sup> the values of the coefficients for 1800 and for 2300, and determined their values for intermediate years by simple interpolation. The discussion is completed by the consideration<sup>4</sup> of the influence of the changes of Saturn's elements, produced by the attraction of Jupiter. This is investigated with much care and elegance. The action of Saturn, as reflected through Jupiter, is not of sufficient importance to require computation.

The theory of Uranus was thus made complete, and needs now only the addition of the perturbations by Neptune, to accommodate it perfectly to the present demands of science.

In the second part<sup>5</sup> of Mr. Le Verrier's "*Recherches*," the theory of Uranus, thus remodelled, is compared with the whole series of known observations, in order to discover how large the discrepancies between the observed and computed course of the planet actually were. In entering upon this division of his subject, the author shows<sup>6</sup> that the sum of the errors in the perturbations as given by Bouvard — considering those perturbations only whose value had completely changed during the period through which our observations extend, and making full allowance for the erroneous masses of Jupiter and Saturn, which Bouvard used — amounted to twenty-nine sexagesimal seconds.

Bouvard's tables appear, indeed, throughout this part of the investigation, in a singularly unfavorable light. Le Verrier has repeated<sup>7</sup> the whole of Bouvard's computations, and subjected his tables to a most searching scrutiny. The result must have surprised astronomers.

<sup>1</sup> *Recherches*, §§ 48 – 58.

<sup>2</sup> *Mécan. Cél.*, Bowditch's Trans., III. p. 283.

<sup>3</sup> *Recherches*, p. 61.

<sup>4</sup> *Ibid.*, § 50, p. 65.

<sup>5</sup> *Ibid.*, p. 89.

<sup>6</sup> *Ibid.*, § 61, p. 90.

<sup>7</sup> *Ibid.*, pp. 91 – 99.



The first inaccuracy alluded<sup>1</sup> to by Le Verrier is in the eccentricity of the orbit. In order to discover the value of the eccentricity which was used in constructing the tables, we can refer directly to Mr. Bouvard's Introduction,<sup>2</sup> or can deduce it from the table<sup>3</sup> of the equation of the center. These, however, do not agree with one another, and in order to decide which of them is erroneous, we must try, if possible, some third method of finding the eccentricity assumed. We are enabled to do this by the algebraical expression of the equation of the center, which is also given<sup>4</sup> in the preface. But, strange to say, we obtain in this way still a third value differing from both the others. The example, which Le Verrier gives<sup>5</sup> to illustrate this, is the value of the equation of the center for a mean anomaly of  $90^\circ$ .

The first value deduced from the preface is . . .	<sup>gr.</sup> 5 93 57.52
The second " " " " " . . .	5 92 71.04
The value given by Table X. is . . . . .	5 93 48.00

Mr. Le Verrier assures us, farther, that any attempt to decide definitely, by means of the radius-vector, what was the true eccentricity on which the tables were based, would lead only to the discovery of new discordances.

In the secular motion of the mean longitude, Tables I. and II. do not agree. The error in Table II. would, according to Le Verrier, give an error of  $21''.5$  in the computed place for Flamsteed's observation of 1690. In the formation of equations of condition, Bouvard appears<sup>6</sup> to have been equally unfortunate, both as regards the analytical and the numerical parts of the work. Finally, a series of typographical errors is given,<sup>7</sup> sixteen of them being in the single table of the equation of the center, and the majority of them errors of grave importance.

Le Verrier was thus compelled to repeat the whole work, and the catalogue of errors which he has given is a sufficient indica-

<sup>1</sup> *Recherches*, p. 92.

<sup>2</sup> *Tables*, p. ii.

<sup>3</sup> Table X. p. 95.

<sup>4</sup> *Tables*, p. xv.

<sup>5</sup> *Recherches*, p. 93.

<sup>6</sup> *Ibid.*, pp. 93-95.

<sup>7</sup> *Ibid.*, pp. 96, 97.

tion of his thoroughness and high standard of precision. And when we remember, that neither Airy<sup>1</sup> had detected these errors, nor Bessel published any thing concerning them, excepting the notice of the one term already referred to, we cannot but still more admire the searching rigor of Le Verrier's investigations.

The author has next given an ephemeris of the heliocentric<sup>2</sup> and geocentric<sup>3</sup> places of Uranus for several successive days, at the epochs of the ancient observations, and of those modern ones which he had chosen as most appropriate for the comparison. For these latter he did not construct normal places, but selected a series of two hundred and sixty-two of the best observations, some taken in opposition and some in quadrature, and suitably distributed over the interval between 1781 and 1845. These, together with the ancient observations,<sup>4</sup> were then compared<sup>5</sup> with the theory. The comparisons were made in right-ascension and declination, and the differences subsequently converted into differences of longitude and latitude. Each of these differences furnishes one equation, but, on account of the smallness of the errors in latitude,<sup>6</sup> the computation was founded upon the longitudes alone; and the mean equations of condition<sup>7</sup> used were those depending upon the longitudes after grouping together all the observations made at the same period.

By means of these Le Verrier was enabled to solve the important problem,<sup>8</sup> — “*Is it possible to satisfy the whole of the preceding equations by a proper determination of the values of the unknown quantities which they contain?*” He found, that the elements, furnished even by those equations of condition derived solely from the modern observations, were entirely incapable<sup>9</sup> of representing the course of the planet since its discovery; — the discordance in the mean positions deduced from ten observations in the years 1781 and 1782 amounting<sup>10</sup> to 20.5 sexagesimi-

<sup>1</sup> “*Account*,” p. 126.

<sup>2</sup> *Recherches*, § 71, pp. 100 – 110.

<sup>3</sup> *Ibid.*, § 73, pp. 112 – 124.

<sup>4</sup> Specially reduced, pp. 124 – 126, § 74.

<sup>5</sup> *Recherches*, § 77, pp. 129 – 136.

<sup>6</sup> *Recherches*, p. 137.

<sup>7</sup> *Ibid.*, pp. 138 – 141.

<sup>8</sup> *Ibid.*, p. 142.

<sup>9</sup> *Ibid.*, § 79, p. 144.

<sup>10</sup> *Ibid.*, p. 143.

mal seconds. It is impossible to believe that such an error actually exists, and we thus see that the theory is inadequate.

The same result is obtained by considering the data from two other entirely different points of view. In the *first* place, the consideration<sup>1</sup> of the equations of condition formed from the observations at eight equidistant epochs, comprehending an interval of ninety-eight years, and of the relations between their second differences, shows<sup>2</sup> that the resulting values of the variation of the mean motion are totally incompatible with one another, and that some change in the then existing theory was inevitably necessary. *Secondly*, from the relation between the correction of the elements, the mean anomaly at the epoch, and the error of the heliocentric longitude, Mr. Le Verrier forms<sup>3</sup> eighteen equations of condition; and, by an extremely elegant process,<sup>4</sup> deduces the amount of discordance between theory and observation, which could, under the worst possible combination of unfavorable circumstances, result from the errors to which the observations are subject. He allows<sup>5</sup> to each of the normal places from modern observations a possible error of four seconds, and to the position deduced from Flamsteed's three observations in 1715, a possible error of fifteen seconds. But even by assuming all the errors at their maximum, and all acting to diminish the discrepancy, Le Verrier shows that only 92" out of 356" can be accounted for, leaving a discordance of 264 sexagesimal seconds still unexplained.<sup>6</sup>

In the third part<sup>7</sup> of the work, which corresponds to the second memoir<sup>8</sup> in the *Comptes Rendus*, the author proceeds to the discussion of the great problem of the cause of the anomalies which he had proved to exist. In introducing this part of the *Recherches* by some remarks on the difficulty of the problem, Le Verrier remarks,<sup>9</sup> that the unforeseen obstacles which he

<sup>1</sup> *Recherches*, § 80.

<sup>2</sup> *Ibid.*, p. 146.

<sup>3</sup> *Ibid.*, p. 147, § 81.

<sup>4</sup> *Ibid.*, §§ 82, 83, pp. 147 - 149.

<sup>5</sup> *Ibid.*, pp. 149, 150.

<sup>6</sup> *Recherches*, p. 150.

<sup>7</sup> *Ibid.*, p. 151.

<sup>8</sup> *C. R.*, XXII. p. 907.

<sup>9</sup> *Recherches*, p. 151.

encountered would more than once have deterred him from the farther prosecution of his labor, had he not been fully impressed with a conviction of its importance. The readers of the work cannot but be struck with this remark, for the impediments encountered and the apparent contradictions of the results would indeed have dismayed any less gifted mathematician. Entering upon the immediate subject, the author considers<sup>1</sup> in a cursory manner various hypotheses which have been suggested to account for the apparently anomalous motion of Uranus, and assigns his reasons for rejecting them. Astronomers must also be impressed with the argument<sup>2</sup> which his computations had furnished him, against the hypothesis, that a comet might have produced the disturbances in question. It is, that both the series of ancient observations and that of the observations since 1820 are alike incapable of according with elements deduced from the motion of the planet for the forty years immediately following its discovery; in other words, that between 1690 and 1845 *two* perturbations had occurred. As regards the hypothesis of an unknown planet, too, he inferred that it could not be within the orbit of Uranus, as its effect on Saturn must, in that case, have been more perceptible. "It is easy to conclude," said<sup>3</sup> he, "that its perturbative action would only be exerted at the particular time when it passed in the neighborhood of Uranus, and the small difference which there would be between the periods of revolution of the two bodies would prevent this circumstance from having taken place more than once in the period which the observations of the planet include. This consequence is contradictory to our deduction from the observations. The period comprised between 1781 and 1820 *shows no trace whatever* of large perturbations; and, on the other hand, can neither be connected with the previous nor with the subsequent observations."

This last was unquestionably written after the researches were completed. The planet of Le Verrier's theory must have acted

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<sup>1</sup> *Recherches*, § 86.

<sup>2</sup> *Ibid.*, p. 152.

<sup>3</sup> *Ibid.*, p. 153; *C. R.*, XXII. p. 914.



on Uranus twice during the interval comprised between the observations of 1690 and 1845. But it is now known that only one maximum of perturbation by Neptune occurred within that period, namely, in 1822. With regard to the fact that the observations between 1781 and 1820 were capable of being perfectly represented by elliptic elements, the question naturally arises, whether osculating elements might not be found, capable of representing the motions of Uranus for any period of forty years, within moderate limits of error.

In case the perturbing body be exterior to Uranus, Mr. Le Verrier showed<sup>1</sup> that it could not be at any very remote distance, such, for example, as three times the mean distance of Uranus; for, in that case, the mass to be attributed must be so large as to affect Saturn very much more than the theory of that planet allows us to assume. It is most natural, then, to commence the first rough approximation, by assuming the new distance to be about double that of Uranus, — and the more so, as this distance would correspond to the curious empirical formula<sup>2</sup> of Wurm, Titius, and Bode, which represented, though in a very rough way, the distances of the planets from the sun. Le Verrier is entitled to praise for holding himself independent of this “law,” which, as Gauss long since showed,<sup>3</sup> is not only totally devoid of that precision which characterizes nature’s laws, but fails entirely when legitimately applied to Mercury. As it is, perhaps the influence of this ill-omened formula may have been instrumental in depriving both Le Verrier and Adams of the satisfaction of arriving, by theoretical means, at a knowledge of the elements and mass of Neptune, and thus making a physical as well as a mathematical discovery.

The problem to be considered was fortunately simplified, in some degree, by the fact, that the perturbations in latitude were very small. Uranus moves in a plane but slightly inclined to the ecliptic, and the same must, therefore, in all probability, be

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<sup>1</sup> *Recherches*, p. 153.

<sup>2</sup> Bode, *Berl. Astr. Jahrb.*, 1791, p. 188. Lalande, *Bibliographie*, p. 845.

<sup>3</sup> *Monatliche Correspondenz*, VI. p. 504.

true of the disturbing body. Le Verrier, then, assuming that the unknown planet moved in the ecliptic, proceeded to investigate the following questions<sup>1</sup>: —

*“Is it possible that the irregularities of Uranus are due to the action of a disturbing planet, situated in the ecliptic at a mean distance double that of Uranus? And if so, where is this planet situated? What is its mass? What are the elements of the orbit which it describes?”*

There is, as he says, but one route to follow in the discussion of this question. “It is necessary<sup>2</sup> to form expressions for the perturbations, due to the new body, in functions of its mass and of the unknown elements of the ellipse which it describes; we must introduce these perturbations into the coördinates of Uranus, calculated by means of the unknown elements of the ellipse which this planet describes. Putting the coördinates, thus obtained, equal to the coördinates observed, we must take as unknown quantities in the resultant equations of condition, not only the elements of the ellipse described by Uranus, but also the elements of the ellipse described by the disturbing planet whose position we seek.”

Taking, then, not the planet's coördinates, but the elements of its orbit, as the unknown quantities, Le Verrier follows<sup>3</sup> the course thus indicated, using the common formulas, and omitting all consideration of the terms proportional to the time which can be confounded with the mean motion, and of the constants which can be combined with the epoch. The first attempt at a solution was based upon eight equations<sup>4</sup> similar to those<sup>5</sup> to which we have before alluded. These were founded on observations<sup>6</sup> subsequent to 1747, all of Flamsteed's observations being neglected. The second differences were combined precisely as before. The six equations<sup>7</sup> thus obtained were reduced, by a dexterous elimination<sup>8</sup> of the epoch, eccentricity, and mean motion, to three

<sup>1</sup> *C. R.*, XXII. p. 915; *Recherches*, p. 154.

<sup>2</sup> *Recherches*, p. 154, § 88.

<sup>3</sup> *Ibid.*, §§ 90–95.

<sup>4</sup> *Ibid.*, p. 162, § 96.

<sup>5</sup> *Ibid.*, § 80.

<sup>6</sup> *Ibid.*, p. 146.

<sup>7</sup> *Ibid.*, p. 162.

<sup>8</sup> *Ibid.*, p. 164.

others,<sup>1</sup> whose constant terms were the same as those in the equations above alluded to. From these he obtained<sup>2</sup>  $\frac{1}{4740}$  ( $m = 2.11$ ) as a first approximation to the mass. But from the numerous obstacles to accuracy which are manifest in the course of this solution, Le Verrier inferred<sup>3</sup> that the interval of ninety-eight years was not sufficient for his purpose, but that it was necessary to extend the interval as far as observations would permit, and to form the equations of condition with all possible rigor.

This is done in the second solution,<sup>4</sup> — a solution which is pre-eminently a discussion of limits, and a brilliant combination of ingenuity, of analytical skill, and of laborious calculation, — a solution which cannot be adequately described without departing from the popular form prescribed to me in this Report. Account is taken of all the allowable errors in the ancient observations, and “the field of inquiry narrowed with consummate skill.”<sup>5</sup> The epoch being assumed at the beginning of the present century, all the values of its mean longitude which, when substituted in the equations of condition, would give a negative mass, as well as those which would give a mass large enough to affect Saturn sensibly, were promptly rejected.<sup>6</sup> The expression obtained for the mass is in the form of a fraction;<sup>7</sup> the numerator and denominator of which were separately examined, the former by an elegant application of Sturm’s formula. The result<sup>8</sup> was, that the longitude of the epoch must be included between the limits  $97^\circ$  and  $190^\circ$ , or between  $263^\circ$  and  $359^\circ$ , in order to render the corresponding mass positive; and if those masses be rejected which the motion of Saturn forbids us to assume, Le Verrier found<sup>9</sup> that the longitude of the epoch must be comprised between  $108^\circ$  and  $162^\circ$ , or between  $297^\circ$  and  $333^\circ$ . But, after laborious calculation, he also found that neither of

<sup>1</sup> *Recherches*, p. 164.

<sup>2</sup> *Ibid.*, § 100.

<sup>3</sup> *Ibid.*, p. 165.

<sup>4</sup> *Ibid.*, §§ 101–124.

<sup>5</sup> Peirce, *Proc. Amer. Acad.*, I. p. 66.

<sup>6</sup> *Recherches*, p. 169.

<sup>7</sup> *Ibid.*, §§ 104–107.

<sup>8</sup> *Ibid.*, p. 174.

<sup>9</sup> *Ibid.*, § 112, p. 181.

these limitations will allow the places in 1690 and 1747 to be represented with tolerable accuracy, "so that the consequence which would seem to result from the discussion, thus conducted, would be, that it was impossible to represent the course of Uranus by means of the perturbative action of the new planet."

Le Verrier was fortunately undismayed by this result, although many a good mathematician and experienced computer would, for less reason, have abandoned his apparently unprofitable labor in despair. But even Le Verrier, according to Biot,<sup>1</sup> revolved the matter in his mind for three months without advancing a step. He discovered subsequently, as he next states,<sup>2</sup> that, by neglecting two little inequalities of the longitude, in themselves so small that one would suppose himself warrantable in omitting them with perfect impunity, all the details of the solution became different, and the errors in 1690 were completely changed. He found,<sup>3</sup> still farther, that in spite of the apparent limitations furnished above by the resulting negative value of the mass, the assumption of values outside these limits, for the longitude of the epoch, enabled him perfectly to represent the motions of Uranus. He arrived, in short, at the fundamental proposition,<sup>4</sup> "*that there was in the ecliptic but one single region in which the perturbing planet could be placed, so as to account for the motions of Uranus ; that the mean longitude of this planet on the 1st of January, 1800, must have been between 243° and 252°.*" Continuing his numerical computations, and calculating,<sup>5</sup> for each one of the eighteen normal places, the values of the perturbations corresponding to different hypotheses as to the longitude of the epoch, Le Verrier found<sup>6</sup> "that all the observations of Uranus could be represented by means of the perturbative action of a planet whose mean longitude was 252° on the 1st of January, 1800, and whose eccentricity and perihelion longitude are determined by the formulas" already given. The corresponding longitude in 1847.0 would be 325°, and, as above stated, Le Ver-

<sup>1</sup> *Journal des Savans*, Jan., 1847.

<sup>2</sup> *Recherches*, p. 181.

<sup>3</sup> *Ibid.*, p. 182.

<sup>4</sup> *Ibid.*, § 114, p. 185

<sup>5</sup> *Ibid.*, § 116, pp. 187, 188.

<sup>6</sup> *Ibid.*, p. 193.



rier closed his memoir of June, 1846, by expressing his hope that astronomers might detect the planet.

The fourth book<sup>1</sup> contains a more precise determination of the elements of the orbit, and, though the processes are given more in detail, is essentially the same as the memoir of August 31, in the *Comptes Rendus*.<sup>2</sup> In this book the limits of the real and the apparent place of the disturbing body are computed with still greater precision. The hypotheses as to the mean distance vary<sup>3</sup> from 36.2 to 39.2; those as to the longitude of the epoch, from  $234^{\circ}$  to  $270^{\circ}$ . For these different hypotheses, six in number, the coefficients of the equations of condition are minutely computed.<sup>4</sup> Thirty-three equations of condition are formed<sup>5</sup> by comparison with observed geocentric places. From these equations six unknown quantities, and among them the elements of the orbit of Uranus, are eliminated, and their places supplied by the unknown quantities representing the corrections of the mean distance, and of the longitude of the epoch. The resulting<sup>6</sup> thirty-three are solved<sup>7</sup> by the method of least squares, and give,<sup>8</sup> as the final solution, the orbit on page 21 of this Report.

Le Verrier, in thus assigning definitely the planet's position in the heavens, expressed<sup>9</sup> the belief, that its disc would be large enough to indicate its planetary character to the attentive observer. Uranus, at the distance 19, has an apparent diameter of  $4''$ . Assuming a position and mass of the new planet conformable to his computations, and its density the same as that of Uranus, Le Verrier inferred, that its apparent diameter in opposition must be about  $3''.3$ , and its specific brilliancy about one third that of Uranus.

The orbit and mass thus obtained represent all the ancient observations within eight seconds, except Flamsteed's in 1690, to

<sup>1</sup> *Recherches*, p. 196.

<sup>2</sup> *C. R.*, XXIII. p. 428.

<sup>3</sup> *Recherches*, p. 198.

<sup>4</sup> *Ibid.*, § 130, pp. 203-220.

<sup>5</sup> *Ibid.*, § 131, p. 222.

<sup>6</sup> *Ibid.*, § 135, p. 231.

<sup>7</sup> *Ibid.*, § 136, p. 233.

<sup>8</sup> *Ibid.*, §§ 137-141, pp. 234-236.

<sup>9</sup> *Ibid.*, p. 237; *C. R.*, XXIII. p. 430.

which Le Verrier allows<sup>1</sup> but little weight, being content with representing it within 20".\*

The author next determines<sup>2</sup> the extreme limits within which the disturbing planet is necessarily situated, allowing<sup>3</sup> a large error to all the observations, — five seconds, for instance, to the modern ones, and twenty-five to Flamsteed's in 1690. The result is,<sup>4</sup> that *the mean distance must be between the limits 35.04 and 37.9*. Assuming the corresponding times of revolution as the extreme limits of the period, he finds,<sup>5</sup> by an ingenious geometrical process, the limits of the planet's place, which they give for 1847.

The fifth and last part<sup>6</sup> of the *Researches* corresponds to the memoir presented to the Academy of Sciences on the 5th of October, after the discovery of the planet, but was evidently written before the welcome news had reached Mr. Le Verrier. In this he endeavors to deduce from the perturbations of the latitude the position of the place in which the new planet must move. He infers<sup>7</sup> that the observations in latitude concur with those in longitude in indicating the existence of a disturbing planet, that<sup>8</sup> the plane of this planet's orbit must be inclined about  $4^{\circ} 38'$  to that of the orbit of Uranus, and that<sup>9</sup> a single observation of the orbit of this new body would suffice to make known approximately the plane in which it moves. These deductions as to the plane of the orbit Le Verrier submitted with much diffidence, on account of the smallness of the perturbations in latitude from which they must be made.

Mr. Adams's investigation is of a totally different nature, though characterized by remarkable ability and mathematical

\* In order to do justice to this observation, it must be stated that Neptune, with the mass  $\frac{1}{15840}$ , used by Prof. Peirce, satisfies<sup>10</sup> this observation within a single second.

<sup>1</sup> *Recherches*, p. 238.

<sup>2</sup> *Ibid.*, §§ 143–148.

<sup>3</sup> *Ibid.*, p. 240, § 143.

<sup>4</sup> *Ibid.*, p. 240, § 144.

<sup>5</sup> *Ibid.*, pp. 242, 247.

<sup>6</sup> *Ibid.*, p. 250.

<sup>7</sup> *Ibid.*, p. 251.

<sup>8</sup> *Ibid.*, p. 251.

<sup>9</sup> *Ibid.*, p. 252.

<sup>10</sup> *Proc. Amer. Acad.*, I. 333.

power. In it the question of limits is neither discussed, nor in the least degree involved, but the problem is directly proposed, — “To find the most probable orbit and mass of the disturbing body which has acted on Uranus.” The question, whether it be necessary to assume the existence of such a body, is not discussed. The labors of Bouvard were supposed to have set this question at rest. Mr. Adams states,<sup>1</sup> that his first solution was attempted in 1843, assuming the orbit to be a circle, the distance, nearly \* in conformity with “Bode’s law,”<sup>2</sup> twice that of Uranus, and taking solely the modern observations into account. The errors of the tables were taken<sup>3</sup> from Bouvard’s equations of condition as far as the year 1821, and, for later dates, from Schumacher’s *Astronomische Nachrichten* and the Reductions of the Greenwich Planetary Observations.

Mr. Adams inferred<sup>4</sup> from his results that a good general agreement between theory and observation might be obtained, and therefore commenced<sup>5</sup> a more accurate investigation, the results of which were communicated to the Astronomer Royal in the letter<sup>6</sup> of October, 1845, above<sup>7</sup> referred to, and to Prof. Challis, some time<sup>8</sup> in the month of September.

Flamsteed’s observation of 1690 was entirely rejected.<sup>9</sup> The chief inequalities given by Bouvard were recomputed,<sup>10</sup> without the detection of any error, excepting the one pointed out by Bessel. Airy’s mass of Jupiter<sup>11</sup> was introduced in the place of the one used by Bouvard. Those inequalities depending on the square of the disturbing force, which had been pointed out by Hansen, were also recomputed.<sup>12</sup> The differences between the calculated and observed heliocentric longitudes were converted<sup>13</sup>

$$* x = 4 + 2^{n-2} \cdot 3.$$

<sup>1</sup> “*Explanation*,” § 3, p. 4.

<sup>2</sup> *Berl. Astr. Jahrb.*, 1791, p. 189.

<sup>3</sup> *Expl.*, § 4.

<sup>4</sup> *Ibid.*, § 3.

<sup>5</sup> *Ibid.*, § 10 *et seq.*

<sup>6</sup> “*Account*,” p. 129.

<sup>7</sup> *Expl.*, p. 15.

<sup>8</sup> “*Account*,” p. 128, letter No. 6.

<sup>9</sup> *Ibid.*, p. 130. “*Explanation*,” § 25.

<sup>10</sup> “*Explanation*,” p. 5, § 7.

<sup>11</sup> *Mem. R. Astr. Soc.*, X. p. 47.

<sup>12</sup> *Expl.*, p. 6.

<sup>13</sup> *Ibid.*, p. 7, § 9.

into differences of *mean* longitude. Observations made near opposition were selected,<sup>1</sup> when possible, and the series of modern observations divided into groups, from which were deduced<sup>2</sup> normal places at intervals of three years. The equations of condition formed from these served as the basis of the entire computation.

Assuming, then, as a first hypothesis,<sup>3</sup> the mean distance of the new body to be twice that of Uranus, Adams computes<sup>4</sup> the value of the fundamental perturbations. The values obtained<sup>5</sup> are almost identical with those<sup>6</sup> of Le Verrier. But with regard to the influence of the third and fourth terms, Adams does not appear to have experienced the inconveniences by which Le Verrier states that he was so much impeded.<sup>7</sup> Both mathematicians agree,<sup>8</sup> however, in the rejection of the small perturbations of the second order, dependent on the square of the eccentricity of the disturbing planet. Taking the mean opposition in 1810, as the epoch, Mr. Adams has elegantly arranged<sup>9</sup> his equations of condition in such a way, that they separate themselves into two groups, each having<sup>10</sup> but five unknown quantities. The coefficients are then readily computed by summation. By eliminating<sup>11</sup> the unknown quantities in each group, it will, therefore, be comparatively easy, for any assumed value of the mass and longitude at the epoch, to obtain a correction by the method of least squares. But as the unknown quantities are the same in all the equations of each group, these equations may be added,<sup>12</sup> and the substitution of the elements of Uranus, already approximately known, gives values for the mass and epoch. The equations of condition drawn from the ancient observations, omitting that in 1690, are the suited<sup>13</sup> for the determination of

<sup>1</sup> *Explanation*, p. 5, § 6.

<sup>2</sup> *Ibid.*, pp. 6, 7.

<sup>3</sup> *Ibid.*, p. 8, § 12.

<sup>4</sup> *Ibid.*, § 12.

<sup>5</sup> *Ibid.*, p. 9.

<sup>6</sup> *Recherches*, p. 161, § 95.

<sup>7</sup> *Ibid.*, p. 181.

<sup>8</sup> Adams, *Expl.* §§ 11, 12; Le Verrier, *Recherches*, pp. 158, 201.

<sup>9</sup> *Expl.*, § 13.

<sup>10</sup> *Ibid.*, § 14.

<sup>11</sup> *Ibid.*, §§ 15–20.

<sup>12</sup> *Ibid.*, § 21, p. 13.

<sup>13</sup> *Ibid.*, § 23.



the eccentricity and perihelion longitude. This determination is very skilfully made, and the elements<sup>1</sup> furnished by the whole analysis are those communicated<sup>2</sup> to the Astronomer Royal in October, 1845.

Mr. Adams next proceeded<sup>3</sup> to repeat the whole investigation, assuming the mean distance to be 37.25, the first hypothesis having been 38.36. For the time when the computations on this hypothesis were made, no date is assigned. They were probably made during the summer of 1846, as the resulting elements were communicated<sup>4</sup> to Mr. Airy on the 2d of September of that year. They are as follows<sup>5</sup> : —

Epoch,	. . . . .	1810.328
Mean longitude,	. . . . .	264° 50'
Longitude of perihelion,	. . . . .	298° 41'
Eccentricity,	. . . . .	0.120615
Mean longitude, Oct. 6, 1846,	. . . . .	323° 2'
Mass,	. . . . .	$\frac{1}{6665}$

The theoretical corrections of the mean longitude on each hypothesis are next<sup>6</sup> given, — the parts due to the changes in the elements of Uranus and to the action of the hypothetical planet being written separately, — and these corrections are compared with observation.

The modern observations of Uranus are thus admirably represented<sup>7</sup> down to the year 1840. The ancient ones were represented within tolerable limits, excepting the observation in 1771, where the discrepancy amounts to 11".8 on the first, and 12".8 on the second hypothesis. Le Verrier has not given any comparison of this observation with his final orbit.<sup>8</sup>

In the observations since 1840, however, Mr. Adams did not find so satisfactory an agreement. The differences, as deduced from the three oppositions immediately preceding the investigation, were<sup>9</sup> by the two hypotheses, —

<sup>1</sup> *Expl.*, § 31.

<sup>2</sup> *R. Ast. Soc. Notices*, pp. 129, 151.

<sup>3</sup> *Expl.*, § 32, *et seq.*

<sup>4</sup> *R. Ast. Soc. Not.*, VII. pp. 138, 151.

<sup>5</sup> *Expl.*, §§ 47, 48, p. 25.

<sup>6</sup> *Ibid.*, §§ 49–51.

<sup>7</sup> *Ibid.*, § 52.

<sup>8</sup> *Recherches*, § 142, p. 238.

<sup>9</sup> *Expl.*, § 53, p. 28.

	I.	II.
1843,	7".11	5".77
1844,	8.79	7.05
1845,	12.40	10.18

The errors for these three years are, thus, about one fifth less in the second hypothesis. The first had assumed that the ratio of the mean distances of the two planets was equal to  $\sin. 30^\circ$ , the second that it was  $\sin. 31^\circ$ . Mr. Adams roughly inferred<sup>1</sup> from this consideration, that the true ratio would be about equal to  $\sin. 35^\circ$ , which gives a mean distance of 33.42.

The application of the Rule of Three to a problem so complicated as that which the orbit of the disturbing planet presented was, we are bound to believe, intended merely as a rude means of conjecture. It appears, therefore, surprising, that this inference should be dwelt upon as one of the merits of Mr. Adams's investigation, and as tending to show that the solution at which he arrived corresponded to the orbit and place of Neptune. Even had Mr. Adams intended to apply the rule of simple proportion, it is impossible that he should have founded it upon the comparison of three oppositions alone. Le Verrier has shown<sup>2</sup> that the assumption of even 35 as the mean distance would lead to *intolerable* discordances. Peirce has further proved<sup>3</sup> that an important change in the character of the perturbations takes place near the distance 35.3. It is, therefore, evident that no claims can be based upon the rough inference alluded to. And it is but just to assume that Mr. Adams would disclaim any intention to dwell upon this point, although stress has been laid upon it<sup>4</sup> by one of the most eminent of his countrymen.

After giving the formulas for the corrections of the tabular radius-vector of Uranus upon each of his hypotheses, Mr. Adams closes by stating,<sup>5</sup> that, on account of the shortness of the pe-

<sup>1</sup> *Expl.*, p. 29; "*Account*," p. 139.

<sup>2</sup> *Recherches*, p. 240.

<sup>3</sup> *Proc. Amer. Acad.*, I. 66.

<sup>4</sup> Herschel, *Outlines of Astr.*, p. 517, note.

<sup>5</sup> *Expl.*, § 60.

riod (98 years, the observation in 1690 having been rejected), and the smallness of the perturbations in latitude, all his attempts to fix the plane of the orbit had been unsatisfactory.

I have thus endeavored to give an account of the origin and progress of the theory of Uranus up to the discovery of Neptune, and the publication of those computations which had led astronomers to suspect its existence and direction. Of the discussion concerning priority which unfortunately arose, it is not necessary to speak. There cannot be the slightest doubt of the fact, that the several investigators were entirely independent of one another; but as many persons, especially those not professedly devoted to the pursuit of science, attach importance to the question of priority, I have endeavored to state the facts as impartially as possible, and thus to give the data by which any one may be enabled to judge for himself. The discussion which subsequently arose was of such a nature as to throw the controversy between the partisans of the French and of the English geometer entirely into the background.

Before passing from the discovery of Neptune to the subsequent development of the theory, it is proper to allude to the charts published by the Berlin Academy at the instance<sup>1</sup> of Bessel; inasmuch as these furnished the means by which Neptune was discovered at Berlin upon the first search. They have also directly contributed, within the last four years, to the detection of several members of our solar system. The chart<sup>2</sup> embracing the region in which Neptune was found had been drawn up with great accuracy by Dr. Bremiker, the eminent coadjutor of Prof. Eneke, in the Berlin Astronomical Almanac. Although sometime printed, it had been published but a short time. Had Prof. Challis been in possession of this map, he would probably<sup>3</sup> have discovered Neptune on the 4th of August, as he observed it as a fixed star on that day,<sup>4</sup> while sweeping for the planet.

<sup>1</sup> *Abhandl. d. Königl. Akad. Berl.*, 1824, p. iii.; *Astr. Nachr.*, IV. pp. 297, 437.

<sup>2</sup> Hour XXI.

<sup>3</sup> *Astr. Nachr.*, XXV. p. 102; *Ast. Soc. Not.*, VII. p. 146.

<sup>4</sup> *Ast. Soc. Not.*, VII. p. 148.

He observed it again, August 12. He also noted it September 29, as seeming to have a disc; but the news of the discovery at Berlin on the 23d arrived before the next fair evening.<sup>1</sup>

The slowness of the planet's motion of course rendered it impossible to find the true orbit, till after the lapse of considerable time, but elements upon the hypothesis of a circular orbit were computed within the first month by Adams,<sup>2</sup> Galle,<sup>3</sup> and Binet.<sup>4</sup> These agreed very nearly with one another, and coincided especially in showing the distance from the sun to be about 30. Mr. Adams afterwards computed<sup>5</sup> a second circular orbit, which gave the same result.

Mr. Valz, of the Marseilles Observatory, endeavored<sup>6</sup> early in the year 1847 to deduce the form of the orbit from the small arc described by the planet since its discovery. He found himself, however, unable to obtain a reliable value for the eccentricity, but assigned  $\frac{1}{10}$  as the result of his computations. At the same time he requested attention to the fact, that, in a letter to Arago concerning the perturbations of Halley's comet, published<sup>7</sup> Sept. 12, 1835, he had expressed his belief in the existence of a planet exterior to Uranus.

The means of obtaining elliptic elements was afforded by the fortunate discovery of an ancient observation of Neptune. This discovery was made independently by two astronomers, Mr. Walker in Washington, and Dr. Petersen in Altona, each of whom computed an approximate ephemeris for Neptune, and came to the conclusion that no observer except Lalande had catalogued in the vicinity of the planet.

The modes of research employed by Dr. Petersen and Mr.

<sup>1</sup> *Ast. Soc. Notices*, VII. p. 147.

<sup>2</sup> *Astr. Nachr.*, XXV. p. 106.

<sup>3</sup> *Ibid.*, XXV. pp. 192, 311; *Bericht d. Königl. Preuss. Akad.*, 1846, p. 280; *Ast. Soc. Not.*, VII. p. 148; *Astr. Nachr.*, XXV., p. 311.

<sup>4</sup> *Comptes Rendus*, XXIII. p. 798.

<sup>5</sup> *Ast. Soc. Not.*, VII. p. 148.

<sup>6</sup> *Comptes Rendus*, XXIV. p. 638.

<sup>7</sup> *Ibid.*, I. p. 130.



Walker were essentially different. Dr. Petersen<sup>1</sup> compared a chart of the stars of the *Histoire Céleste*, in a zone extending from 2° south to 4° north latitude, and from 14<sup>h</sup> to 17<sup>h</sup> right-ascension, directly with the heavens, and found that three stars observed by Lalande were no longer visible. On a second comparison, he saw that the recorded places of two of these were probably vitiated by a typographical error of one minute of time. The third star had been observed May 10th, 1795, and was entered<sup>2</sup> as follows in the *Histoire Céleste*: —

Mag.	Middle Thread.	Zen. Dist.
7.8	14 <sup>h</sup> 11 <sup>m</sup> . 23 <sup>s</sup> .5	60° 7' 19"

Dr. Petersen then calculated from Galle's circular elements the position of Neptune for the time of this observation, and found an agreement sufficiently close to convince him that the missing star was Neptune.

Walker had, in the United States, arrived at the same result by a totally different investigation.<sup>3</sup> He made no use of the telescope. He had at hand a large collection of observations, from which he had already computed elements of sufficient accuracy to show that Lalande had only swept in the neighborhood of Neptune upon the 8th and 10th of May, 1795. For these nights he computed a *locus* of Neptune, by assuming different eccentricities, upon the two hypotheses of the present true anomaly being greater or less than 180°. The catalogue of Lalande's stars within this locus was then subjected to a rigid scrutiny. The stars which have been since observed, those more than 15' north or south of the planet's computed path, and those below the ninth magnitude, being rejected, there remained but one star in the list, and that was less than a minute north of the computed declination of Neptune for that right-ascension. This discovery was made on the 2d of February, and on the next day Mr. Walker communicated to the astronomers of the

<sup>1</sup> *Astr. Nachr.*, XXV. pp. 291, 303.

<sup>2</sup> *Histoire Céleste*, p. 158, obs. 8.

<sup>3</sup> *Washington Union*, Feb. 9, 1846; *Proc. Am. Phil. Soc.*, IV. p. 318; *Proc. Amer. Academy*, I. p. 57; *Astr. Nachr.*, XXV. p. 383.

Washington Observatory his conviction that the missing star was Neptune. Upon the 4th, the weather first became clear, and Prof. Hubbard of the Observatory had the gratification of finding<sup>1</sup> that the star was no longer in the place where Lalande had seen it.

It was thus rendered a moral certainty, by the independent labors of the American and the German astronomer, that Neptune had been observed by Lalande, and that the eccentricity of the orbit was very small. This enabled Mr. Walker to compute elliptic elements<sup>2</sup> during the month of February, 1847, which represented the motion of the planet with great accuracy; and Mr. Adams also communicated<sup>3</sup> elliptic elements to Prof. Schumacher, under date of May 19th. By means of the ephemerides of Neptune published<sup>4</sup> by these two astronomers, the planet was followed without difficulty through the opposition of 1847.

Mr. Walker and Dr. Petersen had both immediately written to Le Verrier to inform him of their researches, and Le Verrier communicated to the French Academy of Sciences, at the same meeting,<sup>5</sup> the results of the investigations at Washington and at Altona.

But in speaking of Mr. Walker's elements, he remarked<sup>6</sup> that "the small eccentricity which appeared to result from Mr. Walker's computations would be incompatible with the nature of the perturbations of the planet Herschel."

The hypotheses of Walker and Petersen received a striking confirmation from the examination of Lalande's MSS. by Mr. Mauvais, of the Royal Observatory, Paris. Mauvais discovered<sup>7</sup> that Lalande had observed the star on the 8th, as well as on

<sup>1</sup> *Proc. Amer. Acad.*, I. p. 64; *Proc. Am. Phil. Soc.*, IV. p. 318.

<sup>2</sup> *Proc. Amer. Acad.*, I. p. 67; *Proc. Am. Phil. Soc.*, IV. p. 319; *Astr. Nachr.*, XXV. p. 383.

<sup>3</sup> *Astr. Nachr.*, XXV. p. 399. See also *R. Ast. Soc. Not.*, June 11, 1847.

<sup>4</sup> *Astr. Nachr.*, XXV. pp. 51, 241.

<sup>5</sup> March 29, 1847.

<sup>6</sup> *Comptes Rendus*, XXIV. p. 531.

<sup>7</sup> *Comptes Rendus*, XXIV. p. 641; *Astr. Nachr.*, XXVI. p. 97.

the 10th of May. But in consequence of the non-accordance of the two observations, he had only inserted the latter in the *Histoire Céleste*, and annexed to it the (:) which he used to denote a doubtful observation. Both Mauvais and Peirce, after a careful reduction of both observations, found<sup>1</sup> that the discordance corresponded precisely to the motion of Neptune in two days, — so that the question of identity is now beyond dispute.

Meantime,<sup>2</sup> Prof. Peirce had taken a remarkable step. From the distance, 30, and consequent angular motion, without any hypothesis as to the character of the orbit, he arrived at the startling conclusion,<sup>3</sup> “that the planet Neptune is not the planet to which geometrical analysis had directed the telescope; that its orbit is not contained within the limits of space which have been explored by geometers searching for the source of the disturbances of Uranus; and that its discovery by Galle must be regarded as a happy accident.”

This conclusion, paradoxical as it might at first have appeared to many, was announced with a candor and moral courage only equalled by that of Le Verrier in his original prediction of the planet's place. The reasoning<sup>4</sup> by which Peirce defended his position deserves, even at this late day, the most careful consideration. It is so clear and convincing, that it would seem unnecessary to dwell upon it, were it not that even now astronomers<sup>5</sup> of high eminence do not hesitate to dispute the ground there taken; although the arguments adduced against it are rather of a popular than of a mathematical character.

Peirce first alluded<sup>6</sup> to the two fundamental propositions of Le Verrier, viz.: —

1st. That the planet's mean distance must be between 35 and 37.9;

<sup>1</sup> *Comptes Rendus*, XXIV. p. 666; *Proc. Amer. Acad.*, I. p. 149.

<sup>2</sup> March 16, 1847.

<sup>3</sup> *Proc. Amer. Acad.*, I. p. 65.

<sup>4</sup> *Ibid.*, pp. 65–68.

<sup>5</sup> Herschel, *Outlines of Astronomy*, § 776, p. 516.

<sup>6</sup> *Proc. Amer. Acad.*, I. p. 66.

2d. That the mean longitude for January 1st, 1800, must have been within the limits  $243^{\circ}$  and  $252^{\circ}$ ; and announced that he had convinced himself that, although neither of these was inconsistent with the observations made upon Neptune since its discovery, yet that the two combined were utterly irreconcilable with observation; — that if the first proposition were assumed as true, the mean longitude in 1800 must have been at least forty degrees from the limits of the second; — and that, if we adopted the second proposition, the time of revolution must be less by forty years than the inferior limit given by the first.

“It is not, however,” continued Peirce, “a necessary conclusion that Neptune will not account for the perturbations of Uranus; for its probable mean distance of about 30 is so much less than the limits of the previous researches, that no inference from them can be extended to it. An important change, indeed, in the character of the perturbations, takes place near the distance 35.3; so that the continuous law by which such inferences are justified is abruptly broken at this point, and it was hence an oversight in Mr. Le Verrier to extend his inner limit to the distance 35. A planet at the distance 35.3 would revolve about the sun in 210 years, which is exactly two and a half times the period of revolution of Uranus. Now if the times of revolution of two planets were exactly as 2 to 5, the effects of their mutual influence would be peculiar and complicated, and even a near approach to this ratio gives rise to those remarkable irregularities of motion which are exhibited in Jupiter and Saturn, and which greatly perplexed geometers until they were traced to their origin by Laplace. This distance of 35.3, then, is a complete barrier to any logical deduction, and the investigations with regard to the outer space cannot be extended to the interior.

“The observed distance, 30, which is probably not very far from the mean distance, belongs to a region which is even more interesting in reference to Uranus than that of 35.3. The time of revolution which corresponds to the mean distance 30.4 is not 168 years, being exactly double the year of Uranus, and the



influence of a mass revolving in this time would give rise to very singular and marked irregularities in the motions of this planet. The effect of a near approach to this ratio in the mean motion is partially developed<sup>1</sup> by Laplace in his theory of the motions of the three inner satellites of Jupiter. The whole perturbation arising from this source may be divided into two portions or inequalities, one of which, having the same period with the time of revolution of the inner planet, is masked to a great extent behind the ordinary elliptic motions, while the other has a very long period, and is exhibited for a great length of time under the form of a uniform increase or diminution of the mean motion of the disturbed planet."

Peirce closed this most important paper by showing,<sup>2</sup> that, if the period of Neptune were more than  $166\frac{1}{2}$ , and less than  $169\frac{1}{2}$  years, the conclusion was inevitable, that its period was precisely twice as long as that of Uranus.

Walker has since found<sup>3</sup> the period of Neptune to be 164.6 years. The two periods are therefore not precisely commensurable, but they approach commensurability so nearly that some of the terms<sup>4</sup> of the perturbations of Uranus require careful investigation, which, according to the theories of Le Verrier and Adams, would be merged in the longitude of the epoch and other elements of the elliptic motion.

In reference to the apparent inconsistency of the assertion, that the discovery was "a happy accident," Peirce still farther showed<sup>5</sup> that the problem was susceptible of several solutions, decidedly different from one another, and from those of Le Verrier and Adams, and equally complete with theirs. "The present place of the theoretical planet," to quote from his communication<sup>6</sup> of May 4th, 1847, to the American Academy, "which

<sup>1</sup> *Mécanique Céleste*, Bowditch's Transl., I. p. 656 ; IV. p. 126.

<sup>2</sup> *Proc. Amer. Acad.*, I. p. 68.

<sup>3</sup> *Smithsonian Contributions*, II. App. I. p. 6.

<sup>4</sup> *Proc. Amer. Acad.*, I. p. 334.

<sup>5</sup> *Boston Courier*, April 30, 1847.

<sup>6</sup> *Proc. Amer. Acad.*, I. p. 144.

might have caused the observed irregularities in the motions of Uranus, would, in two of them, be about one hundred and twenty degrees from that of Neptune, the one being behind, and the other before, this planet. If Le Verrier or Adams had fallen upon either of the above solutions instead of that which was obtained, Neptune would not have been discovered in consequence of geometrical prediction. The following are the approximate elements for the three solutions, at the epoch of January 1st, 1847:—

	I.	II.	III.
Mean longitude, . . . .	319°	79°	199°
Longitude of perihelion, . . .	148°	219°	188°
Eccentricity, . . . .	0.12	0.07	0.16

In each of these the mass is 0.0001187, and the period of sidereal revolution double that of Uranus.”

Continuing the investigation, Prof. Peirce arrived at another singular result. He found himself unable to reconcile the observed motions of Neptune with the assumption, that it was the chief source of the unexplained irregularities in the motion of Uranus. This result was in all candor acknowledged to the Academy, but with the remark<sup>1</sup> that he considered “it would be presumptuous in him to claim for his investigations a freedom from error which the greatest geometers have not escaped, especially in the face of the vastly improbable conclusion to which his analysis tended.” He subsequently<sup>2</sup> succeeded, as is well known, in entirely explaining the motions of Uranus by the action of Neptune, using the mass  $\frac{1}{20000}$ . The failure of the first attempt was in consequence of his not having carried the development of the disturbing function to a sufficient number of terms, but having contented himself in the first approximation with the number of terms used<sup>3</sup> by Le Verrier in his formulas. The numerical formulas of Le Verrier’s *Recherches* can only

<sup>1</sup> *Proc. Amer. Acad.*, I. p. 145.

<sup>2</sup> *Ibid.*, I. p. 332.

<sup>3</sup> *Recherches*, § 94.

apply to mean distances within the limits assigned by that geometer.

During the summer of 1847, Peirce determined the approximate perturbations of Neptune by the other planets, and communicated <sup>1</sup> them to Walker in November. Walker, using these values, deduced a pure elliptic orbit, from the discussion of 689 observations, including those of Lalande. This orbit,<sup>2</sup> together with the normal places on which it was founded, and with the expressions for the heliocentric coördinates, was presented to the American Academy, December 7th. At the same meeting, Peirce communicated<sup>3</sup> the corresponding values of the entire perturbations of Neptune up to the terms depending on the cubes of the eccentricities. He used the masses of the planets assumed by Le Verrier in his theories of Mercury and Uranus, with the exception of the mass of Uranus, for which he took Lament's determination.<sup>4</sup> The results were given both in the usual form and in that which Le Verrier had adopted in his theory of Mercury. Peirce also gave<sup>5</sup> the particular values of the perturbations of the true anomaly and radius-vector, for the epoch of Lalande's observations, and for every three months from October, 1846, to January, 1851. Through the labors of Peirce and Walker, the elliptic orbit of Neptune, with the numerical values of its perturbations by all the other planets, was thus known at the beginning of December, 1847, with such accuracy, that an ephemeris<sup>6</sup> founded on these data satisfied the observations of September, 1848, within one and a tenth seconds of time in right-ascension, and six and a half seconds in declination.<sup>7</sup>

Applying anew Peirce's second values for the perturbations,

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<sup>1</sup> *Proc. Amer. Acad.*, I. p. 285.

<sup>2</sup> *Ibid.*; *Proc. Amer. Phil. Soc.*, IV. 378.

<sup>3</sup> *Proc. Amer. Acad.*, I. pp. 287-295.

<sup>4</sup> *Mem. R. Ast. Soc.*, XI. p. 59.

<sup>5</sup> *Proc. Amer. Acad.*, I. pp. 294, 295.

<sup>6</sup> *Astr. Nachr.*, XXVII. p. 347.

<sup>7</sup> *Ibid.*, XXIX. p. 191.

and using still later observations, Walker presented,<sup>1</sup> March 6, 1848, a second pure elliptic orbit, which has represented<sup>2</sup> the course of Neptune so well up to the present time, as to render a nearer approximation unnecessary, if indeed it were possible.

The perturbations of Uranus by Neptune were also communicated<sup>3</sup> by Prof. Peirce on March 6th. This geometer had found<sup>4</sup> Neptune capable of entirely accounting for all the motions of Uranus, provided a mass of about  $\frac{1}{20000}$  be adopted.

The determination of the mass has been a problem of some difficulty. Soon after the discovery of the planet, Mr. Lassell, of Liverpool, discovered<sup>5</sup> a satellite. Observations of this satellite, made at Pulkowa, at Liverpool, and at Cambridge, have given masses\* for Neptune differing very considerably from one another. The question must be expected to remain unsettled for some time. Peirce seems to have provisionally adopted the mass ( $\frac{1}{13640}$ ) deduced by him from Cambridge observations alone.

The following table<sup>6</sup> of differences between the observed and calculated longitudes of Uranus is taken from Peirce's communication, and shows how well the motions of this planet now accord with theory. The first column contains the discrepancies which exist when the action of Neptune is not taken into consideration. The second and third give those which would remain, did the theoretical planets of Le Verrier and Adams actually exist, and act upon Uranus. The third column contains the discrepancies, after allowing for the influence of Neptune, supposing the mass to be that deduced by Peirce from the Cambridge observations alone.

\* Struve found from Pulkowa observations  $\frac{1}{14496}$ . Peirce, from English and American observations,  $\frac{1}{18780}$ ; from American observations alone,  $\frac{1}{19540}$ .

<sup>1</sup> *Proc. Amer. Acad.*, I. p. 331.

<sup>2</sup> *Smithsonian Contrib. to Knowl.*, II., Appendix I. p. 5.

<sup>3</sup> *Proc. Amer. Acad.*, I. p. 334.

<sup>4</sup> *Ibid.*, p. 332.

<sup>5</sup> *Astr. Nachr.*, XXVI. p. 165.

<sup>6</sup> *Proc. Amer. Acad.*, I. p. 333.



## RESIDUAL DIFFERENCES BETWEEN THE THEORETICAL AND OBSERVED LONGITUDES OF URANUS.

Date.	Without any exterior Planet.	By Le Verrier's Theory, with Mass = $\frac{1}{3322}$ .	By Adams's Theory, with Mass = $\frac{1}{5066}$ .	Introducing Neptune with Peirce's Mass = $\frac{1}{18640}$ .
1845,	+ 6.5	- 0.3	+ 10.2	- 0.9
1840,	+ 0.7	+ 2.2	+ 1.3	- 1.1
1835,	- 4.5	- 0.8	- 1.2	+ 2.0
1829,	- 7.8	- 2.2	+ 2.0	+ 0.8
1824,	- 7.6	- 5.4	+ 1.7	- 2.0
1819,	+ 3.8	+ 0.4	- 2.2	+ 1.0
1813,	+ 4.5	- 0.9	- 1.0	- 0.3
1808,	+ 3.8	+ 0.8	0.0	- 0.4
1803,	- 3.4	+ 0.8	+ 1.6	+ 0.8
1797,	- 6.7	- 1.0	- 0.5	+ 0.3
1792,	- 7.8	+ 0.3	- 1.1	+ 0.3
1787,	+ 2.0	- 1.2	- 0.2	- 0.5
1782,	+ 20.5	+ 2.3	0.0	- 3.0
Ancient. {	1769,	+ 123.3	+ 3.7	+ 1.8
	1756,	+ 230.9	- 4.0	- 4.0
	1715,	+ 279.6	+ 5.5	- 6.6
	1690,	+ 289.0	- 19.9	+ 50.0
				+ 0.8

Beside that solution of the "inverse problem of the perturbations of Uranus," which Le Verrier and Adams obtained, we have seen that, *using their data*, there are a number of other solutions, one of which corresponds to the orbit and mass of Neptune. Had Le Verrier been in possession of observations intermediate to those which he used, he would not have arrived at so harmonious results. The fact, however, that Neptune does not correspond to his solution, cannot in the least detract from the merit or intrinsic value of his investigations. These may be looked upon as a complete discussion of two distinct problems.

In the first place, Le Verrier demonstrated the existence of a disturbing planet. He solved this problem completely by proving, not only that it was impossible to represent the motions of Uranus without the assumption of some unknown disturbing body, but that the perturbations were of that analytical form

which belongs to the influence of an exterior planet. In so far as this goes, Le Verrier may be considered the discoverer of Neptune.

In his solution of the second problem, that of the orbit and mass, he was not so successful. But, inasmuch as, by using all the observations within his reach, he found an orbit and mass capable of accounting for the observed motions of Uranus, he must be, in the opinion of mathematicians, legitimately entitled to all the admiration which he would have received had such a planet actually moved in that orbit. He omitted, it is true, the consideration of the terms depending on a near approach to commensurability; but this, although certainly a defect, cannot be considered as an error in the theory, for, within the limits where he had reason to suppose that the orbit was situated, these terms are almost uniformly negligible. His laborious and elegant researches have been crowned with brilliant success, and Mr. Le Verrier himself rewarded by the consciousness of having been the immediate occasion of the discovery of Neptune. And although the agreement of Neptune's direction at the time of discovery with the direction of the theoretical planet was but accidental, it almost seems as though the heavens strove to show themselves propitious, so happy was the accident, so wonderful the coincidence.

In order to show as clearly as possible the relative positions of Neptune, and of the theoretical planets of Le Verrier and Adams, I have computed the following table, which gives the true longitude and the radius-vector of each of the three, for every tenth year of the two centuries immediately succeeding Flamsteed's first observation of Uranus. It will be observed from this table, that the longitude of the planet of Le Verrier's theory coincided with that of Neptune in 1840; and that Neptune would be in conjunction with Adams's planet about the year 1856. The closest agreement of the radius-vectors was not far from the year 1830, and the greatest discordance in the years from 1710 to 1720, at which time the distance of Neptune was

about midway between that of Uranus and that of the two hypothetical planets.

Date.	TRUE LONGITUDE.			RADIUS-VECTOR.		
	Neptune.	Le Verrier.	Adams.	Neptune.	Le Verrier.	Adams.
1690,	341.1 <sup>o</sup>	65.1 <sup>o</sup>	81.3 <sup>o</sup>	29.92	39.09	40.69
1700,	4.4	79.2	94.5	29.84	39.63	41.30
1710,	26.7	93.0	107.3	29.79	40.89	41.66
1720,	49.1	106.5	119.8	29.77	40.04	41.74
1730,	70.8	120.2	132.0	29.80	39.85	41.48
1740,	93.8	134.0	145.5	29.86	39.40	41.09
1750,	116.0	148.2	158.9	29.95	38.71	40.39
1760,	138.0	163.0	172.6	30.04	37.84	39.46
1770,	160.1	178.9	187.3	30.10	36.93	38.37
1780,	181.8	195.1	202.7	30.22	35.68	37.15
1790,	203.5	212.7	219.2	30.28	34.58	35.91
1800,	225.9	231.4	236.8	30.30	33.57	34.74
1810,	246.8	251.2	255.5	30.28	32.78	33.75
1820,	268.5	270.9	275.2	30.23	32.64	33.06
1830,	290.2	291.4	295.5	30.15	32.29	32.77
1840,	312.0	312.0	315.9	30.02	32.63	32.91
1850,	334.2	332.0	335.9	29.96	33.32	33.47
1860,	356.4	351.0	355.0	29.87	34.26	34.37
1870,	18.7	8.9	14.1	29.81	35.43	35.49
1880,	41.1	25.8	29.9	29.77	36.48	36.73
1890,	63.4	39.4	45.7	29.78	37.99	37.97

Mr. Adams has taken no personal part in the controversies which have arisen since the discovery of Neptune, but has continued to devote himself to the pursuit of science. Mr. Le Verrier has published<sup>1</sup> several articles in the *Comptes Rendus*, in order to defend his claim to be considered the actual discoverer of Neptune, by showing that this planet might have been brought within the limits of his theory. In England, Sir John Herschel has taken<sup>2</sup> similar ground in favor of Mr. Adams.

<sup>1</sup> *Comptes Rendus*, 1848, Sept. 11th, Oct. 2d, etc.

<sup>2</sup> *Outlines of Astronomy*, pp. 309, 509-512, 516-518.

The argument of Le Verrier is,<sup>1</sup> that, since he was obliged to found his computations upon irregularities, which, on account of the probable inaccuracy of the observations, were uncertain by a tenth part of their whole value, it would be very natural that this want of precision should affect the positions thence deduced for the disturbing planet, and that these positions should themselves be wrong by their tenth part.

I should not have alluded to this reasoning had not Mr. Le Verrier published it, and it will perhaps even now be considered unadvisable to endeavor to refute it. According to this argument, an error of 3.0 would be allowable in the mean distance 30, — of 4.0 if the mean distance were 40, etc., — errors which would make the attraction of the planet to be exerted in a direction totally different from the true one. But even this allowance would not correct the error of the radius-vector in 1710 and 1890.

As a rejoinder to the last argument, Mr. Le Verrier says,<sup>2</sup> that, “when there are perturbations, he can tell where Neptune is,” but to demand that he “should do it long after the perturbative action has disappeared, is simply to exact of him what is impossible, — a species of miracle.” Yet at the time of the early observations, when the radius-vector of Neptune differed from that of the theoretical planet by ten times the radius of the earth’s orbit, Uranus was, according to Mr. Le Verrier’s theory, undergoing a perturbation by Neptune.<sup>3</sup> It is unnecessary to allude in this connection to Le Verrier’s deduction<sup>4</sup> from his computations, that the small eccentricity of Neptune’s orbit would be *incompatible* with the nature of the perturbations of Uranus.

With regard to the mass, Le Verrier uses a somewhat different argument. Assuming the mass to be, as Peirce has found,  $\frac{5.2}{100}$  of what had been predicted, he shows<sup>5</sup> that this corresponds to an error of but a fifth in the diameter of Neptune. This is very true, — no schoolboy will deny it, — but it was the mass,

<sup>1</sup> *Comptes Rendus*, XXVII. p. 273.

<sup>4</sup> *Comptes Rendus*, XXIV. p. 531.

<sup>2</sup> *Ibid.*, p. 275.

<sup>5</sup> *Ibid.*, XXVII. p. 277.

<sup>3</sup> See Herschel’s *Outlines of Astronomy*, p. 517, § 776.



not the diameter, which he sought. He has farther alluded<sup>1</sup> to the fact, that, even now, astronomers are in doubt concerning the exact mass of Uranus, inasmuch as the masses deduced from its action on Saturn and from the periods of the satellites do not harmonize with each other; and has thence inferred that the same discordance should be expected between the masses of Neptune as deduced from the perturbations of Uranus and from the satellite observations. It is an interesting question, and one which still remains open, whether the discrepancies between the two computations of the mass of Uranus might not be reconciled by a proper investigation of the influence of Neptune upon Saturn. This is one of the most important questions, connected with Neptune, which remain undecided, and it is earnestly to be hoped that some one of the three illustrious geometers who have labored so faithfully upon the perturbations of Uranus by Neptune may now investigate the Saturn-perturbations produced by the new planet.

The only other point of Mr. Le Verrier's argument to which I will allude is that in which he says,<sup>2</sup> — "The orbit calculated by Mr. Walker, from a position in 1795, and the small arc observed since the discovery, can very well be erroneous by many degrees, either in 1887 or in 1757, and if I have admitted the positions which it has given for these epochs, it is solely by courtesy, and because it presents for me no inconvenience." Of this I may be permitted to say, that Mr. Walker's laborious and accurate investigations have given us the orbit of Neptune to a very high degree of precision, and deserve the gratitude and admiration of astronomers, — not such an imputation as this. It would be contrary to all probability should the place given by Mr. Walker's orbit for those years be false by two minutes.\*

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\* Walker's orbit represents the course of Neptune as well as can be possibly desired from 1795 to 1848, an interval of 53 years. The error in 1887 or 1757 would, according to the doctrine of chances, be to that in either of the years above named in the ratio  $65^2 : 27^2 = 5''.8 : 1''.0$ , and the resulting error, therefore, less than six seconds.

<sup>1</sup> *Comptes Rendus*, XXVII. p. 278.

<sup>2</sup> *Ibid.*, p. 327.

We have seen that it represented the observations so perfectly during the opposition of 1848, as to need no correction whatsoever.

The argument<sup>1</sup> of Sir John Herschel is mostly based upon the circumstance, that the directions of the real and of the hypothetical planet were nearly identical at the time of Neptune's discovery, and upon the fact that the radius-vectors were not very different at that time. But surely it cannot be considered as an analogy between the two orbits, that the perihelion of one was so near the aphelion of the other.

Reasoning like this seems, however, utterly inapplicable to researches of such nicety and analytical refinement as characterize those upon the perturbations of Uranus. It would allow to these investigations no other merit than the success with which Neptune's apparent place was approximately predicted. It is an effort to show that the uncertainty of the calculations was so great, that Neptune's perturbative influence may be included within their limits.

The debt of gratitude which astronomy owes to Le Verrier and to Adams may not be thus diminished. The arguments, which tend to prove that Neptune is the planet of their theory, can only be based upon the supposition of error in that theory, a supposition which I am unwilling to admit. Investigations conducted with the care and precision which characterized these must not be so lightly dealt with. The combined labors of Le Verrier and Peirce have incontrovertibly proved, that, by reducing the limits of error assumed for the modern observations to 3'', there can be but two possible solutions of the problem. There are two different mean distances of least possible error, — one of which is 36, and the other 30. The one is included within the theory and limits of Le Verrier, and corresponds with Adams's solution; the other is the orbit of Neptune.

This simple view of the case — a view which it seems to me impossible for those not interested in the matter to avoid taking

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<sup>1</sup> *Outlines of Astronomy*, pp. 511, 516.

—reconciles all the computations and observations, as well as the discords and contentions. It does not detract in the slightest degree from the well-earned fame of the illustrious geometers, who had arrived at a solution of the problem, and I am not aware that it has ever been opposed by mathematical reasoning.

CAMBRIDGE, *July*, 1849.

# NOTICE OF THE HAIL STORM

WHICH PASSED OVER NEW YORK CITY, ON THE  
FIRST OF JULY, 1853.

By ELIAS LOOMIS,

Professor of Mathematics and Natural Philosophy in the University of the City of N. York.

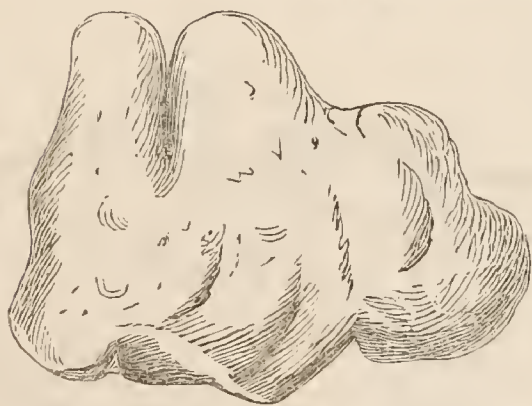
ON the first of July, 1853, a very remarkable hail storm passed over the city of New York. The day had been uncommonly hot and sultry, the thermometer having risen to 90 degrees, and the air was believed to contain an unusual amount of vapor. A little before 5 o'clock in the afternoon, a heavy black cloud was observed to rise 'in the northwest, the wind at the time blowing moderately from the northeast, and subsequently from the east. As the cloud advanced and covered the northwestern sky, while it was still clear in the southeast, numerous streaks of zig-zag lightning appeared to issue from the front margin of the cloud and descend towards the earth. I noticed the approach of the storm from my lodgings in Eighth street, within a quarter of a mile of the University. About five o'clock the wind came strong from the northwest, and the rain poured down in torrents. Presently I heard a loud thump upon the roof of the opposite house; soon another thump; and presently a third and fourth. I was not long in discovering that the noise was produced by the fall of hailstones of a size such as I had never before witnessed. They were few in number—but their average size was little less than that of a hen's egg; and one or two I am persuaded were fully as large as my fist. They almost invariably broke on striking the pavement; so that I could not secure either of those large stones except in fragments; and moreover the rain was falling in torrents. I however hastened to the yard in the rear of the house, hoping to find some upon the grass which had not been broken in the fall. After the rain had nearly subsided, we



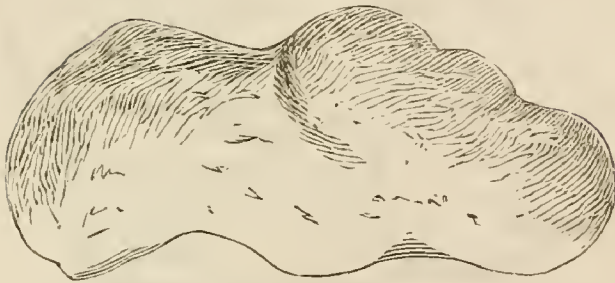
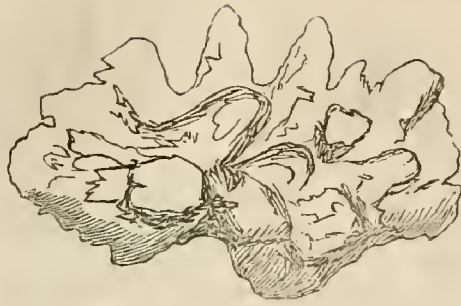
found several handfuls of hailstones of good size, though altogether inferior to those which I saw in the street. They generally consisted of very transparent and solid ice, with many air bubbles; but they were not spongy in the centre as they are sometimes found. Hailstones sometimes occur which appear to be little more than pretty compact snow-balls. In the present instance, the hail was *not* of this kind. The large stones, however, generally consisted of an irregular assemblage of angular pieces of ice, which individually did not much exceed the size of hazel nuts—but they were cemented very firmly together. Indeed there was no appearance of seams or joints between these individual portions—but the ice was equally strong throughout every part of the mass. Their structure therefore did not indicate that several small hailstones were separately formed and were subsequently cemented together; but rather that all were formed simultaneously about a common nucleus. Several persons independently, and without concert, suggested that the conglomerated mass resembled rock-candy; and the comparison appeared to me to be a very just one. There was a decided appearance of a tendency to crystallization. This tendency was in many cases towards a pyramidal form; in others they bore a resemblance to hexagonal prisms; and in some it appeared to me the tendency was towards a cubical form—though as the angles were all much rounded by the melting of the ice, I did not find any complete eubes.

Several of the stones which we picked up in the yard measured two inches long, and one measured over two and a half inches. These had been lying several minutes in a warm drenching rain; and it is my full conviction that two or three of those which I saw in the street were three and a half inches long, by two and a half inches wide, and they did not appear to deviate much from the spheroidal figure. A friend of mine, who is by profession a painter, and who saw and handled the hailstones, at my request made a sketch of some of the most remarkable of those which we picked up. These are shown in the accompanying figures which are drawn of the natural size. It is to be understood however that these were unquestionably smaller than many which we saw fall.

The rain, accompanied by thunder and lightning, continued for six or eight minutes, when its violence somewhat abated—it returned again with renewed energy, but soon afterwards entirely ceased. Another, but more moderate shower followed half an hour later, yet without either hail or lightning. Throughout the entire storm, the wind had blown with considerable force, but not with destructive violence, in that part of the city which is southwest of the University; and in the lower part of the city there was comparatively little wind.



Figures 1 to 4—Hailstones.



Figures 5 to 8—Hailstones.

In the upper part of the city, however, in the neighborhood of the Crystal Palace, the wind blew with destructive violence. A high brick wall was blown flat to the ground—a block of four wooden buildings (not entirely completed) was prostrated—and a small wing of the Crystal Palace was blown down. The fall of hail was heavy, and considerable glass in the Crystal Palace, and the buildings in the vicinity, were broken.

During the first part of the storm, the lightning was unusually severe. Several buildings and trees in New York and Williamsburgh were struck by the electric fluid, and one or two barns were burned to the ground.

I have succeeded in tracing this storm for a distance of full twenty five miles, and for about two-thirds of this distance have followed the track personally on foot. The portion of the track which I have myself surveyed, commences about a mile and a half southwest of Paterson, N. J., from which point it proceeds in a southeast direction—passing over the village of Acquackanonck, together with the cities of N. York and Williamsburgh—and from this point the storm can be traced with diminished energy to Jamaica Bay. Near Paterson, the wind is believed to have been more violent than in any other part of the above mentioned track. Where it swept through the forests, many large trees, of one to two feet in diameter, were overturned—while others were snapped off and twisted like reeds. This remark applies to a distance of about three or four miles from the commencement near Paterson. In the neighborhood of Acquackanonck, a few trees were overturned—but not a large number. East of Acquackanonck, the track soon crossed the Hackensack meadows where the ground is low and flat, and there were no trees to be overturned. I have obtained no information of the effect of the wind upon the high ridge on the west bank of the Hudson river—but the entire length of the track across New York was marked by violence, as above stated. This region was particularly exposed since it was the highest ground encountered by the storm in its passage across the island. Having crossed East river, the storm passed centrally over Williamsburgh, where it caused more damage than in any other part of its course. The steeples of two churches (the first Presbyterian and the Dutch) were blown down; the roof of a third church was partially blown off—the roofs of a large number of dwelling houses were carried away—the tin from numerous roofs was rolled up in long solid coils, and in some cases carried off—while in others, it clung to the roofs and was left after the storm in long massive windrows.

The breadth of the track near Paterson is thought not to have exceeded half a mile—perhaps was somewhat less than this—and



the destructive violence did not extend beyond these limits, until the storm reached Williamsburgh : but here the wind was almost equally violent over a space a mile and a half in breadth, while houses were unroofed over a track two miles in breadth. Beyond Williamsburgh, the wind was less destructive—the track became broader and less distinctly defined—and the general course deviated more to the east. The storm was reported as severe at Jamaica and South Jamaica. From the commencement near Paterson to Williamsburgh the track did not deviate sensibly from a straight line ; and its course was from N. 40° W. to S. 40° E.

Throughout the entire track here mentioned, hail fell of unusual size. Near Paterson the stones were smallest in size, but most abundant in quantity. The destruction caused to the fruit and the crops was such as not unfrequently occurs in France, but has seldom been witnessed in this country. When I visited the spot a few days after the storm, the trees looked as if they had been pelted by myriads of heavy stones. The leaves were strewn thick upon the ground ; and most of those which still clung to the branches, were riddled through and through, and dried upon the stems. The rails of the fences bore marks of large gashes where the brown weather-worn surface had been nicked off and a fresh surface exposed, as if by a volley of stones from a troop of mischievous boys. Upon the north side of the houses along the track, scarce a pane of glass was left entire, and the clapboards were covered thick with gashes an inch in diameter, where the paint was chipped off. Fields of wheat and rye, which had not been harvested, were beaten down as flat as if a heavy iron roller (such as is sometimes employed for smoothing gravelled walks) had been dragged over them ; and fields of corn were totally destroyed. On some fields, I was assured that after the storm the ice lay in a solid compact mass two inches thick. Large quantities of ice still remained unmelted on the ground the next morning, and a tenant on one of the farms collected a considerable quantity and carried it into Paterson, (two miles distant,) to show to his landlord ; and I was informed that in a hollow, against the side of a house, the ice accumulated to such a depth, that on the evening of July 2nd, the day after the storm, a bushel basket full of ice was shoveled up ; the stones varied in size from that of a pigeon's egg, to a hen's egg. The track of the ice did not deviate much from the track of greatest violence of the wind, and followed the same general direction. but covered a somewhat greater breadth. From Paterson to the Hackensack meadows beyond Acquackanonck, the damage caused by the hail was very great, amounting to nearly an entire destruction of the crops of wheat, rye, oats and corn, as also the cherries, peaches, apples, etc., within the limits of the track.

In the city of New York, the damage done by the hail was not very great ; for the stones were not numerous, although of prodigious size. The ship-yard of Mr. Thomas Collyer at the Dry Dock, was covered with singularly shaped pieces of ice,—one of which was measured and found to be  $6\frac{1}{2}$  inches in circumference—another seven inches, and a third measured three inches long, and two inches thick.

In Williamsburgh the hail appears to have destroyed more glass than in New York. In many houses nearly half the glass was broken in windows which were unprotected on the north side. Over 400 panes of glass were broken from the north side of a single school house.

On the same day with the preceding storm, large hail is said to have fallen at several places in Pennsylvania. About three o'clock in the afternoon, a terrific hail-storm passed over Northumberland doing great damage. Hail-stones are said to have been picked up measuring  $7\frac{3}{4}$  inches in circumference ; and several thousand panes of glass were broken in that town.

At  $5\frac{1}{2}$  o'clock, P. M., a severe hail-storm passed about 20 miles north of Philadelphia. At Upper Dublin, the storm was very destructive. Several barns were unroofed, many fruit and forest trees were blown down—and many fields of wheat and oats so badly damaged, as scarcely to pay for harvesting. One hailstone was measured, and its greatest circumference found to be  $6\frac{1}{2}$  inches, and its smallest five inches,—and this was half an hour after the storm had abated. At Norristown and Doylestown the crops were much injured by the hail, and at Burlington, N. J., the wind was exceedingly violent.

It is not probable that either of these storms was the same as that which passed over New York. The hail-storm near Philadelphia, was about simultaneous with that at New York. The storm at Northumberland may have been identical with that at Upper Dublin, the distance of the places being 100 miles—the interval of time  $2\frac{1}{2}$  hours—and the direction nearly parallel with the track of the New York storm.

It would appear that a violent wave of great extent set in from the northwest, which rolled over both New York and Philadelphia, and within this wave were formed about simultaneously several distinct veins of hail.

*Was the storm which passed over New York a whirlwind ?*

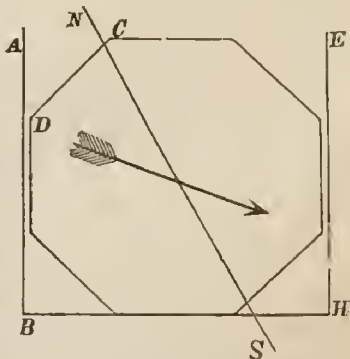
I have surveyed every part of the track of the storm where I have heard of any violent effects, especially with reference to the decision of this question. Throughout Williamsburgh, I could find no unequivocal evidence of rotation. The steeples which were prostrated, fell in a direction coinciding very nearly with that of the storm's progress, that is, towards the southeast. In the case of one of the churches whose steeple was blown down

(the first Presbyterian Church) I noticed a phenomenon which I considered worth recording. The track crossed the ridge of the church at an angle of about  $45^{\circ}$ . On the leeward side, the tin roof was started from the boards (but not broken) and puffed up forming a wrinkle about twenty feet long, two or three feet wide, and ten inches high. This appears to me to indicate the operation of a force beneath, pushing up the tin; but not being able to tear the tin open, bulged it up and left it in a ridge.

This phenomenon appears to be analogous to what often occurs in tornadoes, and I ascribe it to a rarefaction of the air on the leeward roof. A current of air, forcibly impelled over an obstacle like the roof of a building, by friction drags along with it the air lying upon the leeward side of the roof, producing a partial rarefaction, and the air beneath by its expansion, tends to lift the roof. Thus the leeward roof is often carried away, while the windward roof remains. In the present case, this upward pressure lifted the tin about ten inches, stretching but not tearing it. This force appeared to be insufficient to tear the tin from its fastenings—perhaps because from the carrying away of the steeple, and the ripping up of the adjacent edge of the tin, the air beneath found a ready escape.

In the neighborhood of the Crystal Palace, occurred a phenomenon which appeared to indicate the existence of currents blowing nearly in opposite directions. The wooden buildings, which have already been mentioned, were blown toward the southeast—but the brick wall, the line of which run from  $N. 28^{\circ} E.$  to  $S. 28^{\circ} W.$ , fell toward *the west*; that is, in a direction nearly contrary to that of the storm's progress.

The following appears to me to be the explanation of this phenomenon. The Latting Observatory is an octagonal tower, 300 feet high and 75 in diameter at base, sloping uniformly to the top. In the annexed figure, the octagon represents the base of the tower, and the line  $NS$  represents a meridian. On the west side of the tower, was erected a brick wall  $AB$ , 25 feet high, and only three feet from the side of the tower. At the south end, it was connected with another wall  $BH$ , but at the north end it was entirely free, and had no support except an iron clamp projecting from the side of the tower. The direction of the storm's progress is indicated by the arrow. It might have been anticipated that the wall  $AB$  would have been thrown towards the east upon the tower; whereas in fact it was thrown outwards towards the west. But we know





from the testimony of spectators, that this wall fell at the first onset of the blast, when, as we shall presently see, the wind blew nearly from the north, or perhaps a little east of north. Now a violent current from the north, driven into the triangular space A D C, would necessarily become condensed between the wall and the tower, exerting a force to push the wall outward, and the wall had little strength to resist the pressure, being weak not only from its great height, but also being unsupported at the north end. The bricks also had been recently laid, and the mortar was not yet dry. On the east side of the tower, was a similar wall E H, but only 14 feet high, which was not prostrated. Its security is probably to be ascribed to its inferior height.

The following facts at first seemed to me a little puzzling. Near Paterson, a large oak limb, a foot thick, was twisted off at the height of fifty feet, and prostrated in a direction pointing S.  $20^{\circ}$  E., the top of the limb lying towards the base of the tree. Within a short distance I found another large oak limb, torn off at a great height and thrown towards S.  $40^{\circ}$  E., with its top also turned towards the base of the tree. Not far off, I found a third limb in a similar position. Broken limbs were generally found to have been carried eastward, with the top pointing to the S. E., and the base towards the N. W. The three cases I have here specified were exceptions to the general rule, and it appears to me that they are to be explained by supposing that the branches turned a somerset in falling.

A like case occurred with the steeple of the first Presbyterian Church in Williamsburgh. The spire fell across the street—the top struck a brick house on the opposite side of the street and broke off, while the upper portion of the spire remained imbedded in the roof of the house which was crushed in by the blow. The remainder of the spire, which was now the frustum of a pyramid, fell down into the street; but it is probable that the smaller end of the frustum struck the pavement first, for the steeple turned a somerset along the street towards the east, and lay after the storm with the smaller end of the frustum turned *towards* the church.

A similar supposition will satisfactorily account for the observed position of the three limbs above mentioned.

In the woods between Acquackanonck and Paterson, I measured with a pocket compass the direction of a large number of prostrate trees. The following list shows the entire range of the bearings which I measured; *not* arranged in the order in which they were measured, but classified according to the points of the compass. They were

S.  $70^{\circ}$  W.; S.  $20^{\circ}$  W.; S.  $15^{\circ}$  W.; S.  $10^{\circ}$  W.; south; S.  $10^{\circ}$  E.; S.  $20^{\circ}$  E.; S.  $25^{\circ}$  E.; S.  $30^{\circ}$  E.; S.  $35^{\circ}$  E.; S.  $40^{\circ}$  E.; S.  $45^{\circ}$  E.; S.  $50^{\circ}$  E.; S.  $60^{\circ}$  E.; S.  $70^{\circ}$  E.; east.



These bearings were measured at various points upon a portion of the track about two miles in length; and it will be noticed that there is not a single instance of a tree which was prostrated towards any point between East, North and West. The bearings extend from east, through south, to S.  $70^{\circ}$  W., a range of  $160^{\circ}$  degrees; but I found only one instance of a bearing approaching nearly so close to the west point. With but one exception, the bearings were all confined between east and S.  $20^{\circ}$  W. The wind did not then blow from every point of the compass indifferently, at least not with sufficient force to prostrate trees, but it blew only from the northward, including northeast and northwest. Neither was the wind a simple rectilinear current. What law then did the wind observe? Was its motion merely centripetal? Did it revolve in a whirl? Or did it follow some other law?

In order to decide these questions, I attempted to apply the method which I had successfully practised in the Mayfield tornado of Feb., 1842. This method consisted in selecting groups of prostrate trees which lay upon each other, and measuring successively the bearings of the bottom tree, of the next in order, and so on to the top, and regarding these bearings as indicating the successive directions of the wind at the point of observation, as the storm passed by. At Mayfield I had no difficulty in finding groups of trees piled upon each other, frequently four or five in a group; but at Paterson I found in no case more than two trees crossing at a considerable angle, and only five instances of this description. The following are the observations in the cases referred to, the bearing first mentioned in each case being that of the bottom tree.

1.  $\left\{ \begin{array}{l} \text{S. } 50^{\circ} \text{ E.} \\ \text{S. } 20^{\circ} \text{ E.} \end{array} \right.$  2.  $\left\{ \begin{array}{l} \text{S. } 40^{\circ} \text{ E.} \\ \text{S. } 25^{\circ} \text{ E.} \end{array} \right.$  3.  $\left\{ \begin{array}{l} \text{S. } 20^{\circ} \text{ E.} \\ \text{S. } 40^{\circ} \text{ E.} \end{array} \right.$  4.  $\left\{ \begin{array}{l} \text{S. } 40^{\circ} \text{ E.} \\ \text{S. } 10^{\circ} \text{ E.} \end{array} \right.$  5.  $\left\{ \begin{array}{l} \text{S. } 70^{\circ} \text{ W.} \\ \text{S. } 60^{\circ} \text{ E.} \end{array} \right.$

The first four cases present no angle greater than  $30^{\circ}$ ; the fifth case presents an angle of  $130^{\circ}$ ; that is, the two trees were turned in nearly opposite directions.

From a comparison of all the facts, I conclude that the wind blew first from the northeast, and that this current was succeeded by a north and presently a northwest wind. The following are my reasons for this conclusion.

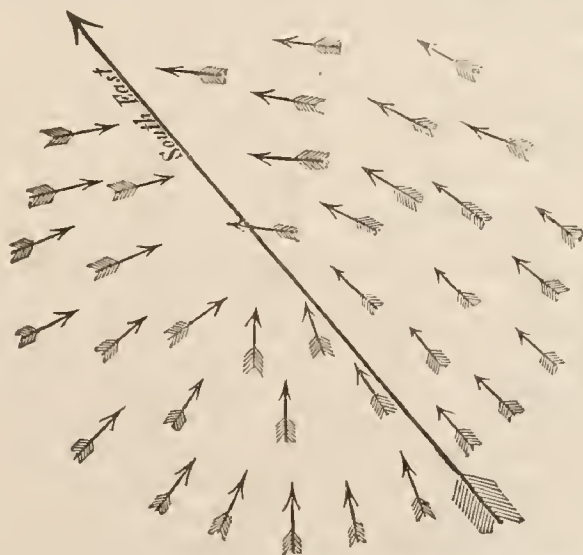
1. The fifth case of interfering trees, just mentioned, taken in connection with all the bearings observed, points to this conclusion. We find that one large tree was prostrated with its top turned towards a point S.  $70^{\circ}$  W. Upon it lay another large tree with its top turned S.  $60^{\circ}$  E. We may safely infer that these directions corresponded nearly with the direction of the wind when the trees were prostrated, and that the wind came first from a point N.  $70^{\circ}$  E., and was succeeded by a current from N.  $60^{\circ}$  W. In each of the other cases of interfering trees, the angle of crossing was so small, as to convey no very distinct information

on this question. In three cases out of the four, the first blast appears to have been a little more westerly than the final one ; but all the trees were prostrated by a northwest wind.

2. A very intelligent farmer, whose house was close upon the northeast margin of the track, about four miles from Paterson, gave the following testimony. He first took refuge from the hail under a shed on the southwest side of his barn, the wind then blowing from the N. E. After a short time, the hail began to beat upon him, the wind having veered to the N. W., and he was obliged to seek a shelter on the southeast side of his barn in order to escape the hail.

3. It is known from the testimony of several individuals, that the wind at New York was easterly on the first approach of the storm.

Upon comparing these facts, it appears to me that the direction of the wind at the time of its most destructive violence may be tolerably well represented by the annexed diagram, showing a



current from the N. E., on the front of the storm ; and from the N. W. in the rear, the whole having a progressive motion towards the S. E., which would give to each place in succession (unless near the southwest margin of the track) first a N. E. wind, and afterwards a N. W. wind.

I do not then find in this case that evidence of a complete rotation which I have found in some other tornadoes ; but it is possible that at a little elevation above the earth's surface, the rotary motion may have been more decided.

*What was the cause of the hail?*

The hail was caused by a violent upward movement of the air, carrying along with it an unusual amount of vapor, which was suddenly condensed, and at so low a temperature, that it was frozen in large semi-crystalline masses.

That there was a violent upward movement of the air, I infer from the following considerations.

1. Rev. J. W. McLane of Williamsburgh was in the street near his house, and noticed the coming up of the storm. He says the cloud was very dense and black—moved rapidly forward—and under the main sheet, the clouds boiled up in a violent and angry manner, which led him to anticipate a severe blast. Other observers have testified to substantially the same facts.

2. It appears impossible that two currents, in close juxtaposition, should blow from nearly opposite quarters, with sufficient violence to prostrate large trees, unless there is opportunity for the air to escape by an upward movement. This conclusion is also in perfect harmony with what we have frequent occasion to observe in small sand whirls and water spouts.

*How was the cold which formed the hail produced?*

According to the observations of Pouillet, in France, the temperature of hail-stones when they fall, is sometimes as low as  $25^{\circ}$  Fahr. They must then have been formed at a temperature considerably below that of melting ice—a temperature probably as low as  $20^{\circ}$  Fahr. How can so low a temperature be produced in the hottest weather of July? The temperature of the air diminishes as we ascend from the earth, and at the height of 8800 feet above New York, is estimated at  $32^{\circ}$  in summer. At the height of 12000 feet the temperature is reduced to  $20^{\circ}$ . Were the hail stones in the present case, formed at an elevation of 12000 feet? It does not appear to me that we are at liberty to make such an assumption. In the summer of 1835, several hail-storms passed over the southern part of France, where there were insulated peaks of mountains, which afforded precise means of measuring the elevation of the hail. In the storm of July 28th, 1835, no hail fell on the summit of the Puy du Dome, an elevation of 4800 feet above the sea; but a few stones fell at the height of 3700 feet, while at the foot of the mountain, the ground was covered to the depth of three inches, and some of the stones weighed eight ounces. On the 2d of August of the same year, a hail cloud enveloped the summit of the mountain, rising therefore to the height of at least 5000 feet.

It does not therefore appear to me that we are at liberty to assume that the hail of July 1st, was formed at an elevation much exceeding 5000 feet, and here the summer temperature may be estimated at  $46^{\circ}$ . This cold is of course insufficient to form ice.



It is believed that during the passage of a hail storm, the temperature of the upper air is considerably below the mean. The simple presence of clouds in the lower atmosphere would tend to produce such an effect. The atmosphere derives its heat from the earth, and is but little affected by the direct passage of the solar rays. The heat which the earth imbibes from the sun is continually thrown off by radiation;—but when the surface of the earth is covered by a cloud, this radiant heat is intercepted, and the temperature of the lower air is thereby elevated. On a still night, the presence of clouds sometimes causes the thermometer to stand ten or fifteen degrees higher than it would otherwise. But if, by the interposition of a cloud, the lower atmosphere becomes unusually hot, the atmosphere above the cloud must receive less than its usual supply of heat, that is, it must become unusually cold.

Moreover, in the storm of July 1st, the hail was formed in a current blowing violently from the northwest, which came therefore from a higher latitude, and of course brought with it a diminished temperature. I have no data sufficiently precise for estimating the effect to be ascribed to this cause, but I think we may conclude that at the time of the storm in question, at an elevation of 5000 feet above New York, the temperature could not have differed much from 32°. We have not however yet reached the temperature necessary to the production of hail.

Another source of cold is to be found in the evaporation from the surface of the hailstones. It is well known that if we tie a piece of thin muslin upon the bulb of a thermometer, and then after dipping the bulb in water, swing it rapidly through the air, the thermometer will sink below the temperature of the air, several degrees, sometimes ten or fifteen; an effect due to evaporation. During a hail storm, the hot air from the earth's surface is carried by the upward movement to a considerable elevation, by expansion it is cooled, and a portion of its vapor is condensed. The drops thus formed at a temperature not far from 32°, are still further cooled by evaporation from their surface, (the evaporation being promoted by their rapid motion;) the remainder of the drop is congealed; and as new vapor is precipitated, it is congealed upon the former;—thus forming concentric layers round the nucleus. Since water, like nearly every other substance, in passing to the solid state, inclines to crystallization, the ball as it increases, does not generally retain the spherical form, but shoots out irregular prisms.

*How does a hailstone remain suspended in the air long enough to acquire a weight of half a pound?*

This difficulty is not, to my mind, a very formidable one. I conceive that hail stones are formed with great rapidity. The vapor is condensed with great suddenness and almost instantly frozen. I think very large hailstones may be formed in five



minutes. In a vacuum, a stone would fall from the height of 5000 feet in less than 20 seconds—but drops of water and hail stones fall with only a moderate velocity. From my own observations of the hail stones of July 1st, I estimated the velocity of their fall at about 40 feet per second. At the uniform rate of 40 feet per second, a stone would be more than two minutes in falling 5000 feet.

In order to obtain some reliable data for estimating the velocity of hail stones, I have computed the greatest velocity of a number of small bodies differing in size and specific gravity. Dr. Hutton determined by numerous experiments the resistance of the air to bodies moving with different velocities; and in the third volume of his *Tracts*, p. 218, has given a table of the air's resistance to a sphere 2 inches in diameter. His experiments also indicated that the resistance, of spheres increases in a ratio somewhat greater than the squares of the diameters. This excess for spheres of from 2 to 4 inches diameter was about  $\frac{1}{30}$ th part of the resistance. The second column in the following Table is taken from Hutton's *Tracts*, the resistances for velocities from 50 to 100 being supplied by interpolation. The resistance for a sphere 1 inch in diameter is found by taking one-fourth of the numbers in the second column, and diminishing the result by one-thirtieth part. Each succeeding column is derived from the preceding in a similar manner.

*Resistance of the air to Spheres of different Diameters.*

Velocity per second	Sphere 2 inch- es in diameter.	Sphere 1 inch in diameter.	Sphere $\frac{1}{2}$ inch in diameter.	Sphere $\frac{1}{4}$ inch in diameter.	Sphere $\frac{1}{8}$ inch in diameter.
feet.	ounces.	ounces.	ounces.	ounces.	ounces.
5	0.006	0.001	0.000	0.0001	0.0000
10	0.026	0.006	0.001	0.0004	0.0001
15	0.058	0.014	0.003	0.0008	0.0002
20	0.103	0.025	0.006	0.0015	0.0004
25	0.163	0.039	0.010	0.0023	0.0006
30	0.237	0.057	0.014	0.0033	0.0008
35	0.325	0.078	0.019	0.0046	0.0011
40	0.427	0.103	0.025	0.0060	0.0015
45	0.544	0.131	0.032	0.0077	0.0019
50	0.676	0.163	0.039	0.0095	0.0023
55	0.821	0.198	0.048	0.0116	0.0028
60	0.981	0.237	0.057	0.0138	0.0033
65	1.155	0.279	0.067	0.0163	0.0039
70	1.343	0.325	0.078	0.0190	0.0046
75	1.546	0.374	0.090	0.0218	0.0053
80	1.764	0.426	0.102	0.0249	0.0060
85	1.996	0.482	0.116	0.0282	0.0068
90	2.243	0.542	0.131	0.0317	0.0076
95	2.505	0.605	0.146	0.0354	0.0085
100	2.780	0.672	0.162	0.0392	0.0095
200	11.340	2.764	0.668	0.1615	0.0390
300	25.800	6.235	1.507	0.3641	0.0880

In a vacuum, a body falling under the influence of gravity is continually accelerated; but when a heavy body falls through the atmosphere, the resistance increases with the velocity, until the resistance becomes equal to the weight of the body. When this takes place, there can be no further increase of velocity, and the body will afterwards descend with a uniform motion. In order therefore to determine the greatest velocity which a heavy body can acquire by falling through the atmosphere, it is only necessary to compute the weight of a sphere of given diameter, and then to search in the preceding table for the velocity due to an equal resistance upon a body of the proposed diameter. The following Table exhibits the results for spheres of lead (assuming the specific gravity 11.35), of iron (specific gravity 7.78), of water, of ice (sp. gr. 0.93), and cork (sp. gr. 0.25); the diameters varying from two inches to one-eighth of an inch.

Diam.	Weight of a sphere of					Final velocity of sphere of				
	Lead.	Iron.	Water.	Ice.	Cork.	Lead.	Iron.	Water.	Ice.	Cork.
2 in.	ounces.	ounces.	ounces.	ounces.	ounces.	feet.	feet.	feet.	feet.	feet.
2 in.	27.5132	18.8593	2.4241	2.2544	0.6060	310	257	94	90	47
1 "	3.4392	2.3574	0.3030	0.2818	0.0757	223	185	68	65	34
$\frac{1}{2}$ "	0.4299	0.2947	0.0379	0.0352	0.0095	161	134	49	47	25
$\frac{1}{4}$ "	0.0537	0.0368	0.0047	0.0044	0.0012	117	97	36	35	18
$\frac{1}{8}$ "	0.0067	0.0046	0.0006	0.0006	0.0001	84	70	25	24	12

Thus it appears that a hail stone in the form of a sphere two inches in diameter, falling through a tranquil atmosphere cannot possibly acquire a velocity exceeding 90 feet per second, and spheres of a smaller size would acquire a still less velocity. Also a hail stone of irregular shape would experience more resistance than a sphere, and would acquire a smaller velocity in falling. An upward current of air moving with a velocity of 90 feet per second, or 61 miles per hour, would sustain a sphere of ice two inches in diameter; also an upward current of 30 miles per hour would sustain hail stones of half an inch in diameter, and would greatly reduce the velocity of stones of larger size. The strong upward movement which is known to exist in the neighborhood where hail is formed, is therefore quite sufficient to sustain hail stones of the largest kind as long as they can be kept within the influence of this vortex. After they have entirely escaped from the influence of the vortex, small stones would fall to the earth from an elevation of 5000 feet in about two minutes; and very large stones in one minute. I see no difficulty therefore in supposing the great mass of hail to remain in the air five minutes before reaching the earth—and that in peculiar cases, stones may remain supported for ten minutes and even a great deal longer. This period appears to me sufficient to account for the hail which fell at New York.

*Why did the hail in the present case attain to such unusual size?*

Because of the following circumstances which are unusually favorable to its formation. The temperature of the air before the storm was  $90^{\circ}$ , and it is my opinion that the dew-point could not have been less than  $80^{\circ}$ ; in other words the atmosphere contained about as much vapor as it is ever known to contain in this latitude. This vapor was suddenly lifted to a region of great cold, and rapidly condensed and frozen. The strong upward movement helped to sustain the crystals as they increased in size, until the upward force was no longer equal to gravity—or until they escaped from the influence of the vortex. Most of the stones would fall in five minutes and be of moderate size; others might be sustained ten or fifteen minutes, and attain enormous dimensions.

*How did the hail in this storm compare with the most remarkable cases on record.*

There are well authenticated cases of hailstones having fallen in England and France weighing half a pound—and even more than this—but the accounts of hail stones weighing so much as one pound, do not appear to me entirely satisfactory. A mass of ice of the specific gravity 0.93, weighing eight ounces must contain nearly 15 cubic inches; or would make a cube whose edge is nearly 2.5 inches. I have selected a piece of ice which was estimated to be about the size of the largest stone which I saw fall on the 1st of July, and found it to weigh eight ounces. But these large stones of July 1st appeared to me unusually white, and may therefore be conjectured to have had a spongy nucleus—which would have reduced the weight to perhaps six ounces.

The hail therefore in the present storm was somewhat smaller than has been observed to fall in other places.

Since the preceding was written, I have received notice of several remarkable hail storms in different parts of the United States, two of which were so extraordinary that I have added an account of them to this paper.

*Hail Storm experienced at Warren, N. H., Aug. 13, 1851.*

My first information respecting this storm was derived from a letter from Dr. Peter L. Hoyt, dated Wentworth, Grafton Co., N. H., Aug. 3, 1853. The following is an extract from Dr. Hoyt's letter.

"Perhaps a brief notice of a hail storm which occurred in this vicinity on the 13th of August, 1851 may be of interest to you. This shower, about one o'clock, P. M., passed from the west towards the east over an extent of four or five miles around the base of Moosehillock Mountain, in the towns of Benton and Warren. The largest and most hail fell in the north east part of



the latter town, in a basin between the mountains near the source of a stream called Baker's river. I stood at the railroad depot in Wentworth, at the time of this shower, distant in an air line six or seven miles. It was the most remarkable appearing cloud I think I ever saw—so black and dense, encircling and covering the mountain, and shutting down to the earth.

"The hail was of prodigious size and in great quantities. The largest of the stones was of an irregular shape, rough and angular, suggesting the idea to some that they were made up of a number adhering together. They were however very solid and not easily broken.

"One was weighed upon the spot at the time of the shower, and weighed 20 ounces; and the person who informed me of this was of the opinion that he saw one fall and break in pieces which was still larger. They looked, he said, like vast pieces of ice that had been broken above, and were falling to the earth. A quantity was gathered in a basket and brought to Warren village, a distance of three or four miles, and there exhibited at least an hour after the shower, and in a hot and sultry afternoon. One of them there weighed 14 ounces, and measured 10 inches in circumference. Twelve of the largest taken out of the basket weighed on the counter scales in the store, seven pounds.

"About 4 o'clock, p. m., three hours after their fall, a box of them was brought to Wentworth village, where I reside, a distance of about eight miles. One of them was shown to me. Its diameter according to my best judgment was from 2 to 2½ inches. It had the appearance of being originally somewhat angular, with the angles melted off. It was perfectly solid and clear.

"So large was the quantity of hail in many places in Warren, that a cart load might have been gathered without moving from the place. Luckily the track of the storm was not through the most cultivated part of the towns, but along the borders and skirts of the forests, where the population was scattering. Crops of hay and grain were ground to the earth, poultry were killed—cattle's backs were bruised—and the roofs of many buildings were badly broken. But little glass was broken from the fact that the direction of the hail was nearly perpendicular to the earth."

Immediately upon receiving this letter, I wrote to Dr. Hoyt, stating that the facts which he had communicated respecting the size of the hail were so remarkable, that they ought to be substantiated by such evidence as would be deemed conclusive in a court of justice; that it was therefore important that he should obtain written statements from the identical persons who weighed the stones; and that it would not be derogatory to the dignity of science for these persons to make affidavit to the truth of their statements before a Justice of the Peace. I also suggested several



additional points upon which it was desirable that information should be obtained. In reply I received another letter from Dr. Hoyt accompanied by documents such as I had suggested. The following is extracted from his reply.

"As yet I have been unable to substantiate the weight of a hail-stone at 20 ounces; yet throughout the town of Warren the impression prevails that one was so weighed. The enclosed affidavit of Mr. Libby, and statement of Mr. Flanders fixes the extreme weight of two stones weighed by them at  $17\frac{1}{4}$  and 18 ounces; with the firm belief of Mr. Libby that had he weighed them at the time of falling, their weight would have been increased some two or three ounces.

"I have the names of some two or three other individuals who weighed several stones of a pound weight,—others were weighed weighing three fourths of a pound. One several hours after the storm weighed 14 ounces. A tin pail full containing fifteen, weighed 10 pounds—four collectively weighed three pounds, etc. Incredible as the above may appear to some, they are facts which can be proved by a multitude of evidence.

"This storm was remarkable for the amount of ice which fell as well as the size of the stones. Mr. Flanders thinks that in Benton the average depth of the hail was about four inches, and from enquiry along the track of the storm, I should judge that he is not far from right in his estimate. The extreme width of the hail was about two miles, and the length over a cultivated district perhaps about five or six miles. How far east it extended I have no means of knowing, as it entered a forest of many miles in width. The largest hail stones fell near the edge or skirts of its track; the thickest and greatest amount or depth of hail fell in the centre. Although the sun came out "boiling hot" as one man expressed it, after the shower, still the hail remained on the ground in many places until the next day. An owner of a saw mill, on the stream of water which has its source in the forests over which the shower passed, told me that the water kept swollen for two or three days, when from common showers of rain it would have fallen in twenty-four hours. This he attributed to the gradual melting of so large a quantity of ice in the woods. I think there was but little if any difference in the distribution of the large stones along the track, as the two whose weights are given by Mr. Flanders and Mr. Libby were picked up about five miles apart, and near the extremes of its track before it entered the forest. On the borders or skirts of the cloud the large stones fell scattering, and as it approached the centre it was as if the whole contents of the cloud were let down in ice. During the time of the hail, which lasted some twenty or thirty minutes, there was but little rain; after the hail it rained briskly for ten or fifteen minutes.

*"Shape of the hail.*—In Benton at the commencement of the hail, the masses were angular, having a resemblance to broken ice; while further along the track they assumed a smother and more uniform surface, being oval or oblong. In many instances the surface is described as being notched or scalloped; and in some few as being covered with icy spikes, like icicles somewhat resembling a burr. It is the opinion of those who examined these stones the most minutely, that they were not formed by the union of several masses, but were distinct and individual in formation. They were compact and very solid; so hard that they might be thrown with great force against a house and not be broken.

*"Velocity.*—All agree that the hail fell with great velocity and force. Mr. E. W. Cleasby, a very correct and veracious man, whose statement is appended to this letter, says that hail stones very solid and weighing in the vicinity of 10 or 12 ounces, averaging one on about every two feet, fell in a piece of unmowed grass. In their passage through the grass they entangled it so as to carry it imbedded into the sward ground to the depth of some two or three inches, and after the melting of the hail, left the turf full of holes like little bird's nests. These holes remained through the season. As a test of the force necessary to effect this, he repeatedly with a pitch fork handle having a rounded head, tried to strike it into the ground to an equal depth, and was unable to do it.

"Many of the barns in this neighborhood have their roofs covered with what are styled 'long shingles'—that is with spruce shingles without previous boarding. Whenever these large stones fell upon such roofs, they broke a hole completely through; and one man having sought refuge in a barn under such circumstances, was obliged to hide under the scaffold. The marks and bruises upon the buildings caused by the hail are still to be seen. Says one person, 'they looked like little pumpkins falling.' The roar and rattle of the hail was distinctly heard at the village in Warren, a distance of four or five miles, and was likened to the noise of a heavy train of cars.

*"Wind.*—During the storm there was but little wind. The hail fell nearly perpendicularly. The general bearing of the wind as appears from my weather table on that day, was west-south-west; and the direction of the shower was in correspondence with this.

*"Heat.*—No record of the heat was kept in the neighborhood of the storm. My thermometer at 2 o'clock, p. m., indicated 76 degrees. I very well recollect that after witnessing the passage of this cloud to the north, the sun broke out very hot and scorching. Such also is the testimony of people living there.

"You ask if it was possible that the larger stones could have been formed by the cementing or freezing together of several while lying on the ground? I should think it *impossible* that such could be the case. Furthermore the general opinion among the inhabitants is that each stone was a unit in formation."

The following is a copy of the affidavit of Mr. Libby already referred to.

Warren, N. H., Aug. 24, 1853.

I live in Warren and witnessed the hail storm on the 13th of August 1851, between the hours of one and two o'clock, p. m. I weighed a number of the hail stones which fell at that time, but not until after the shower had ceased—perhaps an hour and a half or two hours after. During this time it was very hot. The largest which I weighed was  $17\frac{1}{4}$  ounces in weight. The others varied in the vicinity of a pound. I am fully in the belief that had they been weighed at the time of falling, their weight would have been some two or three ounces more. Previous to weighing them I washed the dirt from them in water. They were very irregular in shape, somewhat scolloped, with ice projections from their surface. I picked these stones up from soft ploughed ground where they were imbedded more than half their size in the ground. During the time that the hail was falling there was but little rain, with little or no wind. After the hail there was a warm rain of some ten or fifteen minutes duration.

JOHN LIBBY.

Sworn to before me,      JESSE LITTLE, *Justice of the Peace.*

The following is the statement of Mr. Flanders already referred to.

Wentworth, Aug. 30, 1853.

I live in Benton, N. H., County of Grafton, and resided there at the time of the hail storm on the 13th of August, 1851. I weighed a number of the hail stones after the rain was over. The heaviest one weighed 18 ounces; the others ranged in the vicinity of a pound. They were very irregular in shape—some nearly square—some scolloped—some angular as if made up of several pieces. According to my best judgment there was an average depth of four inches of hail which fell at that time.

GRANVILLE E. FLANDERS.

The following is the statement of Mr. Cleasby.

Warren, N. H., Sept. 3, 1853.

This certifies that several of the hail stones which fell here on the 13th of August, 1851, were measured by members of my family. According to my best recollection the circumference of the largest was fourteen inches one way and nine the other. Their form was very generally oval.

EZRA W. CLEASBY.



The preceding evidence satisfies me that hail stones fell in New Hampshire on the 13th of August, 1851, weighing *more than one pound* ; and I do not know of any satisfactory evidence that hail of equal size has ever been seen in any other part of the world.

*Hail Storm at Montrose, Iowa, on the Mississippi River, in Lat. 40° 30', about the middle of June, 1838.*

The following notice of this storm is derived from a letter received from Mr. D. W. Kilbourne, who resided at Montrose in 1838, but now lives at Keokuk, twelve miles below Montrose.

“About four o'clock in the afternoon, a very heavy black cloud rose in the northwest, the wind at the time blowing strong from that quarter. There was much thunder and lightning ; at the same time it was clear in the east and southeast.

Very soon however the whole sky seemed to be covered by clouds ; there was a heavy mist, and it was almost as dark as night. Rain immediately followed, and for a few moments fell in torrents. Then hail stones began to fall. At first they were small and excited no surprise in myself or family ; but they continued to increase in quantity and size to such an extent as to excite not only our wonder but our fears. The hail storm continued nearly ten minutes, and during all the time small and large hail fell together. The wind was high.

As soon as the storm abated so that it was safe to go out, my family were all engaged in picking up the stones. We then selected the largest and measured their circumference. The largest one found *measured ten and one-fourth inches*. There were a large number that measured from two to ten inches in circumference. I gathered up with my own hands in one spot on the grass without moving, a half bushel measure full.

Mrs. Kilbourne placed several of the largest ones upon the top of common sized glass tumblers, and when melted they filled the tumblers so that some of them could not be moved without spilling the water.

The hailstones were irregular in their formation, and presented very much the appearance of rock-candy. The ice was solid and transparent. We did not weigh them.

The hail fell only about half a mile in width, and not more than two miles in length from west to east. No hail fell on the east side of the river. But few white families resided in the neighborhood at that time, or in the county ; and I do not believe the hail stones were particularly noticed or measured except at our house.”





## A PHILOSOPHICAL SURVEY OF THE OCEAN.

*By J. S. MAURY*

*Explanations and Sailing Directions to accompany the Wind and Current Charts, and published by authority of Hon. John C. Dobbin, Secretary of the Navy. By M. F. MAURY, Lieut. U. S. N., Superintendent of the National Observatory.*

MAN has as yet but partially learned the great truth which Nature has ever been striving to teach him, that he no sooner becomes acquainted with the laws which govern any of her operations, than they become subservient to his use, and reveal to him the secret that they were made for that very end. The way, and the only way, which conducts him to this mastery over the powers of nature, lies through the slow but sure process of induction, as prescribed by Lord Bacon.

The method pursued in constructing Maury's Wind and Current Charts, is in exact accordance with the Baconian philosophy. It consists, first, in collecting all the facts of the case; secondly, in classifying those facts, grouping under distinct heads such as are similar; and, thirdly, in observing what language they speak—what new truths they reveal. These constitute principles, and principles in science, when they are applied to practical use, become rules in art.

The first example of the application of this method to the phenomena of the Ocean, was set by Wm. C. Redfield, Esq., of New York, more than twenty years since, in his investigation of the phenomena and causes of Atlantic Gales. After any storm which he proposed to investigate, he collected as many as possible of the log-books of vessels that had been caught in the storm. These he submitted to careful and diligent inspection, noting in what direction each vessel took the wind; how that direction changed during the progress of the storm; and with what degree of violence the wind blew at successive periods. Such a comparison between vessels situated in different parts of the storm, revealed to him the great fact, that the storm was a *whirlwind spinning on its axis like a top, and at the same time making a slow progress along its path*. The same comparison, extended to other storms, indicated an unexpected uniformity in their modes of action; a uniformity which further revealed the surprising fact, that these apparently law-

less and destructive blasts, which seem to be out of the course of nature, are in fact governed by laws no less fixed, than those which control the movements of the planets. They were found to rotate always from right to left; to move with greatly accelerated velocity towards the center of the storm; and to pursue along the coast of the United States, paths which, when plotted on paper, appeared remarkably similar to each other, and of a definite order of curvature. The inquirers into natural phenomena are learning to think that there is nothing "lawless" in Nature, since whenever she is interrogated with precision, her responses are equally precise; and so often has it appeared that events of the natural world which were deemed the most capricious, are essential parts of an established order of things, that it is as good philosophy as poetry to say, that not a dew drop glistens, or a leaf trembles, but helps to fulfill some grand design.

By this happy application of the principles of the inductive philosophy to ocean storms, a new field of philosophical inquiry was laid open, which has since been most sedulously cultivated, not only by Redfield himself, but by Reid, Espy, Thom, Piddington, Dové, and others. The result of these labors has been a set of rules, which will help the mariner who encounters a storm, so to steer his vessel as to escape its violence, while without such a knowledge of the laws of storms, he might run directly into the jaws of destruction.

Although the method pursued by Lieut. Maury in constructing his Wind and Current Charts, is similar to that of Mr. Redfield in investigating the laws of storms, his immediate object is different. It is not so much to assist the mariner to escape the violence of the elements, as it is to enable him to turn them to his own advantage. Instead of regarding them as impediments to his progress, with which he is forced to maintain a desperate and endless warfare, he thus learns to make them quicken his speed, by so timing his voyage and steering his vessel, as ever to sail with the wind and float with the current. Lord Bacon places it among the first of his philosophical maxims, that man is the servant of Nature, and can do nothing only as he is obedient to her mandates; and this is true while he is *learning* her secrets. Nature, until her laws are discovered, is the tyrant, and man the slave; but the instant these laws are understood, man becomes the master and Nature the humble menial, to bear his messages or to drag his car. The lightning, before his terror and his scourge, now submits itself to his authority more truly than in fabulous story it awaited the nod of Jove.

The situation of Lieut. Maury at the grand depository of the log-books of our national marine, would seem to have given him special advantages for such an investigation; but the facts thus amassed in the old log-books were so deficient in precision or defective in details, that he found it impossible to derive from them sufficient data for completing the proposed charts. He next brought the subject before the American Association, (then called the National Institute,) who expressed a deep interest in it, and appointed a committee to urge its importance upon the attention of the government. The Hon. John Y. Mason, then Secretary of the Navy, entered warmly into the plan, as did those enlightened men who succeeded him in office. In 1845, the labor was commenced anew, and a fresh supply of log-books was procured from our men of war. In 1848 were issued the first three sheets of the Wind and Current Charts. They contained only the tracks of men of war, but their utility was at once apparent, for they enabled Mr. Maury to point out at once a shorter, quicker, and better route to Rio than that usually pursued. This was announced as a discovery, and it was soon verified. A Baltimore vessel was the first to try this new route. She crossed the line the 24th day out, (it has since been done in 18 days,) the usual time before being 41 days, and made the trip to Rio and back in 75 days, a period by many days shorter than had before been occupied by the same voyage. Navigators began now more fully to comprehend the object and to understand the utility of these researches, and came forward with offers of hearty and gratuitous coöperation. In a short time a large fleet, without the promise or hope of pecuniary reward, were lending their zealous aid. Ship after ship joined the corps of observers; so that more than a thousand navigators are busied night and day in all parts of the world in making observations, and collecting materials of great value to science, commerce, and navigation.

The marked approbation which the illustrious Humboldt (who is better qualified than any other man that has ever lived, to form a just estimate of the plan) gave to these researches, contributed much to increase their popularity both at home and abroad. He says in a letter to the U. S. Consul at Leipzig, "I beg you to express to Lieut. Maury, the author of the beautiful Wind and Current Charts, prepared with so much care and profound learning, my hearty gratitude and esteem. It is a great undertaking, equally important to the practical navigator, and for the advance of meteorology in general. It has been viewed in this light in Germany by all persons who have a taste for physical geography. The shortening of the



voyage from the United States to the equator, is a beautiful result of this undertaking. The bountiful manner in which these Charts are distributed raises our expectations still higher."

In the year 1851, a proposition came from the British government, inviting the coöperation of our government in establishing a uniform system of meteorological observations, both on sea and land. The opinion of Lieut. Maury being requested, he expressed himself very friendly to the object, but thought there would be insuperable difficulties in carrying the plan into execution here, since a great part of the meteorological observations in the United States, are not subject to the control of the national government, being under the direction of state governments, or learned institutions, or private individuals. Still a uniform system of observations might be arranged between the British and American governments, which should be fully carried out on board their respective ships of war and at their military posts; and contemplating from such a system great benefits to navigation and to science, he proposed a meteorological conference—that England, France, Russia, and other nations, be invited to coöperate with their ships, by causing them to keep an abstract log, according to a form to be agreed upon, and that authority be given to confer with the most distinguished navigators and meteorologists, at home and abroad, for the purpose of devising, adopting, and establishing a universal system of meteorological observations for the sea as well as for the land. The British proposition did not look much to observations to be taken at sea, while the plan of Lieut. Maury contemplated from them the finest results. He urged the utility of a conference upon the subject of a uniform system of meteorological observations on board British and American *ships*, as well as at British and American posts, stations, and observatories, because on board every properly appointed ship of both nations, all, or nearly all, the observations, which would probably be recommended for this universal system, are already made; it being the custom to keep a log book on board every ship, and to enter in it remarks and observations upon the winds, the weather, and the sea; and all that is requisite to impart a new and greater value to these observations is, that they should all be made at the same time, recorded in a stated journal—the "abstract log" kept for the purpose—and then be made available by being returned to the office appointed to receive them.

The Hon. Wm. A. Graham, then Secretary of the Navy, with an enlightened comprehension of the subject and of the advantages it promised, returned an answer to the proposal

from the British government expressive of the high interest felt by his department in the proposition, and authorizing the Superintendent of the Naval Observatory to confer as to such a uniform plan of observations, with the proper officers, at home and abroad, and in concert with them to agree upon a system of observations both on sea and land. Mr. Maury proceeded to publish a pamphlet embodying the features of his plan, to which he gave a wide circulation. Several governments responded to the proposition, as did many learned societies. When the original proposition, as amended by the American government, to include the sea also in the system of research, went back to the British government, it was by that government referred to the President and Council of the Royal Society for a report. It was most favorably entertained by that illustrious body, and the labors of Lieut. Maury are represented to the government in the most flattering terms. The efforts already made in this country up to that time, (1852,) are thus comprehensively exhibited in that Report:

"The proposition and the results obtained, of Lieut. Maury, to give a greater extension and more systematic direction to the meteorological observations to be made at sea, appear to be deserving of the most serious attention of the Board of Admiralty. In order to understand the importance of this proposition, it will be proper to refer to the system of observations which has been adopted of late years in the Navy and Merchant Service of the United States, and to some few of the results to which it has actually led. Instructions are given to naval captains and masters of ships, to note in their logs the *points of compass from which the wind blows*, at least once in every eight hours; to record the *temperature of the air*, and of the *water*, at the surface, and when practicable, at considerable depths of the sea; to notice all *remarkable phenomena* which may serve to characterize particular regions of the ocean, more especially the direction, the velocity, the depth, and the limits of the *currents*. Special instructions also are given to *whalers* to note down the regions where whales are found, and the limits of the range of their different species. Detailed instructions are given to all American ship-masters upon their clearing from the custom house, accompanied by a request that they would transmit to the proper office, after their return from their voyage, copies of their logs, as far at least as they relate to their observations, with a view to their being examined, discussed, and embodied in Charts of the Winds and Currents, and in the compilation of Sailing Directions to every part of the globe. For some years the instructions furnished received very little attention, and very few observations were made or communicated; the publication, however, in 1848, of some charts founded upon the scanty materials which had come to hand, or which could be collected from other sources, and which indicated much shorter routes than had hitherto been followed to Rio, and other parts of South America, was sufficient to satisfy some of the more intelligent ship-masters of the object and real importance of the scheme; and in less than two years from that time, it had received the cordial coöperation of the master of nearly every ship that sailed. Short as is the time that this system has been in operation, the results to which it has led have proved of very great importance to the interests of navigation and commerce. The routes to many of the most frequented ports in different parts of the globe, have been materially shortened; that to San Francisco, in California, nearly one-third; a system of southwesterly monsoons,

in the equatorial regions of the Atlantic, and on the west coast of America, has been discovered; a vibratory motion of the trade wind zones, with their belts and calms, and their limits for every month of the year, has been determined; the course, bifurcations, limits, and other phenomena of the Gulf Stream have been more accurately defined; and the existence of almost equally remarkable systems of currents in the Indian Ocean, on the coast of China, on the northwestern coast of America, and elsewhere, has been ascertained. There are in fact very few departments of the science of meteorology and hydrography, which have not received very valuable additions; whilst the most accurate determination of the parts of the Pacific Ocean (which are very limited in extent) where the sperm whale is found, as well as the limits of the range of those of other species, has contributed very materially to the success of the American whale fishery, one of the most extensive and productive of all the fields of enterprise and industry."

It was ascertained that a number of the leading governments of Europe were averse to making any change in their established systems of meteorological observations on the *land*, while they would heartily coöperate in promoting a uniform system on the sea. It was therefore deemed advisable to confine the proposed concert to observations at sea, and, at the suggestion of Lieut. Maury, official invitations were issued by Hon. Mr. Everett, then Secretary of State, to a conference to be held at Brussels, in August, 1853. The conference met accordingly, and consisted of representatives from Portugal, France, England, Belgium, Denmark, Sweden, Norway, Russia, Holland, and the United States. By order of the Secretary of the Navy, Lieut. Maury was commissioned to attend the conference on the part of our government. The history of this celebrated and important meeting is given in the work before us, in full, and indeed with needless prolixity,—a fault which characterizes a large portion of the documents that proceed from Washington.

The "abstract log," or formula for observations agreed on by the conference, consisted of twenty-two blank columns, with an additional column of "Remarks." It might be tedious to enumerate the various particulars which constitute the headings, but it may be truly said they leave little to be desired, embracing as they do every subject relating to the phenomena of the ocean and the atmosphere that can contribute to our knowledge of either.

Under a system of observations so excellent in itself, and rendered efficient by so many zealous collaborators, we may reasonably anticipate that our knowledge of the ocean will be rapidly extended; that new laws governing its winds and its currents will be successively developed, until it will appear that every wave that rolls, and every breeze that blows is a part of some great system; that from the knowledge thus acquired will arise new facilities for navigation, and be opened new mines of



wealth; that as in other departments of nature, man having now become the master where he was before the slave, he will no longer be borne off his track by the currents, or wrecked by the winds and waves, but will turn to his own account the violence of these elements, and their seeming irregularities, and compel them to speed him on the way. With such visions of the future, we may now advantageously take a comprehensive view of the ocean, in its phenomena, its laws, and its useful products.

The waters of the ocean cover nearly three-fourths (or more exactly, five sevenths) of the surface of the globe; and of the thirty-eight millions of miles of dry land in existence, twenty-eight belong to the northern hemisphere. The mean depth of the ocean has been variously stated, but may for the present be taken at four miles: the numerous soundings now in progress will soon enable us to speak with more definiteness on this point. Enough has already been done to prove that the depth is exceedingly unequal; that like the surface of the earth, the bottom of the ocean here rises in mountain peaks, and there sinks in deep valleys. Until recently the deepest sounding ever made, was that by Captain Scoresby in the polar seas, which was short of a mile and a half. As late as 1848, the maximum sounding was that of Captain Ross, in the South Atlantic, and gave 27,600 feet, or a little over five miles, without finding bottom. But more recently, at a point of the Atlantic farther north, Lient. Walsh, of the U. S. Schooner *Taney*, sounded, without reaching bottom, to the depth of 34,200 feet, or nearly  $6\frac{1}{2}$  miles. Within a short time Captain Denham communicated to the Royal Society a report of having reached the bottom of the Atlantic, in a passage from Rio Janeiro to the Cape of Good Hope, at the astonishing depth of 7,706 fathoms, or  $8\frac{3}{4}$  miles; a depth so profound, that the plummet occupied in its descent from the reel nearly  $9\frac{1}{2}$  hours. From these results it appears that the depths of the ocean exceed the heights of the mountains, since the loftiest summits of the Himalaya are little more than 28,000 feet, or  $5\frac{1}{4}$  miles. Notwithstanding these enormous depths, there are large tracts of the ocean comparatively shallow; and in the immediate vicinity of places where no bottom could be found, were spots of no uncommon depths. These facts indicate that the bed of the sea is diversified like the surface of the earth. The Gulf of Mexico is thought not to exceed on an average one mile; and the Greenland seas are of such moderate depth, that whales, when harpooned, often run to the bottom, as is indicated by their appearance when they rise again to the surface. Whales are even supposed to seek a part of their food at the bottom of the sea.



The *pressure* that bodies must undergo at such vast depths, is enormous. As the pressure of a column of water varies in proportion to the depth, and is found by experiment to amount to 500 pounds on a square foot, at the depth of 8 feet, it would be, at one mile below the surface, on the same area, 330,000 pounds; and at the depth of  $8\frac{1}{4}$  miles (the deepest sounding yet made) it would exceed 1200 tons to the square foot. It has long been known that square bottles let down to even a moderate depth into the sea, are crushed; and that junk bottles, when sunk to a greater depth, come up filled with water, if previously empty, or if before full of fresh water, this is displaced and the bottle, when drawn up, is found full of salt water, the great compression of the cork having permitted the exchange. The late Mr. Jacob Perkins, many years ago, instituted an interesting series of experiments of this kind, during his voyage across the Atlantic, with the view of ascertaining the compressibility of water; and afterwards, in Philadelphia, he applied by means of the hydraulic press, a force no less than nearly 2000 tons to the square foot, without changing the water from the fluid to the solid state, as some have imagined might be the case with water under the pressure sustained by sea water in the lowest depths of the ocean. Water itself, however, by such an incumbent pressure, would be sensibly reduced in bulk, and its density would be proportionally increased; so that substances which, like the human body, but little exceed water in specific gravity, might float at a certain depth, before reaching the bottom, if they did not by the same cause themselves undergo a still greater compression. This is commonly the case with light bodies submerged to a great depth, so that parts of a vessel when wrecked in deep water, which would float near the surface, never rise. The Greenland whale is said sometimes to descend to the depth of a mile, but always to come up exhausted and blowing out blood.

Specimens of the matter that was brought up from the bottom of the sea, by our vessels employed in taking deep soundings, at the depth of more than two miles, were transmitted to Prof. Bailey, of West Point, (well known for his great skill in microscopic examinations,) and were found to be filled with the remains of exceedingly minute animalcules, consisting of calcareous shells. Prof. Bailey thinks it impossible that these microscopic animals lived at the depths where those shells are found, but that their home is near the surface, and that when they die their shells settle to the bottom. Mr. Maury remarks that we are taught thus to view the surface of the sea as a nursery teeming with nascent organisms; its depths as the

cemetery for families of living creatures, that outnumber the sands of the sea-shore for multitude.

The *temperature* of the ocean undergoes but slight variations in the torrid zone, being generally from  $80^{\circ}$  to  $83^{\circ}$ , and in the higher latitudes the variations are much less than on the land. It becomes, therefore, a fountain of cool breezes in summer and of warm gales in winter. In certain parts of the Indian Ocean, the hottest sea in the world, the water reaches the heat of  $90^{\circ}$ . At a certain depth below the surface throughout the ocean, we come to a cold stratum of invariable temperature, that of  $40^{\circ}$ . At the equator this is found at the depth of a mile and a quarter, (7,200 feet,) but it comes continually nearer and nearer to the surface until, in latitude  $56^{\circ}$ , it reaches quite to the surface. North of this the cold water is uppermost, and in latitude  $70^{\circ}$  the depth of the invariable stratum is three-fourths of a mile, (4,500 feet.) Nothing could be more favorably situated for evaporation than the waters of the ocean, whether we regard the extent of surface, the elevated temperature, or the agitation by winds; and, accordingly, the amount of water thus raised into the atmosphere, is prodigious, being estimated as sufficient, were none returned to it, to sink the level of the ocean four feet per annum, implying more than 3,000,000 of tons weight, to every square mile. One portion of this vapor is precipitated upon the ocean again; another portion is borne by the winds over the lands, and waters the earth with showers, feeds the springs, sustains vegetable and animal life, and then returns again to the ocean by the rivers. These restore to the sea what the land had before borrowed from it; and thus, by this constant exchange, the land is not drained and the sea is not full. The Mississippi alone delivers to the Gulf of Mexico, nearly fifteen trillions of cubic feet, or about 110 cubic miles of water, which the valley of the Mississippi alone had borrowed from the ocean. These statements give us some faint idea of the energy which Nature puts forth in watering the earth. Her beneficent care is still further manifested in the *purifying* processes which water undergoes in this circulatory system, which is carried on between the sea and the land. All the impurities that can soil the person, or clothing, or dwelling of man; all that can corrupt the air from the decay of organic substances, is received by the rivers and borne away to the sea. Here the tides and the waves meet it, and sweep it far from the shore, and deposit it in the ocean depths. In return, a constant supply of pure water is raised from the sea by distillation, leaving behind all saline and all other foreign ingredients of sea-water; it is borne over the land by winds, where it either falls in

showers of rain, or is still further purified by the process of crystallization, and descends in snow. But since in falling through the atmosphere it imbibes the impurities which may happen to be present in this medium, (a process by which the purity of the atmosphere itself is maintained,) it is again subjected to filtration through the stratum of sand that covers the surface of the earth, and being thus separated from every impurity which it had either transported to the sea or accidentally imbibed on its return, it is restored to the earth to gush forth again in pure fountains, for the use of man.

Since the rivers carry down saline matters to the sea, which they have dissolved in flowing on or under the earth, while by evaporation, in the returning system, water leaves all foreign ingredients behind, the ocean becomes permanently *salt*. It is not, however, certain that all the salt is thus supplied by the rivers. Since the different saline substances contained in river water are appropriated more or less in the marine structures that are constantly forming, as sea-shells and coral groves, it is not easy to determine whether the ocean was originally salt or has borrowed this quality entirely from the land. It amounts, at present, to about  $3\frac{1}{2}$  per cent., and is nearly uniformly distributed over the globe, a proof that the waters of the ocean commingle throughout their whole extent. The numerous *currents* which form so prominent an object of the work before us, keep its waters in continual circulation. No sooner is a portion of the equatorial seas heated, than it expands, and starts for the polar regions, and like portions of the polar waters commence their circuit to the equator. This mutual exchange goes far to prevent excesses of heat on the one side, or of cold on the other, and contributes greatly towards diffusing a uniform temperature over the globe. Until recently these currents were little known, and it is chiefly by investigating their course and the laws that govern them, that the labors of Lieut. Maury and those who aid him in collecting materials for his Wind and Current Charts, have proved so useful to navigation, and will, as we believe, become, as they are improved and perfected by future researches, a still more signal benefit. Among these currents the Gulf Stream is the most remarkable, and that which has longest received the attention of both navigators and men of science. It is a hot sea river issuing from the Gulf of Mexico, where it has a temperature of 86 degrees. In the Straits of Florida its breadth is 38 miles, but it widens as it advances northward, and attains a breadth of 75 miles off Cape Hatteras, and expands still more as it reaches the latitude of the Grand Banks, still preserving a temperature nearly



20 degrees above that of the neighboring seas. Its color (indigo blue) serves to distinguish its borders from the adjoining waters, which are of a dark green hue; but the thermometer is a still more definite guide to its exact limits, and shows that its margin is exceedingly well defined, and that its waters hardly mix at all with the cold and dense waters through which it flows. These, indeed, on either hand, are like banks to it, confining it like the banks of earth that form the margin of an ordinary river. Since the bottom of the sea, as it advances to the north, grows more and more shallow, its breadth of course expands, and thus the lower surface of the stream presents an inclined plane rising in the direction of the stream; and this is what Lient. Maury means by the apparently paradoxical expression, that "the Gulf Stream runs up hill." The amount of water kept in motion by this hot sea river is prodigious, being, as our author supposes, 3,000 times as great as all that the Mississippi pours into the Gulf of Mexico, and equal to one-fourth of the entire water of the Atlantic; and since whatever amount of fluid is withdrawn from the equatorial regions and conveyed to the polar, must be replaced by a corresponding amount in the opposite direction, he concludes that the great current which descends from Baffin's Bay is no less in amount than the Gulf Stream. This it meets near the Grand Banks, where it divides into two portions, one crossing the Gulf Stream at a considerable depth, where its course is detected by the masses of ice which it bears along in its current, and the other flowing down the coast, commonly at a great depth, but occasionally elevated by shoals almost to the surface of the ocean, as at the Banks of Newfoundland, and at Cape Hatteras. The Gulf Stream itself also divides into two parts, beyond the Banks, one portion running northward and flowing along the western side of northern Europe, contributing greatly to soften the rigors of those wintry climates, and the other taking a sweep towards the Coast of Africa, and returning again to the Gulf of Mexico to renew the same grand circuit. The Gulf Stream retaining somewhat of the superior diurnal velocity of the earth in the regions from which it flows, has an easterly tendency as it proceeds towards the higher latitudes, while the polar current, retaining somewhat of its inferior diurnal velocity, has a westerly tendency as it flows southward, clinging closely to the main land. Its presence is recognized even in the Carribean sea, where at a little depth the water is found to be as cold as at the corresponding depth off the Arctic shores of Spitzbergen.



What power can be assigned adequate to the movement of such a vast amount of water as that of the Gulf Stream? The cause usually assigned is the influence of the trade winds, which accumulate the waters of the Atlantic upon the great basin of the Gulf of Mexico. But our author considers the fact of such an elevation of the waters of this basin as is usually represented to take place, improbable, and maintains (what appears to us extremely probable) that the expansion of the waters of the equatorial seas, makes them flow off either way towards the poles, local circumstances determining them to run in particular channels, rather than in one unbroken wave; while the condensation of the cold waters of the polar seas, causes them in like manner to make their way towards the equator.

The tendency of the waters of the middle portions of the Atlantic to join the great current that issues from the Gulf of Mexico, is strongly evinced by the following fact. It is a custom often practiced by sea-faring people, to throw bottles overboard, with a paper stating the time and place at which it is done. Lient. Maury is in possession of a chart representing in this way the tracks of more than one hundred bottles. Of many thousands that have been cast into the sea, these are all that have been found and recorded. This chart indicates that the waters from every part of the Atlantic tend towards the Gulf of Mexico and its stream. Bottles cast into the sea midway between the old and new worlds, near the coasts of Europe, Africa, and America, at the extreme north and farthest south, have been found either in the West Indies, or within the well-known range of the Gulf Stream.

Besides the immense aid which these researches promise to lend to the navigators of the ocean, they will also contribute vastly to promote the discovery and acquisition of its hidden treasures. Already the tracing of warm and cold currents has opened new retreats of the sperm whale, which lives only in warm water, and brought to light new homes of the right whale, which is the tenant only of cold water, and never crosses the torrid zone. So great indeed is the importance of the whale fishery to the United States, that our author, with an excusable degree of enthusiasm, pronounces it to be a source of wealth transcending all the mines of California.

[From *The New Englander* for Nov., 1854.]

THE

## PLURALITY OF WORLDS.



*The Plurality of Worlds.* With an Introduction by President Hitchcock of Amherst College. 12mo. pp. 307. Boston: Gould & Lincoln.

*More Worlds than One.* 12mo. pp. 265. New York: Robert Carter & Brothers.

THE question whether the heavenly bodies are inhabited, is a very ancient one, dating back as far at least as the earliest of the Grecian schools of astronomy, six hundred years before the Christian era. Anaximander, a pupil of Thales, maintained the doctrine of a Plurality of Worlds; and the same opinion, under different forms, was successively held by Pythagoras, Aristotle, Eudoxus, and numerous other ancient astronomers. Among the early fathers of the Christian church, it was often discussed but generally condemned. After the revival of astronomy, the doctrine gained very general currency, being embraced by Tycho Brahe, Galileo, and Kepler. As early as 1640, Bishop Wilkins, afterwards first President of the Royal Society, wrote a book to prove that the Moon is a World, and that the Earth is a Planet. In the former of these treatises it is earnestly argued, not only that the moon is inhabited, but that, in after times, means would be devised for actually making the

journey to the moon, and carrying on commerce with the inhabitants.\*

In 1686, only a year before the publication of the *Principia* of Newton, appeared the celebrated work of Fontenelle on the *Plurality of Worlds*. Fontenelle was Secretary of the French Academy of Sciences, and was well acquainted with the state of astronomy at that period, and better than most of the popular writers of his day; still he was more of a poet than an astronomer, and such a theme as the *Plurality of Worlds*, which gave full scope for his fancy, was exactly suited to his genius, and equally adapted to the taste of the people for whom he wrote. The discussion was conducted in the form of conversations with a celebrated Marchioness, whose lively curiosity and keen discernment, embellished by superior personal charms, gave a high zest to the dialogue, and clothed the subject in a most attractive garb. The work accordingly ran through numerous editions in French, was translated into several other languages, and long afterwards continued to be honored not only with the notice, but with the comments of the learned, among whom were the celebrated Lalande and other distinguished astronomers. Indeed, it may be worthy of inquiry whether this work of Fontenelle was not the first attempt that was ever made to dress up the severer truths of science in a style suited to the readers of plays and romances. Astronomical treatises had been for the most part, up to that time, written in Latin, and wore a repulsive aspect to the unlearned reader; but it was a leading object of Fontenelle to address himself to the taste of the cultivated females of France, and to introduce to them the leading truths of astronomy in a style at once instructive and amusing.

A few years afterwards, the question of the habitability of the planets engaged the attention of one of the most profound philosophers of the age, the celebrated Huyghens. In 1690 appeared his *Cosmotheoros*, in which the author puts forth an elaborate argument in favor of the doctrine of a *Plurality of Worlds*. From the times of Fontenelle and Huyghens, astronomers have been too much occupied with the labors of observation, and with the investigation of the laws of physical astronomy, to engage much in speculations of this kind, but they have generally fallen in with the popular doctrine, that the stars are suns and centers of planetary sys-

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\* This scarce but curious work, written in a very quaint style, shows how crude was the knowledge of astronomy possessed by the English philosophers at the time of the birth of Sir Isaac Newton, even after the discoveries of Kepler and Galileo.



tems like ours, and that the heavenly bodies in general, are the abodes of life and intelligence. If, however, astronomers have regarded the discussion of this question with but little interest, theologians have carried it on with augmented zeal, especially with the view of repelling an objection brought against the doctrine of Christ's redemption, viz: that the earth occupies too small a place in the universe to have been the object of such transcendent care, and of such a painful sacrifice, as are implied in that doctrine. Dr. Chalmers' elaborate and eloquent astronomical discourses were directed mainly towards the refutation of this infidel objection; and Dr. Dick, in his *Celestial Scenery*, and his *Sidereal Heavens*, has given a full and able summary of the arguments in favor of the doctrine of a Plurality of Worlds.

Thus the matter rested until a new interest has been given to it by the recent appearance of the two works placed at the head of this Article, the first denominated "The Plurality of Worlds;" the second, "More Worlds than One." The former was published anonymously, but is ascribed to the prolific and able pen of Dr. Whewell; the latter, which is expressly intended as a reply to the other, is the production of Sir David Brewster. To the scientific and the religious world it is matter of just felicitation, that a subject of such exalted interest to both, has fallen into the hands of two combatants of such high qualifications, whether we regard them as Christians deeply imbued with reverence for the Creator, or as philosophers profoundly versed in a knowledge of his works; unlike, therefore, to the parties by whom this controversy has been sometimes conducted,—a sneering freethinker, on the one side, and a superficial pretender to science on the other. This seems, therefore, a favorable time for settling the question respecting a Plurality of Worlds; or, if it cannot be definitely settled, of determining at least the nature and weight of the evidence for and against the doctrine.

This question has at different times been discussed under various forms, and with various degrees of extension, under the following different issues: Whether the *moon* is inhabited? Whether the *planets* are inhabited? Whether the *stars* are suns and centers of planetary systems, alike the abodes of life and intelligence? Whether the heavenly bodies are peopled with rational and immortal beings like man, or whether beings of different orders, as angels or spirits, inhabit those realms? We shall contemplate the subject only in its most simple form, since evidence going to show the existence of life and intelli-



gence in situations the most favorable, would strengthen the presumption that they pervade the entire universe; or if even in such situations their existence appears improbable, it will be scarcely necessary to discuss the claims of other worlds.

There are a few *principles of reasoning* which it may be well to review, before entering directly upon the main question. Let us bear in mind, then, that probable evidence may exist in various degrees; that when, from the nature of the case, it is difficult to procure any evidence, on one side or the other, there is a tendency in the human mind to ascribe undue weight to slight analogies; that the possibility of a thing is very slight evidence of its reality; that reasoning from analogy is apt to be delusive, and is often abused; that the argument from authority, in a question like this, lying as it does without the pale of instrumental observation and mathematical reasoning, is liable to be overrated; and that men readily believe when any doctrine is supposed to be favorable to their religious faith, or settled opinions. Let us dwell for a moment on these several considerations.

First, *probable evidence may exist in various degrees.* The meteorologist will regard the presence of a peculiar kind of cloud (the *nimbus*) as affording *some* probability of rain. If, at the same time, the barometer is falling, the probability will be strengthened. It will be still greater if the wind is in the rainy quarter; and stronger yet if the dew point is very high, indicating a great amount of moisture in the atmosphere. The concurrence of all these prognostics will render the probability of rain almost a certainty to his mind, although the presence of any one of them, alone, would afford but slight grounds for expecting rain. In fact, the absence of the other signs would, if they remained wanting, overbalance any one of the number, and create a contrary presumption.

Secondly, *when, from the nature of the case, it is very difficult to obtain any evidence at all, there is a tendency in the human mind to give undue weight to the little that is known.* In no subject is this defect of reasoning more apparent than in astronomy, when we desert the only safe guides, observation and geometry; and nowhere in the annals of human knowledge, do we find more wild and extravagant speculations, than among astronomers, when they wander away from the strict line of instrumental observation and mathematical reasoning, into the region of conjecture and analogy. The very inventiveness and originality of some of the greatest astronomers, have allured them into the realms of fancy. Witness the extravagant ideas

of Kepler, and the slight analogies from which Sir William Herschel drew many of his conclusions respecting the universe, although he justly merited the encomium passed upon him by Arago, who called him "the great, inspired observer."

Thirdly, *the possibility of a thing is slight evidence of its reality.* There are a thousand things possible which are not true, or even in the smallest degree probable; and it is very little to say in favor of any doctrine that, *for aught we know*, it *may* be so. The possibility of a thing, however, is good against the plea of its impossibility. When it is maintained that a thing cannot be, it may be sufficient to show a way in which it may take place, although the mode assigned may be far from that by which it is actually brought about. The ancient philosophers held that we can never be certain that the same laws prevail among the heavenly bodies as govern terrestrial phenomena; that the laws of motion, for example, may be very different among the planets and stars from what they are on the earth; but while this doctrine prevailed no progress could ever have been made in celestial mechanics. It was, in fact, a natural fruit of the heathen mythology, which assigned different realms in the empire of nature, to different monarchs, while the doctrine first distinctively assumed and proclaimed by Newton, that the laws of nature are the same throughout the universe, is the natural result of the religion of the Bible.

Fourthly, *the argument from analogy is apt to be delusive, and is often abused.* This has been peculiarly the case in astronomical speculations. A brief consideration, therefore, of the principles which ought to govern us in the application of analogical reasoning in such questions as that of the Plurality of Worlds, will aid us to form a just estimate of the two works under review. If two things agree in all the particulars in which we can compare them, we are justified in concluding that they also agree in other respects; and if the number of particulars in which this uniform agreement holds good, is very great, then our inference that they are alike in other points where we have not the means of making the comparison, is proportionally strong. But if on comparing two things with one another, we find them agreeing in some respects and disagreeing in others, then the conclusion that they are alike in points where the comparison cannot be instituted, is strong in proportion to the number of particulars in which they agree, and weak in proportion to the number of particulars in which they disagree. To illustrate our meaning by a familiar example. Two honey-bees resemble each other in a great number

of points, while they differ in scarcely anything. One of them stings us—we infer that the other can do the same, without making the trial. But now let us compare the bee with an eagle; and to make the case bear more directly on our object, we will suppose that the bee is in our garden, and the eagle high in the sky. The bee we can inspect near at hand; the eagle only at a great distance. Both live and move in the same element; both have wings and fly; and, in short, they are alike in nearly all the particulars in which we can compare them. Yet the points of comparison are very few, and should we infer from these few points of resemblance, an identity in all other respects, we can easily see to what errors such a conclusion would lead us. But suppose the eagle, by means of the telescope, to be brought so near that we can extend the comparison to many other particulars. We now find that the points of disagreement gain upon us: the eagle is covered with feathers; he has no sting; he makes no honey; in short, the points of dissimilarity increase faster than the points of resemblance; therefore, to make inferences respecting the eagle from our knowledge of the bee, would evidently betray us into manifold errors. It will fall in our way by and by to see how far this case corresponds to the actual relations existing between the earth and any of the planets.

Fifthly, the argument from the *authority of great names* in astronomy, is to be received with some caution. Astronomers are of different classes. They are like different sorts of artists engaged in building a palace or temple, where each sort requires some peculiarity of genius and taste, while the laborers on different parts of the edifice may severally be eminent in his own particular department and yet know scarcely anything of the other departments. Some astronomers, like Tycho Brahe, have extraordinary powers for constructing instruments and making observations, while out of this particular field of labor their authority is nothing. The absurdities of the system of Tycho, and his firm belief in judicial astrology, furnish a sad, but not a solitary example, of the amount of weakness which may coexist in the same mind with the highest attributes of genius. Others, like Laplace, have little taste or talent for the labors of practical astronomy, but are great in hunting out the relations of the bodies of the solar system by means of the calculus; and exalted as are the merits of Laplace, and great as is his authority in all matters depending on this wonderful agent in the discovery of truth, yet in subjects of pure speculation, his opinions are far from being entitled to the same deference. Results,



which it required the highest efforts of genius to discover, are often of such a nature that, when placed fairly before the mind, common sense alone is required in order to apply them, and logical powers rather than inventive ingenuity are necessary for drawing the proper conclusions to which they lead. In a few instances, the astronomer, like Kepler, has combined the most profound powers of intellect with the highest gifts of imagination, and has brought forth the grandest discoveries of the philosopher, or the wildest ravings of the poet, according as one or the other of these elements happened to preponderate. On the other hand, Galileo and Newton were severally remarkable no less for the greatness of their intellectual powers, than for the harmonious proportion which constitutes the well-balanced mind; and accordingly their opinions on all subjects to which they applied their minds, are entitled to peculiar weight.

If, then, any question in astronomy is to be settled by the authority of great names, let it be seen what is the amount of the authority on that particular point, remembering that truths which it required the greatest powers and attainments, either in the field of observation or of mathematical analysis, to discover, are no sooner established, than they often fall immediately under the domain of common sense, and require nothing so much as judicious powers of weighing evidence; and such we think to be precisely the case with the question of the Plurality of Worlds.

Finally, *men readily believe any doctrine which is supposed to be favorable to their religious faith, or to their settled opinions on any other subject, and as readily reject what is subversive of such opinions.* When a writer like the author of "*More Worlds than One*," pronounces the doctrine he is defending to be "the hope of the Christian and to be embalmed in the warmth of the affections," we recognize a state of mind unfavorable to sober argumentation.

With the foregoing principles in view, we propose now to enter directly upon the question, *Are the planets inhabited?*

I. SOME FACTS ARE UNFAVORABLE TO THE DOCTRINE.

1. *The extremes of heat and cold.* At different distances from the sun, the heat diminishes as the square of the distance is increased, and consequently is seven times greater in Mercury and twenty-seven times less in Jupiter than on the Earth. If we take the mean temperature of the torrid zone at  $80^{\circ}$  (which is not far from the truth) the heat in the equatorial regions of Mercury is  $560^{\circ}$ , a heat above the melting points of several of the metals; while a place situated even in the



middle latitudes, like New York, (which has a mean temperature of  $54^{\circ}$ ;) would have a temperature of  $378^{\circ}$ ; and in the polar regions where on the Earth the heat is only  $32^{\circ}$ , it would be in Mercury  $224^{\circ}$ , or  $12^{\circ}$  above the boiling point of water. On Jupiter, the extremes of cold would be equally intense. On the equatorial regions the mean temperature, being twenty-seven times less than at corresponding places on the Earth, would be only  $3^{\circ}$  above zero, and in the middle latitudes it would be scarcely above zero. In Saturn, Uranus, and Neptune, the cold would be still more insufferable.

2. The differences in the *Sun's light*, would be equally striking, since light obeys the same law as heat at different distances from the source, and consequently when multiplied seven-fold on Mercury, it would illuminate all objects with an insupportable glare, while on the remoter planets it would fade to a dim twilight, being nine hundred times less intense on Neptune than on the Earth.

3. The variations of *weight* in bodies on the different planets would at least require very different adjustments of the muscular powers of animals from those which exist on the Earth. On the moon a body weighs only one-sixth as much as at the earth, and the position of bodies would be so unstable (being bound to the surface by so slender a tie) that slight disturbances of equilibrium would upset them. Standing upright, or walking, would be for man, with so narrow a base, a very delicate operation. On the other hand, on Jupiter a body would weigh ten and a half times as much as on the Earth, and the position of all things would be so fixed and so difficult to lift or to move, as to require the employment of forces equally extraordinary. Walking would here become excessively tiresome, if not impossible, from the mere weight of the limbs and of the body. But we have not stated the case in its most unfavorable light; the extremes of weight between the heaviest and the lightest of the planets vary as sixty to one, instead of the small range we have contemplated.

4. *Want of air and water.* In respect to the moon it is ascertained by very satisfactory evidence, that it has either no atmosphere at all, or one of so little density that human beings could not live in it; and it is fully established that there is no water in the moon. The absence of water implies the absence of a vegetable kingdom, and consequently of all animal life, which is sustained, either directly or indirectly, on vegetables. Moreover, the surface of the moon, as seen by the most powerful telescopes, presents undoubted signs of consisting almost wholly of naked volcanic lava, of an aspect more

and more dreary as the view becomes more and more distinct. The planets afford some indications of atmospheres, although none of these appear to be perfectly decisive; and cloudy appearances investing several of the planets, particularly Mars and Jupiter, are confidently adduced by some writers as evidence of the existence of water. But clouds and smoke arise from various sources, and their presence is not of course evidence of bodies of water beneath; while the cold of Saturn, especially, (where such cloudy belts are likewise seen,) forbids the supposition of liquid water. White spots about the poles of Mars were first inferred to be owing to snow, by Sir William Herschel, a man of great genius, but withal of lively imagination and much given to the discovery of analogies; but the cold of a polar winter in Mars is too severe to produce such accumulations of snow, since warm and humid currents of air are essential in order to supply the material of snow, and the heat of the polar summer is too low to dissolve it in the manner supposed by Herschel and his numerous followers.

The telescope, it must be acknowledged, has added nothing to the amount of evidence in favor of the doctrine that the planets are inhabited; it has in fact greatly diminished that amount, since the points of dissimilarity to the Earth, which it has revealed to us, have increased faster than the points of resemblance.

## II. But THERE ARE SOME THINGS FAVORABLE TO THE DOCTRINE OF A PLURALITY OF WORLDS.

1. The uniformity of *plan* observable in all the works of nature, and equally in individual bodies and in systems of bodies. The chemist is sure of this when, on analyzing a drop of water, and finding it to consist of two volumes of hydrogen and one of oxygen, he confidently asserts that this is the composition of water; meaning that water, wherever found, in every country, in earth or air, or even in the heavenly bodies, if it exists there, consists in every place and in all time of these two elements, united in precisely these proportions. The anatomist asserts a similar uniformity in the optical structure of the eye; that in every animal, however the organ may differ in size, in color, or in position, its mechanism is everywhere similarly adapted to the properties of light. This uniformity of plan is equally observable in systems of bodies. The Earth and the Moon compose a system governed by certain fixed and mathematical laws; Jupiter and his moons form a similar system, larger and more diversified, it is true, but still governed by precisely the same laws, in respect to the forces which impel them, and the forces by which they tend towards

their primaries, the spaces described by the radius vector of each, and the relation between the distance from the primary and the time of their revolution around it. Saturn, again, is but a repetition of the systems of the Earth and Jupiter, and so of all the other subordinate groups consisting of planets and satellites. But each of these systems, again, is but a miniature of the entire solar system, where we see repeated on a grander scale the same mechanism, each planet revolving about the sun by precisely the same laws as those by which the satellites revolve about their primaries. So similar is the mechanism of the lower when compared with the higher system, that a diagram of Jupiter and his moons, accurately plotted according to their distances, is sometimes mistaken by the unlearned for a representation of the solar system itself. In some of the double stars, called the binary stars, we have examples of the revolution of sun around sun; and here, too, as we might have anticipated, the same mechanism, governed by the same laws, prevails as in all the lower systems; and all analogy tends to the same conclusion, that in the structure of one of these systems nearest to us, as that of Jupiter and his satellites, we actually see the pattern according to which all the systems in the world, and the Universe itself, are framed.

Upon such a uniformity of plan we predicate a uniformity of *purpose*; and can hardly resist the inference that a series of bodies linked together by one and the same bond, (the law of gravitation,) and distributed into families placed under precisely the same regulations, were designed for the same end. Or, to confine ourselves, for the present, to our own solar system, we are almost impelled to the conclusion, that the other families of this realm were designed for the same purpose as the Earth. Nor is it necessary to build this conclusion on any view we entertain of the wisdom or benevolence of the Creator. The mere naturalist would and does declare his conviction, that Nature, when she links together her productions by mutual bonds, and subjects a certain number composing such a group to the same laws, designs them for the accomplishment of the same ends. If, then, he can clearly determine the great purpose for which one member of the series thus related to each other was designed, he infers that this also was the grand purpose for which Nature designed the others.

2. In order, then, to learn the great purpose for which the planets were made, we have only to inquire for what purpose the earth was made; and we think it very evident that *animal life* in general, and *Man* in particular, was the great object for which this world was created. To which part soever of the face of the earth we direct our attention, we find it



everywhere stored with life, and all the arrangements of the natural world adapted for the multiplication, support, and happiness of sentient beings. Some walk the earth, some creep in its caverns; others fill the sea, and others the air. And when we call to our aid the microscope, we are everywhere astonished to see how life abounds where the eye was unable to detect it, peopling every leaf of the forest, swimming in every drop of water, and sparkling in every ocean wave; to find them floating in the air, swarming in slime and mud, forming the dust of the hurricane, and, either living or dead, constituting strata of the earth itself. Nor can it be doubted that the economy of nature, by express adaptations, is regulated with reference to the multiplication and support of all these tribes of the animal creation, a dwelling place and appropriate aliment being provided for all at the very spot where each springs into life. That the geological changes which have taken place on the surface of the earth, resulting in the formation of soils; the diffusion of water so as to be accessible everywhere; the diversified and ample stores laid up and constantly replenishing in the vegetable kingdom; atmospheric air by its elastic properties penetrating every retreat where there are respiratory organs to inhale it; the sun shedding upon all his light and heat; that these all and severally are so many express adaptations for the multiplication, sustenance, and happiness of the animal creation, are facts so obvious as scarcely to require any argument. But the meaning which they express is plainly this—that life and happiness constitute the grand leading objects for which the earth was made.

On advancing one step further and contemplating the relation of man to the external world, we finally arrive at the conclusion that the earth was made for Man—that while the happiness of all the other forms of sentient being has been by no means neglected by the beneficent Creator, yet, that man is the grand ultimate object for which all things in the earth, animate and inanimate, were made. Without any view to the bearing which this sentiment might have on the subject now before us, we have in a former number of this journal endeavored to prove that the *World was made for Man*.<sup>\*</sup> We predicated this in respect both to the powers and the productions of the natural world.

First, we endeavored to prove that the great *powers* of nature, as heat, light, magnetism, and electricity, were created especially and preëminently for man. Heat, when contemplated in its four great relations, namely, to animal life, to combustion,

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<sup>\*</sup> New Englander, February, 1849.



as a mechanical force, and as an agent in effecting chemical changes, is adapted exclusively to the condition and purposes of man, with hardly any reference to the lower animals, except in the first of these relations, that of animal life; as is rendered strikingly apparent by an actual enumeration of the various uses and applications of heat. By the aid of heat man greatly excels them, also, in the variety and quality of his food; for while each animal is confined to a single kind of food, or at most to a small variety of articles, man has a choice of the whole vegetable and animal kingdoms, and is alone endowed with the power of greatly exalting their qualities and increasing their varieties by his own art and skill. The offices which heat performs for the animal tribes terminate in what relates to their mere existence and personal comfort; but with man they have but just commenced, since all those manifold and important uses which he makes of heat as relates to combustion, to mechanical force, or as the agent in effecting chemical changes in matter—uses upon which his dominion over the external world chiefly depends—all these lie beyond the pale of mere animal existence, and are exclusively the birthright of man. With respect to the other great powers of nature, namely, attraction, light, magnetism, and electricity, the relation which animals sustain to them is merely *passive*, while man, besides enjoying the same passive benefits, also employs them in a thousand ways to effect his voluntary purposes. The sun's light, for the ordinary uses of vision, is no less important to the animal tribes than to the human species; but over this element they have no control, while man kindles, manages, and regulates it at his pleasure, dispels with it the darkness of night, decomposes and modifies it into an infinite variety of colors; and it is his sole prerogative, by the aid of the microscope and the telescope, so to exalt his natural vision as to look through all creation.

Secondly, the *productions*, no less than the powers of nature, were created for man. The offices which the several kingdoms of nature perform for the animal tribes, are for the most part limited to such as regard mere existence and personal comfort. The *mineral* kingdom affords them a footstool, or a shelter, but upon man it confers its subterranean riches, its quarries, its mines, its gems. He alone can cause the earth to bring forth food, and to teem with fruits and flowers. In the *vegetable* kingdom man not only, in common with the lower animals, finds means of sustenance, but while they are severally limited to a single, or, at most, a small variety of its productions, its boundless wealth is conferred on man; some portions he uses

for his food, some for his dwellings, his temples, his ships; and some for his medicines. Its forests supply him with fuel; its sugar, wine, and oil, delight his taste; its drugs and coloring stuffs adorn his arts. The *animal* kingdom itself, moreover, was made for man. Some yield their bodies for his food; others supply his clothing; some toil for his convenience and support; some afford materials for his arts; some, oil for his lamps, and medicines for his diseases. Even fossil tribes entombed in ages past, and transformed into coal, are brought up to feed his fires.

We further endeavored to show that the world of *beauty* and *sublimity* was made exclusively for man, and that the same was true of the world of *art*; that man, in fact, is a co-worker with God in the natural, as in the moral world, many things being left in a state apparently unfinished for man to complete, or develop; as metals in the state of ores, steam hidden in water, and glass existing only in its elements, sand and alkali; that it was left for man to establish the *relations* of things, as of heat to fuel and to water, magnetism to iron, and mechanical forces to machinery; that it was reserved for him to form many useful *compounds*, as glass, unknown to nature herself; and that upon him was conferred the high prerogative of exalting the productions of nature, of correcting her deformities, developing her beauties, and multiplying her varieties. From all these considerations we drew the final conclusion, that man sustains a totally different relation to the external world, from that sustained by all the inferior animals; and he is not to be regarded as the last number of a series—the last link in the chain of development—but as a being severed from all other beings of this earth, as well in his position here as in his immortal destiny.\*

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\* In the article alluded to, the argument is summed up in the following paragraph: "However favorable it might be to our argument, we shall not insist here upon the fact that man alone of all the animal races consists of a single species; nor upon the anatomical and physiological distinctions by which he is separated from the animal tribes, such as the peculiar structure of his brain, his possession of hands, and his erect attitude; but we shall leave such points to professed writers on the Natural History of Man. Nor need we urge his possession of the gift of reason, of imagination, or of speech, although these, together with the gift of a hand, may lie at the foundation of the peculiarity of his relations to the external world. But we rest our proposition on the *facts* which have been adduced to show, that he only has dominion over the great powers of nature, to control them at his will and to employ these energies in his service; that all the riches of the several kingdoms of nature, are bestowed on him, except so much as is just sufficient to sustain the life or to provide for the continuance of the inferior animals, and that they themselves are sustained and continued chiefly for his benefit; that to any single species the part of nature which it can appropriate to itself is exceedingly

If, then, we arrive at the conclusion that the planets were made for the same purpose as the earth, and on further inquiry, find that the great object for which the earth was made, is the existence and happiness of living beings in general and of man in particular, we can hardly resist the inference that these remoter worlds are also the abodes of life and intelligence.

We shall arrive at the same result from the consideration, that in nature *all things have their use*. In the progress of knowledge this principle has gained strength. To the earlier anatomists there were parts in the human body for which no use had been discovered; but it would evidently have been rash to infer that they were therefore useless. The progress of the science has demonstrated that these very parts have important uses, and are indispensable to the well being of the whole structure; or if, in any case, the use has not as yet been determined, the anatomist modestly infers that it is useless only in appearance, and only in relation to the state of his knowledge. There was a time when electricity was regarded as an omen of the wrath of heaven, fitted only to terrify and destroy; but a further acquaintance with it has demonstrated, that it is, of all natural agents, that which performs the most interesting services for man. The truth of this principle is more striking in respect to systems of bodies than to individuals; as in the structure of the human body, every part has far higher uses in relation to the other parts, and as a member of a system in which all the parts are mutually dependent, than in its individuality. Now it cannot be denied that the sun and planets compose such a system, and the inference is that every part has important functions to perform. The solar system, in fact, bears a strong resemblance to our own happy frame of government, where municipalities are combined to form states, and states to form the federal union. But if the planets are barren wastes where there is no life to animate and no capacity for happiness, the least we can say is, that, in this

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limited in space and quantity, while the domain of man is boundless as nature herself; that Providence has profusely scattered over all creation the beautiful and sublime, for which no other reason can be assigned than the promotion of his happiness; that he only has the gift of aiding his native powers by instruments of his own invention, by means of which he gains his empire over matter, and not only subdues to his dominion all terrestrial nature, but opens to himself a commerce with other worlds, even to the remotest Nebulæ. The high prerogative of carrying out the designs of the Creator in establishing the relations of things, in forming new compounds, and in exalting the qualities of the productions of nature, appears as unlike any gifts bestowed on the inferior animals, as those which connect him with eternity, and prove that he is severed from all the other beings in this world, no less in his position here than in his immortal destiny."



respect, they present to us a totally different view of the economy of Providence from that which is everywhere set before us in this world. They would present the anomaly of cities without inhabitants, ships without sailors, warehouses without merchandise.

But however strong may be the presumption that the planets are inhabited, it would certainly afford us much satisfaction to be able to point our finger to some contrivance in any of them, undoubtedly designed for sentient beings. A few arrangements of this kind are thought to have been detected among the planets. Thus Jupiter's moons are so adjusted to each other and to their primary, that, although from circumstances arising from the great size of the planet and the slight inclination of their orbits, they are exceedingly liable to be eclipsed,—they cannot all be eclipsed at once, and leave the planet in darkness. Nor is this a casual arrangement, for it depends upon a strictly mathematical adjustment.\* Now things are so related to each other in the economy of nature, that the presence of one often implies that of the other. Such is the relation of the eye to light and of the lungs to air; and thus a specific arrangement on the planet Jupiter to husband the light and turn it upon the planet, implies the presence of eyes to behold it. We could hardly be more certain of the presence of water when we see at a great distance a ship under sail. Laplace, however, has denied that even the moon was made to give light by night, and he would of course deny the validity of our argument respecting the moons of Jupiter. His language is as follows: "Some partisans of final causes (says he) have imagined that the moon was given to the earth to afford it light in the absence of the sun. But in this case nature would not have attained the end proposed, since we are often deprived at the same time of the light of both of them. To have accomplished this end, it would have been sufficient to have placed the moon at first in opposition to the sun, and in the plane of the elliptic, at a distance from the earth equal to one hundredth part the distance of the earth from the sun, and to have given to the earth and moon velocities parallel and proportional to their distances from the sun. In this case the moon being constantly in opposition to the sun, would have described round it an ellipse similar to that of the earth, these two bodies would thus have succeeded each other above the horizon, and as at this distance the moon would not be eclipsed, its light would always replace

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\*That the mean longitude of the first satellite increased by twice that of the third, and diminished by three times that of the second, always gives a remainder equal to  $180^{\circ}$ .



that of the sun." The reason for placing the moon one hundredth part the distance of the earth from the sun, (or at about a million of miles off,) is, that it may thus go clear of the earth's shadow, the length of which is somewhat short of that. But being then four times as far off as at present, her apparent diameter would be diminished to one-fourth, and its surface and light to one-sixteenth their present amount. As the average light of the moon is now half what it would be if constantly full, it is of course eight times what it would be if its position were changed in the manner supposed. It would be still worse for the tides; for the power of the moon to raise the tides being inversely as the *cube* of the distance, the tides would be sixty-four times less than at present; and since the average height of the tides for the whole earth is about twenty-six inches, its height would be reduced to about three-eighths of an inch, a quantity too small to answer any of the valuable purposes which the tides fulfill. A still greater injury would be sustained by navigation in losing the lunar method of finding the longitude; for since the moon would cease to change its apparent position among the stars, the lunar method would no longer be available, and the great practical object of the *Mecanique Celeste* of Laplace—to perfect the method of finding the longitude at sea—would be lost. We shall still believe, therefore, that the moon was made to rule the night, and shall think it probable at least that the moons of Jupiter were designed for the same purpose; and if so, that there are eyes there to behold the light.

The same course of reasoning which has led us to think there is a high probability that the planets are the abodes of life and intelligence, conducts us, when followed out, to the still higher and grander conclusion, that *the stars are centres of other solar systems which, like ours, are filled with sentient beings*. That the light of the stars proceeds from self-luminous bodies, is proved both by its intensity and by direct experiments in polarization; that these bodies are composed of matter like that of which our sun is composed, is shown by its obeying the same law of gravitation, as is especially proved of the *binary* stars, which revolve about a common centre of gravity in conformity with that law; that many of them at least are bodies of immense size, is evinced by the amount of light which, at such a vast distance, they send to the earth;\* and, in short, all things go to prove that the stars are suns, of

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\*Sirius is asserted on the highest authority to emit as much light as sixty-three suns like ours.

different sizes, some perhaps less, but many much larger than our sun, and that they are bodies entirely analogous to that which supplies light, heat, and attraction to our system of planetary worlds. But we have already remarked that, in the economy of nature, uniformity of plan implies identity of purpose; and if on this principle we have inferred that the planets are inhabited, and have further traced the same uniformity of plan to the stellar worlds, we seem bound to predicate of them a similar purpose, namely, that they also are the abodes of life and happiness. For us, indeed, who dwell on "this speck of earth," this seems a bold and adventurous flight; but it is no more so than if the animalcule that feeds on a leaf of a tree were suddenly endowed with reason, and should conclude that all the leaves of the tree were peopled like his own; and then enlarging his sphere of thought, should entertain the grand idea, that the same was true of all the leaves of the forest.

But before we can rest with confidence in conclusions so accordant with reason, and so consonant to our ideas of the greatness and wisdom of Jehovah, it behooves us in candor to return and see whether those considerations, which at first appeared unfavorable to the doctrine of a Plurality of Worlds, will admit of any explanations which will place them in a less unfavorable light.

We must not deny that heat and cold, light and attraction, water and air, have respectively the same properties in other worlds as here; but it is safe to say that the relations which subsist between them and the animal creation, are susceptible of very great modifications, and that objections arising from the heat of Mercury, or the cold of Jupiter, the want of light in the remotest planets, and of air and water in some or all, overlook this fact. Here our bodies are built with reference to the external circumstances of our planet; with muscles proportioned to the force of attraction which binds us to the earth, with eyes adapted to the medium intensity of our light, with organs of hearing and respiration adapted to its atmosphere, and with a digestive system suited to the vegetables, and animals designed for our food. But there is nothing improbable in the idea, no resort to mere possibilities, that the same Almighty Power which has so adapted to the external world the living beings on this planet, may institute corresponding adjustments in respect to the beings of other worlds. One bred on the shores of a lake where there were no fishes or other aquatic animals, and who had seen a man die by drowning, would infer that no animals could live in water; he could never conceive of those adaptations by which this ele-

ment is made the dwelling place of numerous tribes of animals. Indeed, there was a time in the history of the human race, when the inhabitants of the temperate zone believed that life could not exist in either the equatorial or the polar regions. They did not know what resources Providence has for mitigating the extremes of heat and cold, which without such modifications, would render those regions uninhabitable. They did not know how a temperate climate may be reached, even under the equator, by ascending to a certain elevation; nor how the trade winds, and the land and the sea-breezes, temper the heat of the air; nor how the warm currents flow off towards the poles, and are replaced by corresponding cool currents beneath; nor how evaporation is working with increased activity to convert the fierce heat of the sun into latent heat, and thus prevent its accumulation; nor could they ever have imagined that this torrid region, which they had consigned to perpetual sterility and desolation, was all the while teeming with animal life, and sustaining a vegetable kingdom a thousand fold more rich and diversified than their own. In the frozen regions of the poles, the conservative powers of nature are displayed in a manner equally admirable to preserve animal life from destruction. We recognize them in the heat given out by the congelation of water—in the special provision by which the polar seas are bridged with ice, while all the mass of waters below are preserved at a temperature much above the freezing point, and well suited to the habits of the numberless aquatic tribes that frequent those seas—in the accumulations of snow that cover the earth in winter with a thick, warm mantle—in the change wrought in the covering of beasts and birds, greatly augmenting its power of resisting the cold—and finally, in the dormant state into which certain polar animals pass during the season when they are unable to procure their accustomed food.

These examples afford us a glimpse of the power of the Creator, to establish new relations between his creatures and the external world. Nor can we limit his power to adjust material bodies to the medium in which they live, however unlike those circumstances may be to those which here constitute the relations of man and animals to the world without. We see, in fact, examples of this adjustment of the animal system, in the changes which it assumes as external circumstances change. By habitual exposure to a high degree of heat, persons have acquired the power of remaining for some time in ovens hot enough to bake bread; and eyes accustomed to night service, and kept secluded from the light of day, acquire extraordinary powers of vision in feeble lights. We



have examples also of vegetables which live and thrive for a long time without water. Nor are there wanting instances where express organizations are adapted to peculiar conditions of the external medium, as the eyes of cats designed for seeing in the dark, of moles fitted for burrowing in the ground, and of fishes for living in water. In like manner, the feeble light on the planet Saturn, might be compensated and rendered sufficiently intense for all the purposes of vision, either by giving to the eyes of the inhabitants a greater delicacy, or by enlarging their pupils so as to admit a greater amount of light. Although the exact adaptation of the senses and the mechanical powers of man to the external world is such, as generally to render extraordinary adaptations unnecessary, yet we see how easily these are made when new conditions in external circumstances render it necessary for safety or convenience; but even this limited range of adaptations and peculiarities of organization, is sufficient to show the resources of Providence, (or of Nature, if any one prefers that term,) and warrants the belief that life, both animal and vegetable, may exist on the planets, although the circumstances should require a far greater change of constitution than we can observe, both in organic and inorganic nature.

We have thus endeavored to present the evidence for and against the doctrine of a Plurality of Worlds, so far as the evidence or reasoning is purely scientific, without blending it with those moral and religious considerations which have entered of late so much into the discussion. To these we will now more particularly advert.

It was perhaps the infidel Thomas Paine who first attempted to turn the doctrine of a Plurality of Worlds to the account of infidelity. While, by the believer, the doctrine had been hailed as one that greatly exalts our conceptions of the power, wisdom, and glory of the Creator, Paine employed it to swell the torrent of ridicule and contempt which he sought to pour on the doctrine of redemption. The passage in his book (*The Age of Reason*) relating to this subject, is very characteristic of the author, and so profane and contemptuous that we cannot with propriety quote it entire. He avers that the belief that God created a Plurality of Worlds, at least as numerous as the stars, renders the Christian system of faith at once little and ridiculous, and scatters it in the mind like feathers in the air; and since he gives full credit to the doctrine that the heavenly bodies are inhabited, he of course rejects and contemns the Christian system of faith. Taking the doctrine for true, he profanely denounces the work of redemption as a soli-



tary and strange conceit. An able reply to the weak but subtle arguments of Paine was given by Andrew Fuller, near the beginning of the present century, in his work entitled "The Gospel its Own Witness." He remarks that no man can properly be said to believe the doctrine: it is not a matter of faith but of opinion; by which we understand him to mean, that Christians who accept the doctrine at all, do not rest in it, as they do in their belief in the doctrine of redemption, as absolutely true, and therefore as entitled to a place in their system of faith, but only hold it as a probable fact, and as one that does not affect the foundations of their faith, even should the fact be otherwise. But he adds, that he does not wish to avail himself of these considerations, but admitting that the intelligent creation is as extensive as modern philosophy supposes, the credibility of redemption is not thereby weakened, but, on the contrary, in many respects, is strengthened and aggrandized; that if our world be only a small province of God's vast empire, there is reason to hope that it is the only part, (except among the fallen angels,) where sin has entered; that there is nothing inconsistent with reason in supposing that some one part of the empire should be selected as a theatre on which the Creator would perform his most glorious works; that if any one part of God's creation, rather than another, possessed a superior fitness to become a theatre on which he might display his glory, it should seem to be that part where the greatest efforts had been made to dishonor him; and that the events brought to pass in this world, little and significant as it may be, are sufficient to fill all and every part of God's dominions with everlasting and increasing joy.

The foregoing considerations are deemed sufficient by Mr. Fuller to meet the objections urged by the infidel; but he proceeds to argue, that the Christian doctrine of redemption is strengthened and aggrandized by the supposed magnitude of the creation. In support of this position, he alleges that the Bible represents the work of redemption as an act of astonishing condescension, and the more so the greater the number of inhabited worlds; that, according to the scriptural account, before the creation was begun, our world was marked out by eternal wisdom as the theatre of its joyful operations; that, in the Bible, the mediation of Christ is represented as bringing the whole creation into union with the Church or people of God; that not only is the whole creation represented as augmenting the blessedness of the Church, but the Church as augmenting the blessedness of the whole creation; that at a future period we are led by the Scriptures to expect that the earth itself, as well as

its redeemed inhabitants, shall be purified and reunited to the holy empire of God ; and that the Bible represents the punishment of the finally impenitent as appointed for an example to the rest of creation. These various passages of the Bible (which are strikingly arrayed in form by the writer, but which we have not space to insert) go to establish two important truths ; first, that this earth has in fact been highly exalted by the Creator, in comparison with other worlds, by making it the theatre of the work of redemption ; and, secondly, that this event has not been an insulated one in the Universe, but has been the theme of joy and rejoicing in other worlds. While, therefore, it is not claimed that the Bible expressly assures us of the existence of inhabitants in the planetary or stellar worlds, yet it is evident that none of its representations are inconsistent with the doctrine of a Plurality of Worlds.

We have thought the arguments of Fuller the more deserving of notice, because they appear to us to contain the germs of many views which are generally supposed to have been originated by later writers, and especially by Dr. Chalmers. If they go but little way towards establishing the doctrine in question, they at least afford a happy reply to the infidel objection, that the representations of the sacred volume are not in accordance with the discoveries of modern astronomy. The difficulty urged by Paine and other infidels has, however, been found to embarrass and perplex not a few believers ; and it is the special object of the work before us, on the Plurality of Worlds, to relieve minds of this description, rather than to answer the sneers of atheists and deists. To this remarkable volume we will now more particularly give our attention.

The work begins with a concise outline of the discoveries of modern astronomy, embracing the leading facts respecting the extent of the Universe, which he supposes must have been before the mind of Dr. Chalmers when he wrote his *Astronomical Discourses* ; and the objection which it was the leading object of the Doctor to meet, was that which we have already seen was first proposed in Paine's *Age of Reason*, and answered in Fuller's *Gospel its Own Witness*. But he thus announces his purpose :

“ Perhaps, however, we shall make our reasonings and speculations apply to a wider class of readers, if we consider the view now spoken of, not as an objection urged by an opponent of religion, but rather as a difficulty felt by a friend of religion. It is, I conceive, certain that many of those who are not at all disposed to argue against religion, but who, on the contrary, feel that their whole internal comfort and repose are bound up indissolubly with their religious convictions, are still troubled and dismayed at the doctrines of the vastness of the Universe, and the multitude of worlds which they suppose to be taught and proved by astronomy. \* \* \* \* They would willingly think of God

as near to them; but during the progress of this enumeration He appears at every step removed further and further from them. To discover that the Earth is so large, the number of its inhabitants so great, its form so different from what man at first imagines it, may perhaps have startled them; but in this view there is nothing which a pious mind does not readily surmount. But if Venus and Mars also have their inhabitants; if Saturn and Jupiter, globes so much larger than the Earth, have a proportional amount of population, may not man be neglected or overlooked?"

Our author next comments upon Chalmers' celebrated "Answer from the Microscope." Chalmers had remarked that just about the time when science gave rise to the suggestion of this difficulty, she also gave occasion to a remarkable reply to it. Just about the time that the invention of the *telescope* showed that there are innumerable worlds, which might have inhabitants requiring the Creator's care as much as the tribes of this earth do, the invention of the *Microscope* showed that there were in this world innumerable tribes of animals, which had been all along enjoying the benefits of the Creator's care, as much as those kinds with which man had been familiar from the beginning. The telescope suggested that there might be dwellers in Jupiter or in Saturn, of giant size and unknown structure, who must share with us the preserving care of God. The microscope showed that there had been, close to us, inhabiting minute crevices and crannies, peopling the leaves of plants, and the bodies of other animals, animalcules of a minuteness hitherto unguessed, and of a structure hitherto unknown, who had always been sharers with us in God's preserving care. The telescope brought into view worlds as numerous as drops of water which make up the ocean; the microscope brought into view a world in almost every drop of water.

Although this course of reasoning, our author admits, is conclusive in showing that the care and Providence of the Creator do in fact extend to regions much beyond what appears to ordinary observation, and that His eye is never weary, but regards the minutest portions of His works with the same beneficent care as though they constituted the only beings in the Universe, and consequently that the vast concerns of His empire furnish no reason why He should not bestow on "these His lowest works" all the care and concern which the Bible represents, yet our author objects that the argument does not cover the whole ground. The revelations of the microscope may and do remove the objection of the infidel, and they strengthen the probability that the planets as well as the earth are filled with living beings. But do they prove that there are in those worlds beings like man, intellectual and moral beings—creatures living under a moral law, responsible for transgression, the subjects of a Providential government? The nature



of mind is conceived to be, in all its manifestations, so much the same, that we can conceive minds to be multiplied indefinitely without fear of confusion, interference, or exhaustion. There may be in Jupiter creatures endowed with an intellect which enables them to discover and demonstrate the relations of space; and if so, they cannot have discovered and demonstrated anything of that kind as true, which is not true for us also; their Geometry must coincide with ours so far as it goes. The Earth, with its properties, is no more the special basis of geometry than is Jupiter or Saturn, or, so far as we can judge, Sirius or Arcturus. Wherever pure intellect is, we are compelled to conceive that, when employed on the same objects, its results and conclusions are the same. But although the human mind has had the same *powers* and *faculties* from the beginning of the existence of the race up to the present time, the results of the *exercise* of these powers and faculties have been very different in different ages, having gradually grown up from small beginnings to a vast and complex body of knowledge concerning the scheme and relations of the Universe. Should we, therefore, admit that there are in Jupiter minds possessing the same intellectual faculties as ours, it does not follow that they have made the same *progress* as ourselves.

“The condition of man on earth as a condition of intellectual progress, implies such a special guidance and government exercised over the race by the Author of his being as produces progress; and we have not, so far as we yet perceive, any reason for supposing that He exercises a like guidance and government over any of the other bodies with which the researches of astronomers have made us acquainted. The earth and its inhabitants are under the care of God in a special manner; and we are utterly destitute of any reason for believing that other planets and other systems are under the care of God in the same manner. If we regarded merely the existence of unprogressive races of animals upon our globe, we might easily suppose that other globes also are similarly tenanted; and we might infer that the Creator and Upholder of animal life was active on those globes, in the same manner as upon ours. But when we come to a progressive creature, whose condition implies a beginning, and therefore suggests an end, we form a peculiar judgment with respect to God’s care of that creature, which we have not as yet seen the slightest grounds to extend to other possible fields of existence. So far as we can judge, God is mindful of man, and has launched and guided his course in a certain path which makes his lot and state different from that of all other creatures.”

We think the ideas of the author in this part of the book are obscure and difficult of comprehension; but so far as we can understand them, they do not appear conclusive. The same reasons which would lead us to believe in the existence of intelligent beings like man in other worlds, would lead us to think that they are like man in this respect, also,—in his progressive character. Does not the same analogy that guides us to the conclusion that God has created intelligent inhabitants



for other worlds, guide us onward still further to the conclusion that they are like us, progressive races? Are we totally without evidence that God has the same regard for the people of other planets as for this? Granting the existence of such inhabitants, possessed of intellectual powers and capacities, which is the more violent presumption, to suppose that God, after having bestowed upon them high intellectual capacities, has consigned all of them to perpetual barbarism; (being distinguished from the brutes that perish merely by the possession of powers which they never use;) or to suppose that the leaven of knowledge, operating at first on a portion, finally pervades the entire mass,—for such, we think, is the destiny of the human race. It appears to us no more of a gratuitous assumption to infer that beings in the planets, analogous to man, resemble man in making progress in knowledge and wisdom, than to infer that beings analogous to man exist there at all.

Our author says that when we attempt to regard the inhabitants of other worlds as living like us under a moral government, we are driven to suppose them to be, in all essential respects, human beings like ourselves. We do not see this to be at all necessary. The Scriptures inform us of other orders of intelligencies entirely different from man; and that the planetary inhabitants, if such exist, have a totally different relation to the external world from that which we sustain, we endeavored to show in the former part of this article; and, possibly they are as unlike to man as man to angels. We ought to keep in mind the remark which Chalmers makes to the infidel objector,—a remark which our author approves,—that “since we know nothing about the inhabitants of Jupiter, true science requires that we say and suppose nothing about them.” If man had never fallen from the high estate in which he was created, he would have been less a creature of “progress” than he now is.

These partial intimations of the peculiar views of the author, prepare the way for entering more directly on his argument. The first argument is derived from *Geology*; and, by way of introduction, we are presented with a sketch of the latest results of this science,—an outline which, independently of its bearing on the question under discussion, we would recommend to such of our readers as desire to learn, in the smallest possible compass, the present state of geological science, as particularly deserving of their study and attention. This chapter has also the merit of conciseness, which we do not think is a prominent excellence of our author's style. The “argument from Geology” commences in the sixth chapter with the following summary of the previous outline of the science.

"I have endeavored to explain that, according to the discoveries of geologists, the masses of which the surface of the earth is composed, exhibit indisputable evidence that, at different successive periods, the land and the waters which occupy it, have been inhabited by successive races of plants and animals; which, when taken in large groups, according to the ascending or descending order of the strata, consist of species different from those above and below them. Many of these groups of species are of forms so different from any living things which now exist, as to give to the life of those ancient periods an aspect strangely diverse from that which life now displays, and to transfer us in thought to a creation more remote in its predominant forms from that among which we live. I have shown also, that the life and successive generations of these groups of species, and the events by which the rocks which contain these remains have been brought into their present situation and condition, must have occupied immense intervals of time;—intervals so large that they deserve to be compared, in their numerical expression, with the intervals of space which separate the planets and stars from each other. It has been shown, also, that the best geologists and natural historians have not been able to devise any hypothesis to account for the successive introduction of these new species into the earth's population, *except the exercise of a series of acts of creation*, by which they have been brought into being; either in groups at once, or in a perpetual succession of one or a few species, which the course of long intervals of time might accumulate into groups of species. It is true that some speculators have held that by the agency of natural causes, such as operate upon organic forms, one species might be transmuted into another; external conditions of climate, food, and the like, being supposed to conspire with internal impulses and tendencies, so as to produce this effect. This supposition is, however, on a more exact examination of the laws of animal life, found to be destitute of proof; and the doctrine of the successive creation of species remains firmly established among geologists. That the *extinction* of species and of groups of species, may be accounted for by natural causes, is a proposition much more plausible and to a certain extent probable; for we have good reason to believe that, even within the time of human history, some few species have ceased to exist upon the earth. But whether the extinction of such vast groups of species as the ancient strata present to our notice, can be accounted for in this way, at least without assuming the occurrence of great catastrophes, which must for a time have destroyed all forms of life in the district in which they occurred, appears to be more doubtful. The decision of these questions, however, is not essential to our purpose. What is important is, that immense numbers of tribes of animals have tenanted the earth for countless ages, before the present state of things began to be. The present state of things is that to which the existence and the history of MAN belong; and the remark which I now have to make is, that the existence and the history of Man are facts of an entirely different order from any which existed in any of the previous states of the earth; and that this history has occupied a series of years which, compared with geological periods, may be regarded as very brief and limited."

The author next offers various illustrations to show that man is a being totally severed in his nature, attributes, and destiny from all the other beings on the earth, pursuing a course of reasoning somewhat similar to that which we formerly adopted to prove that the "World was made for Man;" yet it is a fact that Man is of comparatively recent origin, having occupied but a moment on the earth, compared with the long ages occupied by the races of animals that preceded him. The author proceeds:

'Here, then, we are brought to the view which, it would seem, offers a complete reply to the difficulty, which astronomical discoveries appeared to place

in the way of religion:—the difficulty of the opinion that man, occupying this speck of earth, which is but an atom in the Universe, surrounded by millions of other globes, larger and to appearance nobler than that which he inhabits, should be the object of the peculiar care and guardianship, of the favor and government, of the Creator of all, in the way which religion teaches us he is. For we find that man (the human race from its first origin till now) has occupied but an atom of time, as he has occupied but an atom of space; that as he is surrounded by myriads of globes which may like this be the habitations of living things, so he has been preceded on this earth by myriads of generations of living things, not possibly or probably only, but certainly; and yet that, by comparing his history with theirs, he certainly has been fitted to be the object of the care and guardianship, of the favor and government, of the Master and Governor of All, in a manner entirely different from anything which it is possible to believe with regard to the countless generations of brute creatures which had gone before him."

Our author feels disposed to give this new argument of his an ascendancy over the conclusions of astronomy itself. Geology (he says) fills the regions of duration with events, as astronomy fills the regions of the Universe with objects. It carries us backwards by the relation of cause and effect, as astronomy carries us upwards by the relations of geometry. Geology, then, has no need to regard astronomy as her superior; and least of all, when they apply themselves together to speculations like these, but in truth Geology has been an immeasurable superiority. Let the difficulty be put in any way the objector pleases. Is it that it is unworthy of the greatness and majesty of God to bestow such peculiar care on so small a part of His creation? But we know from geology that he *has* bestowed upon this small part of his creation, mankind, this special care; He has made their period, though only a moment in the ages of animal life, the only period of intelligence, morality, religion. Or is the objection this; that if we suppose the earth only, to be occupied by inhabitants, all the other globes of the Universe are *wasted*? But here again we have the like waste in the occupation of the earth. All its previous ages, its seas, and its continents, have been wasted upon mere brute life; often for myriads of years upon the lowest, the least conscious forms of life; upon shell-fish, corals, sponges. Why then should not the seas and continents of other planets be occupied at present with a life no higher than this, or with no life at all? Or will the objection be made in this way; that such a peculiar *dignity* and *importance* given to the earth is contrary to the analogy of creation? Were we to follow such analogies, we should be led to suppose that all the successive periods of the earth's history were occupied with life of the same order; that as the earth, in its present condition, is the seat of an intelligent population, so must it have been in all former conditions.



This argument from geology, which we believe to be entirely original with our author, is evidently a great favorite with him, but like other favorites, it is too much caressed. Whatever periods are assigned by geology to the successive steps in the creation of the world, described in the first chapter of Genesis, all, we suppose, will allow that the work was *finished* by the creation of man. But, in the beginning, God made the *heaven* and the *earth*; and God made *two great lights*, the greater light to rule the day, and the lesser light to rule, the night. He made the *stars* also. This description is unfavorable to the idea that, as is held by the Nebular Hypothesis, worlds are still in the process of formation. It moreover gives a plain intimation that the heavenly bodies were *finished* at the same time that the earth was; and were presented along with the earth to the view of the great Creator, when, at the *close* of his work, God saw *every thing* that he had made, and behold it was very good. We think that the author coincides with us in the opinion that all things on the earth were made with ultimate reference to man; that however long the process of creation might have been, (and one day is with the Lord as a thousand years, and a thousand years as one day,) still it was for the human race that the foundations of the earth were laid—its bosom was stored with mineral treasures—its rocks were decomposed and blended with animal remains to form soils—its surface was covered with a vegetable kingdom, and finally stored with innumerable tribes of animals,—some to supply his food, some to perform his labor, and some to furnish his clothing; but all to be subservient, either directly or indirectly, to his use. The true response, therefore, which geology makes to the present question, seems to us to be this: that all her operations having been conducted with ultimate reference to man, the probability is, that the other planets, and heavenly bodies, finished at the same time with the earth, were finished for the same exalted purpose,—that of becoming the abode of races totally severed from all the beings previously existing on those bodies, being made after the image of God. Had the processes of geology been carried on without reference to such an ultimate purpose, they would apparently have been a “waste” of power inconsistent with the known frugality of nature in the use of means; but having had throughout a constant reference to this high end,—the existence and happiness of the human race,—no step in the progress of creation was lost. In like manner, if the heavens were made with reference to a similar grand purpose, the process of their formation, however slow, implies, as in the case of geology, no waste of power.



Length of *time* employed in forming the earth would indeed be a waste, if no useful end were attained: amplitude of *space* would be a similar waste of power, if without a corresponding result.

We have not room to follow our author with the same minuteness through the remaining portions of his work. Beginning with the "outskirts of creation" (the nebulæ) and descending to the fixed stars, and thence to the planets, he has enlarged upon the circumstances unfavorable to the doctrine that they are inhabited; but his reasonings apply to beings *constituted as we are*. He does not, we think, allow sufficient latitude for the differences of adaptation of orders of beings to the external world in which they are severally placed, the probability of which adaptations we have discussed in the former part of this Article. As to the nebulæ, we can infer little respecting their peculiar constitution, or of the probability that the stars, of which some of them at least are composed, are the centers of planetary worlds inhabited by living and intelligent beings, simply because we know so little about them. Analogies for or against the doctrine are weak, because the points of comparison are few. So long as it is a matter of doubt whether the heavenly bodies nearest to us are inhabited or not, it seems inexpedient to extend the discussion to the nebulæ until this point is settled. In our judgment, the author has greatly undervalued the evidence from which it is inferred that the fixed stars are suns, and presented in a form much below its real weight the analogy which suggests that they are centers of planetary systems.

In chapter tenth, under the head of "Theory of the Solar System," we have a summary of the peculiar views of the author respecting the constitution of the Solar System, and the exalted place which the earth occupies in it. We will let the author speak for himself.

"The planets exterior to Mars, especially Jupiter and Saturn, as the best known of them, appear to be spheres of water, and of aqueous vapor combined it may be with atmospherie air, in which their cloudy belts float over their deep oceans. Mars seems to have some portion at least of aqueous atmosphere; the earth, we know, has a considerable atmosphere of air, and of vapor; but the moon, so near to her mistress, has none. On Venus and Mereury, we see nothing of a gaseous or aqueous atmosphere; and they and Mars do not differ much in their density from the Earth. Now does not this look as if the water and vapor which belong to the solar system, were driven off into the outer regions of its vast circuit; while the solid masses which are nearest to the focus of heat, are all approximately of the same nature? And if this be so, what is the peculiar physical condition which we are led to ascribe to the Earth? Plainly this: that she is situated just in that region of the system, where the existence of matter both in a solid, a fluid, and a gaseous condition, is possible. Outside of the Earth's orbit, or at least outside of Mars and the

small Planetoids, there is in the planets apparently no solid matter; or rather if there be, there is a vast preponderance of watery and vaporous matter. Inside the Earth's orbit, we see in the planets no traces of water, or vapor, or gas, but solid matter about the density of terrestrial matter. The Earth alone is placed at the border, where the conditions of life are combined—ground to stand upon—air to breathe—water to nourish vegetables and thus animals—and solid matter to supply the materials for the more solid parts; and with this a due supply of light and heat, a due energy of the force of weight. All these conditions are, in our conception, requisite for life; that all these conditions meet elsewhere than in the Earth's orbit, we see strong reasons to disbelieve. The Earth, then, it would seem, is the abode of life, not because all the globes which revolve round the sun may be assumed to be the abodes of life, but because the Earth is fitted to be so, by a curious and complex combination of properties and relations, which do not at all apply to the others. That the Earth is inhabited is not a reason for believing that the other planets are so, but for believing that they are not. Can we see any physical reason for the fact which appears to us so probable, that all the water and vapor of the system is gathered in its outward parts? It would seem that we can. Water and aqueous vapor are driven from the Sun to the outer parts of the Solar System, or allowed to be permanent there only, as they are driven off and retained at a distance by any other source of heat;—to use a homely illustration, as they are driven from wet objects placed near the kitchen fire; as they are driven from the hot sands of Egypt into the upper air; as they are driven from the tropics to the poles. In this latter case, and generally in all cases in which vapor is thus driven from a hotter region, when it comes into a colder it may be again condensed into water and fall in rain. So the cold, in the air of the temperate zone, condenses the aqueous vapors which flow from the tropics, and so we have our clouds and our showers. And as there is this rainy region indistinctly defined between the torrid and the frigid zones on the earth, so there is a region of clouds and rain, of air and water, much more precisely defined, in the Solar System, between the central torrid zone and the external frigid zone which surrounds the sun at a greater distance."

If the author complains of the advocates of the doctrine of a Plurality of Worlds for straining analogies beyond their proper bounds, what shall we say of the analogy between the vapor driven from "wet objects placed near the kitchen fire," and vapor driven off from the neighborhood of the sun, to the regions of Neptune, 2,800,000,000 miles from the sun? How will the quantity be supplied, requisite for forming such vast globes as Mars, Jupiter, Saturn, Uranus, and Neptune? How does the idea of this mode of formation by matter "driven off" from the sun, correspond to the mode prescribed by the Nebular Hypothesis, which the author seems to favor? And since these places, remote from the sun, are truly represented by the author as regions of extreme cold, how will vapor, driven thither from the vicinity of the sun, fall in rain and showers, or why not fall before it gets beyond the frozen regions of Mars? In the ingenious speculations of Mr. G. P. Bond upon the nature of Saturn's Ring, he has rendered it probable that this is composed of liquid matter, (the cold of Saturn is unfavorable to the supposition that the liquid is water;) and Professor Pierce has urged additional reasons, derived from

analytical considerations, which tend to confirm that conclusion; but independently of these investigations, we are unacquainted with any decisive proofs of the existence of liquids in the planets exterior to the earth, much less of water, or even watery vapor. If the belts of Jupiter and of Saturn consist of clouds of vapor, there is little reason to think that it is watery vapor.

In proof that the earth is placed in that region of the Solar System in which the "planet-forming powers" are most vigorous and potent, our author adduces the phenomena of meteoric stones and shooting stars; and he recognizes the probability that these are formed from the extreme portions of the Zodiacal Light, and that they are of cosmical and not of terrestrial origin. In these general views, so far as respects shooting stars and the Zodiacal Light, (which views were first proposed by the Professor of Astronomy in Yale College,) we are ready to concur; but he has fallen into a very common error in supposing that meteoric stones and shooting stars are similar bodies. Had the myriads of shooting stars, which fell during the great meteoric shower of 1833, been dense bodies like aërolites, we should have had most appalling proof of the fact in their destructive violence. There was no evidence that a single individual of these meteors reached the earth; a high probability was established that none came nearer than within thirty miles of the earth.

Here closes the argument against the doctrine of the Plurality of Worlds, derived from physical considerations. The three concluding chapters are devoted to an explanation of the author's views of the "Argument from Design" and the "Unity of the World," with some valuable reflections on "The Future."

When we took up our pen, our purpose was to offer a brief statement of the arguments for and against the doctrine in question, and then to give an account of both the interesting works placed at the head of this Article; but we have been detained so long over the pages of the former of these essays, as to leave hardly any space for the latter. It may readily be inferred, however, by those who read the latter work, "*More Worlds than One*," (and we hope all who read the one will read the other,) that the objections urged against the reasoning and conclusions of the essay on the "*Plurality of Worlds*," are similar to some we have urged in the former part of this Article. With all the views of the respected author of "*More Worlds than One*," we do not, however, profess to concur.

From the high character which the authors of these works justly sustain as men of profound science and accurate scholarship, we have felt some surprise at the number of erroneous



statements in point of fact, or principle, met with in each essay. We can ascribe them to nothing else than a careless reliance on the memory, where a long interval has passed since the facts were first committed to it, or possibly to a slight failure of this faculty—the first admonition of advancing years. Thus the author of the “Plurality of Worlds” tells us, that Mars is nearly *as large* as the earth; that some of the inhabitants of the earth came to know, *four thousand years ago*, that in a right angled triangle, the square on the largest side is equal to the sum of the squares on the other two sides; that the rising moon is seen from the observatory in Ireland with the same increase of size and light, as if her solid globe, two thousand miles in diameter, retaining all its illumination, really *rested on the summit of the Alps*, to be gazed at by the naked eye; that a railroad carriage, at its ordinary rate of traveling, would reach the moon *in a month*; and that the ordinary rocks at the Earth’s surface, have a *density* of *three* or *four*. The scientific reader will perceive that all these statements are more or less erroneous. Mars is a little more than *one-half* the size of the earth in diameter, and about *one-seventh* in bulk. The discovery of the equality between the square described on the hypotenuse and the sum of the squares of the two sides of a right angled triangle, is commonly ascribed to Pythagoras, who lived less than *twenty-four hundred years ago*. The moon when magnified a thousand times, (a power much beyond what is ever actually applied to the whole body of the moon,) would still leave the moon in effect two hundred and forty miles from us, and her size and light would be very different from the same body viewed on the *summit of the Alps*. A railroad car, in order to reach the moon *in a month*, or thirty days, must travel eight thousand miles a day. The specific gravity of ordinary rocks is not from *three to four*, but from *two to three*. The argument in chapter tenth, from which it is inferred that the earth is really the *largest* planetary body in the Solar System, appears to us not only unsatisfactory, but absurd.

Similar inadvertencies are equally numerous in the work of Sir David Brewster. “With respect to the number of moons or satellites, (says he,) the Earth has the lowest number, *all the planets, exterior to it, having a larger number*.” We think he cannot be ignorant that Mars has no satellite. Again he says: “The temperature of our own globe decreases as we rise in the atmosphere and approach the sun, and it increases as we descend into the bowels of the earth, and go farther from the sun. The increase of heat, as we approach the surface of the earth from a great height in a balloon, or from the summit of



a lofty mountain, *is produced by its atmosphere*; and in Jupiter the atmosphere may be so formed as to compensate, to a certain extent, the diminution in the direct heat of the sun arising from the great distance of the "planet." It is a new idea that the greater heat of the lower portions of the atmosphere, in comparison with the higher, is *produced* by the atmosphere itself, and is not derived from the sun. We have supposed it a settled doctrine of Meteorology, that, since radiant heat, like the sun's rays, passes through a transparent medium without producing much effect upon it, the earth is first heated by the sun, and the atmosphere is heated by circulating over it and coming in contact with the surface of the earth. Of course those portions of the atmosphere are heated most which are nearest the earth, and the upper portions only by the ascent of such volumes as have risen from below in consequence of their diminished specific gravity, on imbibing the heat of the earth.

In chapter fifth, the Sun's diameter is stated at eighty-eight thousand miles, "upwards of one hundred times the size of our earth." We should have supposed this to be a misprint for eight hundred and eighty thousand, and that one hundred times the *size* of the earth meant in diameter and not in volume, had not the same statement recurred in chapter sixth, in a different connection. In the eighth chapter, the author says that no change of place has been observed in *single* fixed stars, excepting that which is common to them all, and arises from the motion of our own system; and yet, according to Sir John Herschel, (Outlines, Art. 852,) among stars not double, and no way differing from the rest in any other obvious particular,  $\epsilon$  Indi,<sup>2</sup> and  $\mu$  Cassiopeiæ are to be remarked as having the greatest proper motions of any yet ascertained, amounting respectively to  $7''.74$ , and  $3''.74$  of annual displacement.

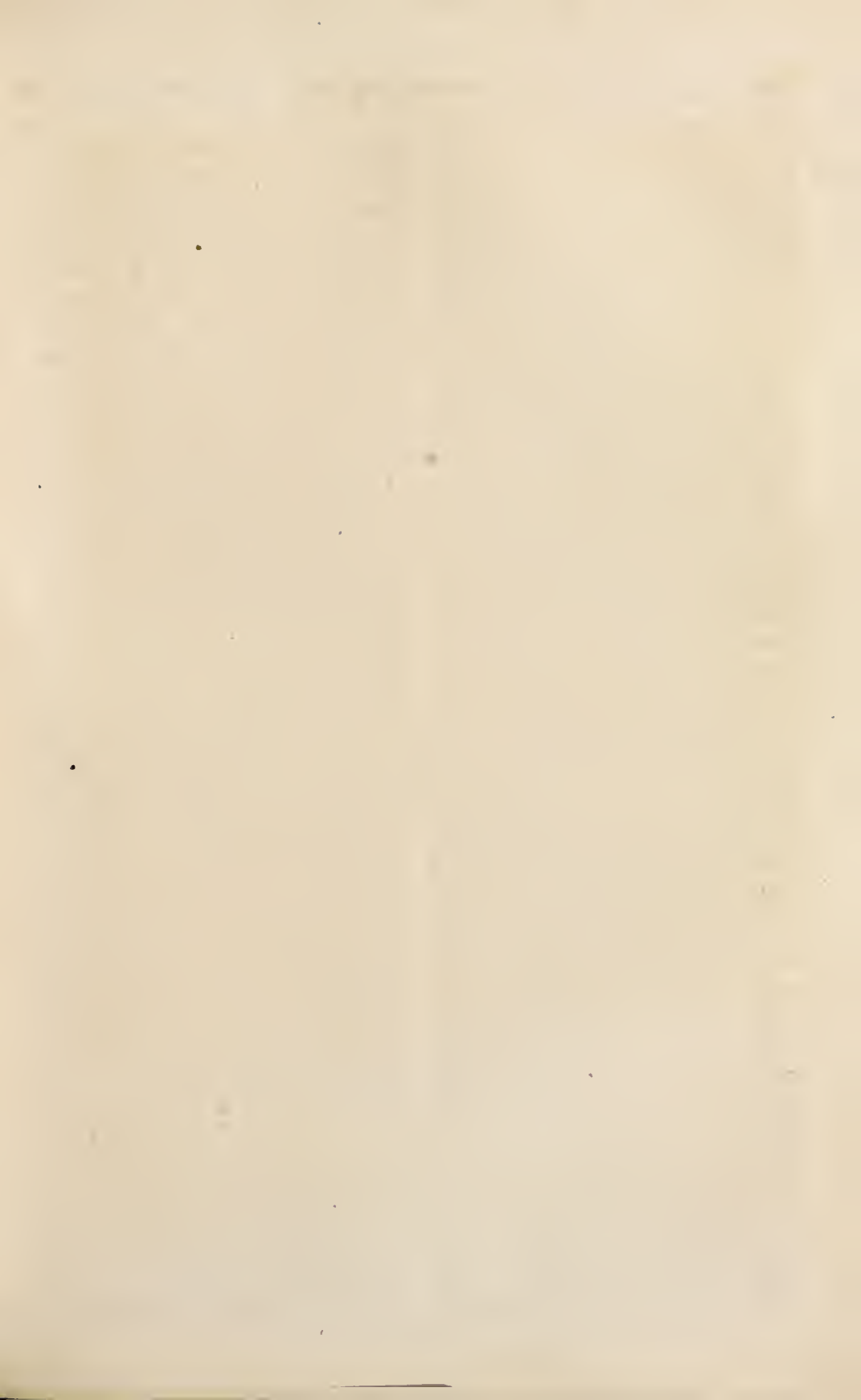
We notice these few inaccuracies in the works before us, without any wish to throw discredit on the authority of the eminent and excellent authors, but because we deem it a matter of much importance that scientific statements, especially when emanating from such a source, should possess entire accuracy. We trust the proper corrections will be applied in subsequent editions, including several other slight errors which we have not room to notice.

We have only a remark or two to make in conclusion. After some reflection on the question of a Plurality of Worlds, (meaning thereby the question whether the planets are inhabited, and whether the stars are suns and centers of planetary worlds,) we are inclined to believe in the affirmative, although

we do so with a full conviction that there is much to be said on the other side. We will suppose that a man had never seen but one clock, namely, his own, and that all his ideas of the structure of an instrument for keeping time, were founded on this. We will now suppose him to visit a manufactory of clocks, where he sees a great number of them in various stages of manufacture, but all in cases and furnished with dial plates, so as to exhibit externally the appearance of being finished. He at once recognizes in the general form and features of the article such a resemblance to his clock, that he immediately concludes the instruments before him to be time-pieces. The dial plate marked with the hours of the day, and the pointers, the one a minute hand and the other an hour hand, are alone sufficient to convince him of this; nor is he less inclined to believe them to be time-pieces because the sizes differ from that of his own clock and from one another, or because the cases are somewhat different, or the color and ornaments unlike those of his own; for none of these circumstances affect the peculiar principles belonging to a time-piece. On opening the cases, however, and inspecting the various structures, he finds reason to doubt the correctness of his first impressions. In one he finds no pendulum, which is so essential a feature in his own clock, that if this were wanting, it could not serve the purpose of a time-keeper. He might therefore infer that, although the external appearance of the instrument led him to believe it was a clock, yet a view of its interior mechanism proves to him the contrary. He would, however, reason erroneously, for clocks can be made to keep time without pendulums, although he had never seen, nor could he conceive of such. Examining further, he finds one of the instruments furnished with a pendulum, but having no weights. Everything seems complete with this exception; but since the vibrations of the pendulum of his clock will not continue long without weights, he either concludes, as before, that the instrument cannot be designed for a time-piece; or, at most, infers that, although possibly designed for such a purpose, it is in an unfinished state,—a state of progress towards completion. But he would again reason erroneously, since springs, as well as weights, afford the means of keeping up the vibrations of a pendulum. On examining a third specimen, he finds both pendulums and weights, but discovers that the instrument has but few wheels compared with his own clock, and hence infers that it cannot be a time-piece, or that there are many wheels to be added before it can become one; as before, it is, he thinks, only in a state of progress towards such an instrument. But here his conclusions may be wrong, because clocks can be

made with fewer wheels than his own clock may have contained. In his progress, however, he at length comes to specimens so perfectly destitute of all mechanism, as fully to justify the inference that they are wholly unfitted for keeping time. If among the hundreds or perhaps thousands of specimens to be seen in the manufactory, a few groups were packed together, or arranged with great exactness, according to a certain order, the presumption would be stronger that these, at least, were all finished time-pieces, than that such were finished, as were promiscuously scattered about the establishment.

The case of the man in relation to the different clocks, is like that of an inhabitant of the earth with regard to the heavenly bodies. At the first view of them with the telescope, finding them to be as large or larger than the earth, round like that, and turning on their axes to afford the vicissitudes of day and night; being also like our own planet, lighted and warmed by the same sun, and revolving about the same center, he concludes that they likewise are the abodes of life and intelligence. This conviction is strengthened by the consideration that they are arranged together in a perfect system, in a certain determinate order, like clocks in readiness for market. On a nearer view he finds them destitute of certain provisions, as air and water, which, if wanting to the earth, would render it uninhabitable; and the aspect of the surface, devoid of a vegetable kingdom, and having the sterile appearance of volcanic lava, still further indicates their unsuitableness for the abodes of animals. Hence he concludes that animals cannot exist there. But he would have no right to draw such a conclusion, inasmuch as animal life can and does exist where there is neither air, nor water, nor vegetables; and though all these are essential to the sustenance of man, yet, since the Creator sustains other tribes of animals without them, it is clearly within His power to do this in the case of rational beings; not indeed constituted like man, but still of a nature adapted to the circumstances in which He has placed them. To infer that rational beings cannot exist without water, or vegetables, or air, is as erroneous as to infer that a clock cannot go without a pendulum.







THOUGHTS ON THE DISCOVERY OF LE VERRIER'S  
PLANET.—BY PROFESSOR OLMSTED.

(*From the New Englander, for Jan., 1847.*)

At a sitting of the French Academy in June last, a paper was presented written by M. Le Verrier, a young astronomer of Paris, the object of which was to prove that there exists, in the solar system, a planet hitherto unknown, situated at double the distance of Uranus from the sun, which on the first of January, 1847, would be at or near a point in the ecliptic whose longitude is 325 degrees. This extraordinary hypothesis has recently been verified by the actual observation, with the telescope, of the body in question. It was first seen by Dr. Galle a distinguished astronomer of Berlin, on the 23d of September, and it has since been seen at London, and more recently at various observatories in our own country.

Although in apparent brightness this stranger is equal only to a star of the eighth magnitude, and consequently must remain forever invisible to the naked eye, yet the telescope invests it with all the characters of a planet, readily distinguishing it from the fixed stars by its perceptible disk, and by its motion around the sun, which, though comparatively slow, is still vastly greater than belongs to any of the stars. It requires more than two hundred years to complete its circuit; and although its exact magnitude is not yet determined, yet enough is known to assure us that it is one of the largest of the planets, and more than a hundred times as large as the earth.

This discovery, by theory alone, of a body hidden so deep in the abyss of space, and until now invisible from the creation of the world, determining not only its existence but its exact place among the stars, proclaims most audibly the perfection at which physical as-

tronomy has arrived; and it invests truth itself with a solemn grandeur, when we think how far into the recesses of nature it will conduct the mind, that diligently follows its leadings, even in the secret retirement of the closet.

The method of investigation, although laborious and intricate, is not difficult to be understood, but may be described in very simple terms. The planet Uranus (Hersehel) has been long known to be subject to certain irregularities in its revolution around the sun, not accounted for by all the known causes of perturbation. The tables constructed with the greatest care for any particular epoch, from observations on the planet, guided and corrected by the theory of universal gravitation, do not accurately give its place at periods of a few years either before or after that time. In some cases the deviation from the true place, as determined by observation, has been two minutes of a degree—a quantity indeed which seems small, but which is still far greater than occurs in the case of the other planets, Jupiter and Saturn for example, and far too great to satisfy the extreme accuracy required by modern astronomy. From 1781, when Hersehel discovered this planet, to 1821, observations had been accumulated on its motions for forty years, a period abundantly sufficient to afford the necessary data for determining the elliptic elements of its orbit. Indeed, there were older observations than these, scattered along a whole century; for before this body was determined to be a planet, it had been recognized and its places assigned as a star of the sixth magnitude. In the year 1821, Bouvard, a French mathematician of emi-

nence, compiled tables of this planet, availing himself of the most recent, and what he deemed the most perfect observations, and allowing for the perturbations occasioned by the attractions of the other planets, chiefly those of Saturn and Jupiter, which on account of their great masses, as well as their proximity to Uranus, would of course more or less disturb his motion. Bouvard himself, however, was struck with the fact that his tables were incompetent to represent the actual places of the planet, as it had been seen by the older astronomers, and he even suggested the possibility of an unknown planet, whose hidden action upon Uranus occasioned the disagreement in question.

For the benefit of such of our readers as have not given their attention to subjects of this kind, it may be premised, that, in accordance with the law of universal gravitation, every body in the solar system is attracted by and attracts every other; that such large bodies as Saturn and Jupiter exert a powerful influence in disturbing other members of the system, their effect being sensibly felt upon the earth, although, when nearest, the former is distant from us about eight hundred and the latter about four hundred millions of miles; that this disturbing force exercised by one body of the system over the others, is proportioned to its quantity of matter, or mass, and is therefore so much greater in the sun than in one of the planets, only because the sun contains so much more matter than the planet; that, in the same body, the power of attraction diminishes rapidly as the distance is increased, being four times less when the distance is doubled, and nine times less when the distance is trebled; or, as astronomers express it, the attraction diminishes in proportion as the square of the distance is increased. In order, therefore, to form tables which shall truly

represent the motions of a planet revolving around the sun in an elliptical orbit, it is necessary not only to estimate the different velocities which the body would have on account of its different distances from the sun, arising from the eccentricity of its orbit, but to allow also for the united effects of all the disturbing influences (perturbations) which result from the actions of the other bodies of the system, some of which tend to accelerate it, others to retard it, and others still to turn it out of its path. Thus the exact place of a ship, even when carried forward by a uniform breeze, can not be determined from the reckoning, only after due allowance is made for all the currents that have either conspired with or opposed its progress. Before such allowances can be made and applied, the exact *weight* of each of the bodies in the system must be known; and it is one of the sublime results at which modern astronomy has arrived, that the planets have in fact been weighed as in a balance, and their respective quantities of matter ascertained with as much precision, as that of an ordinary article of merchandise.

Now the only planets in the solar system heretofore known to disturb the motions of Uranus, are Saturn and Jupiter, the other planets being so far off and so small, that their attraction is insensible. In constructing tables, therefore, to represent the motions of Uranus, or by means of which its exact place in its orbit can at any time be calculated, it was only deemed necessary to allow for the disturbing influences of these two great planets. But after making the allowances required, still, after a few years, Uranus was found by observation to deviate very materially from the calculated place. *Some* other cause, therefore, must disturb its motions beside the attractions of Jupiter and Saturn. Several hypotheses have



been at different times proposed to account for the disagreement in question.

First, it has been urged that at so remote a distance from the sun as eighteen hundred millions of miles, (the distance of Uranus,) the law of gravitation itself loses somewhat of its constancy or uniformity; consequently, tables founded on this law, as those of Uranus are, would not give results exactly conformable to observation. This hypothesis is not only unsupported by any evidence, but is at variance with all known facts in astronomy. Halley's comet, for example, during its late revolution, departed to a distance from the sun equal to twice that of Uranus, (about 3,600,000,000 of miles) yet on its return in 1835, after an absence of more than seventy-five years, it was true to the time appointed, having come to its perihelion within a day of the time assigned to it by calculations founded on the law of universal gravitation. Moreover, we have independent proof of the unerring uniformity of this law, when extended to distances far greater than that of Uranus from the sun, or than that of Halley's comet in its aphelion, since it is found to prevail even among the stars, regulating the revolution of sun around sun, as is now proved concerning the binary stars, those double stars of which the two members revolve about a common centre of gravity, which they do in exact obedience to the law of universal gravitation.

Secondly, the *resistance of a supposed ether*, or subtle elastic medium, diffused through space, has been assigned as the cause of the phenomenon in question. But, were this the cause, we might expect to see it manifested in the motions of the other planets, and the more as their motions are more rapid than those of Uranus. The existence of such a medium has indeed been inferred, in consequence of certain

effects manifested in the movements of Encke's comet; but it may be easily conceived, that an exceedingly light body would indicate such a resistance, while a dense body like the planets would not. A particle of down may experience resistance, when moving swiftly through a medium, where a musket ball would not be sensibly affected.

Thirdly, the hidden influence in question has been ascribed to a *great satellite* of Uranus, hitherto undiscovered. But the perturbations occasioned by such a satellite would be of short period, completing a cycle during the revolution of the satellite about its primary, which would occupy but a short time, whereas the changes in the perturbations occasioned by the cause under consideration are exceedingly slow. Moreover, in order to produce effects on Uranus so great as those to be accounted for, a very large satellite, would be required, of such a magnitude, indeed, that it would not fail to be seen with the telescope.

Fourthly, the disturbing influence of a *comet*, has also been proposed to account for the irregularities of Uranus. But comets have never been known to exert any appreciable influence upon the motions of the planets. The comet of 1770 passed among the satellites of Jupiter, without sensibly disturbing their movements,—a proof that the quantity of matter in these bodies is inconceivably small. Nor, from the eccentricity of the orbits of comets, could we suppose a comet to linger in the immediate vicinity of Uranus so long, as the perturbations which it is assumed to account for are known to have existed.

Finally, the suspicion of the existence of a planet, lying beyond the orbit of Uranus, did not originate with Le Verrier, but had been entertained by several eminent astronomers, for twenty-five years before the subject engaged his attention.

But merely to *conjecture* the existence of such a body, or even to assert its existence without proof, implies very little; but to establish its existence by satisfactory evidence, and still more, to tell *where* it lies among the myriads of stars, to weigh it, to assign its distance from the sun, and the period of its revolution—these are the points of difficulty, and it is the successful solution of the problem under all these various aspects, that constitutes the glory of this youthful astronomer.

Le Verrier did not undertake the formidable task of determining these points, until he had fully proved, that the disagreement between theory and observation in the motions of Uranus, was no fault of the tables themselves; that they were true at least as far as they went. For this purpose he submitted to a new and laborious discussion the observations, both old and new, which had ever been made upon this body, from the time when its place was first noted, on the supposition that it was a fixed star, down to the present time. He re-calculated the formulæ which afforded the basis of the tables of Bouvard, and scrutinized every possible source of error in these tables. Some errors were indeed detected; but after making full allowance for these, the actual place of the planet, as determined by observation, was still widely at variance with that resulting from calculation. For example, in 1838, after calculating the maximum error which *could* exist in the tables,—an amount probably much greater than actually *does* exist,—he showed that it could not exceed 30 seconds of space, whereas the disagreement between the calculated and observed places of the planet, was 125 seconds; and, in 1831, this difference amounted to 188 seconds, of which 140 could not be explained, without admitting some other disturbing influence than that arising from the sun, Jupiter and Saturn.

Assuming, then, the existence of an undiscovered planet, the first inquiry was, *where* is it situated—at what distance from the sun—and in what point of the starry heavens?

First, it could not be below Saturn, that is, between Saturn and the sun, because then it would disturb Saturn more than it did Uranus, whereas the motions of Saturn are fully accounted for without implying any other than the attractions of known bodies. Secondly, it could not be between Saturn and Uranus; for if so, it must be very near Uranus, otherwise it would at the same time disturb the motions of Saturn. But being very near to Uranus, it must be very small, otherwise it would disturb Uranus more than it does. But a small planet, nearly at the same distance from the sun as Uranus, would be very unequal in its action on the latter planet, its disturbing force being great when in conjunction with it, but very small, or quite insensible when at a great angular distance from it. No such changes in the actual perturbations occasioned by the body occur.\* Hence, thirdly, *the body must be beyond Uranus*. Is its orbit near that of the latter planet, or remote from it? It can not be very near, for then the same inequality of action would be observed, as though it were on the other side of Uranus and near to it, which inequality does not exist. Nor can it be *very* remote, for then it must of course be very large in order to produce the perturbations it does, and being very large, its effects would be visible on Saturn as well as on Uranus, as would be the case were its distance so great that the distance between these two planets

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\* This reasoning does not appear to be entirely conclusive, since if the two bodies in question both revolve in orbits nearly circular, and at nearly equal distances from the sun, they might remain in the immediate vicinity of each other for many years.



is small in the comparison. Thus, were it ten times as far from the sun as Uranus is, then the distance between Saturn and Uranus would bear so small a ratio to the whole distance, that a body powerful enough to affect the latter so sensibly, would exert at least an appreciable attraction on the former. Now the other planets, as we recede from the sun, have their orbits placed at distances continually approximating to the ratio expressed by 2, the distances of each planet in succession growing nearer and nearer to double that of its predecessor. Thus Saturn is nearly twice as far from the sun as Jupiter, and Uranus more nearly twice as far as Saturn. Hence it was most reasonable to expect, that the orbit of the planet sought, would be situated at twice the distance of Uranus; that is, at about three thousand six hundred millions of miles from the sun. On trial, Le Verrier found that a planet whose orbit was thus situated, would fulfil the conditions rendered necessary by the changes which the perturbations themselves undergo, and that no other distance would do it. Hence it was inferred, that *the unknown planet revolves around the sun at double the distance of Uranus*. The distance from the sun being determined, and the orbit, like those of the other planets, being supposed nearly circular, its period or time of revolution might be found by Kepler's law,—that the squares of the periodic times are proportioned to the cubes of the distances.\* By this law its period would appear to be about 237 years. This was to be regarded as only a first approximation. We shall find that

\* Hence, the distance being double that of Uranus and the periodic time of the latter being 84 years, we have  $1^3 : 2^3 :: 84^2 : 2^3 \times 84^2 = 8 \times 84^2 = \text{square of the periodic time of the body sought}$ . Therefore, the time itself  $= \sqrt{8 \times 84} = 237.463$  years.

the actual period is somewhat less than this.

It was easy to show that its orbit must be nearly coincident with the ecliptic, since the perturbations occasioned by it were nearly all in the direction of the ecliptic, and not at right angles to it; that is, they were perturbations of longitude and not of latitude.

To these extraordinary but apparently satisfactory results, the paper of Le Verrier presented to the French Academy at their sitting on the first of June, conducts us. Being now fully convinced himself of the existence of the planet sought, and intent on finding its true place, this able astronomer still continued his laborious researches, until he was able to deduce, mathematically, those conclusions which had before rested chiefly on analogical evidence, or at least upon general inferences derived from the doctrine of universal gravitation. Equations were formed between the irregularities of Uranus to be accounted for, and the elements of the body in question, both known and unknown. These equations involved nine unknown quantities, and their resolutions presented difficulties apparently insurmountable; but by the most ingenious artifices the several unknown quantities were successively eliminated, either directly or by repeated approximations. Moreover, in science as in morals, the pathway of truth is easy and simple, and grows continually plainer and plainer, while that of error is thorny, and, as we advance, becomes at every step more and more complicated. All who have attempted difficult solutions of mathematical or physical problems, must have been aware what unexpected facilities often suddenly appear in the resolution of complex expressions, which contain the hidden truth; they must have been most agreeably surprised, to see involved and



apparently unmanageable members of equations cancelling each other, and suddenly vanishing, and difficult expressions falling off at the right and left, and constantly simplifying their work as they approach nearer and nearer to the final expression, which contains the naked truth. Hence the maxim, that *Nature is very kind to those who faithfully study her laws*.

Such encouraging facilities seem to have inspired our young astronomer, in his difficult and laborious undertaking, until he arrived at expressions for the elements of the unknown planet, which gave its exact place among the stars, its quantity of matter, the shape of its orbit, its distance from the sun, and the period of its revolution. At the sitting of the Academy on the 31st of August, these latest results were presented, stated numerically as follows:—

Longitude of the planet, }	326° 32'
Jan., 1, 1847.	
Mass, that of the sun being 1, .	$\frac{1}{3555}$
Eccentricity, . . . . .	0.107
Time of revolution, . . . . .	217.337 yrs.
Longitude of the perihelion, .	234° 45'
Major axis of the orbit, that }	36.154
of the earth being 1, }	

He was therefore enabled to say, that the planet was then just passing its opposition, and consequently was most favorably situated for observation, and, on account of the slowness of its motions, would remain in a very favorable position for three months afterwards. In order to test the correctness of these elements, the effect of a planet, having these conditions, was investigated in relation to the motions of Uranus, in order to see how well the places of that planet, determined by the aid of these corrections for many different periods, would correspond to the places actually observed at those times. We must bear in mind, that the discrepancies between the calculated and observed places, without these corrections, was enormous, sometimes amounting to 125

seconds of an arc. The comparison was made in respect to thirty-three sets of observations, of which *twenty-six* were selected from observations made since 1781, when the planetary character of the body was first made known, and *seven* from the records of previous observers, who had marked its place supposing it to be a fixed star, from that of Flamsteed in 1690, to that of Lemonnier in 1771. The places of Uranus, determined with the new elements, agreed with the places actually observed at these later periods, generally within one or two seconds, and often within the fraction of a second; and with the earlier periods, with one exception, to within about seven seconds. These elements of the unknown body, were varied and the limits ascertained to which such changes could be carried, without involving a greater disagreement between the calculated and observed places; and these limits were found to be included within a very narrow compass. On every side the existence of an unknown planet, having these elements, forced itself on the belief of *Le Verrier*, and he probably felt as confident of its existence before it was seen in the heavens, as he has done since. Still it became an important inquiry at last, whether there was any hope of ever seeing the interesting stranger, or whether after so much toil, the indefatigable student must rest his belief in its existence, solely upon his faith in the immutable laws of truth, whose leadings he had followed into depths of space so profound, and must take his dubious chance for fame in the weak belief of the few, and the total incredulity of the many. In estimating the probability that the planet would be visible to the telescope, he reasoned thus. Uranus has an apparent diameter of four seconds, and since the mass, or quantity of matter, of this planet is two and a half times less than that

of the planet sought, were the density of the latter known, we could easily find its volume, and then, knowing as we do its distance, its apparent diameter would be easily determined. Now it is a known fact, that the densities of the planets decrease as we recede from the sun, and therefore the density of the body in question is probably less than that of Uranus,—a circumstance which would contribute to increase the comparative volume, and of course the apparent diameter. But even allowing the density to be as great as that of Uranus, the apparent diameter will be over three seconds, and consequently, the planet ought to be visible in good telescopes, and with a perceptible disk. If among the small stars situated in that part of the heavens where the planet is at present, a faint body be discerned having a perceptible disk, it will at once be recognized as the planet itself; but if no such appearance should distinguish it from the small stars surrounding it, then a map of these must be carefully inspected; and if any one of the luminous points included in the map shifts its place, indicating a movement more rapid than belongs to any of the fixed stars, then that luminous point will be recognized as the body sought. It happened, fortunately, that charts of that region of the heavens were in the course of publication at Berlin, containing a perfect representation of all stars to the tenth magnitude; and the very folio containing the constellation Capricornus, in which the hidden body was supposed to be, was then just issuing from the press. Le Verrier, therefore, wrote to M. Gallé of Berlin, communicating his latest results, and requesting him to reconnoitre for the stranger, directing his telescope to a point about five degrees eastward of a well known star called *Delta Capricorni*. So precise and complete were these directions, that the Prussian

astronomer no sooner pointed his telescope to the region assigned, than he at once recognized the wondrous body. Its place was only 52 minutes of a degree distant from the position marked out for it by Le Verrier, and its apparent diameter was almost the same that he had assigned.

M. Gallé's letter to Le Verrier announcing his discovery, reached the latter while an article of his on the latitude of the planet, was in the course of preparation for the sitting of the Academy on the 5th of October. This confirmation of all his hopes, is added to his paper in a modest postscript, in terms less evasive of exultation than might have been anticipated. But the very phraseology of Gallé indicates that, previous to the actual discovery, he had himself but feebly embraced the idea of its existence. "The planet (says he) which you have described, *really exists!*" The congratulatory letters which now flowed in from the most celebrated astronomers of Europe, occupy a conspicuous place in the *Comptes Rendus* of Oct. 5th, being communicated to the Academy by M. Arago, accompanied by very interesting remarks on the history and importance of the discovery. "Other astronomers (said M. Arago) have sometimes found, accidentally, a *movable point*, in the field of their telescopes, which proved to be a planet; but M. Le Verrier described the new body without having occasion to take a single look towards the heavens—he saw it *at the point of his pen*. He determined, by the power of the calculus alone, the place and the magnitude of a body situated far beyond the known limits of our planetary system; of a body whose distance from the sun exceeds 1200 millions of leagues, and which in our most powerful telescopes offers a disk scarcely perceptible. In fine, the discovery of Le Verrier is one of the most brilliant manifestations of



the exactness of modern astronomical systems; and it will encourage the ablest geometers to search, with new ardor, for the eternal truths which, according to an expression of Pliny, *lie hidden in the majesty of theories.*" M. Arago adds, that he had received from M. Le Verrier a most flattering commission—the right of naming the new planet, and therefore he proposes to call it *Le Verrier*. When Sir William Herschel first discovered the planet Uranus, he named it after his royal patron, *The Georgian*; but this being an unpopular appellation in France, La Lande proposed to call it *Herschel*, and this name has continued in our own country to the present time. But, as the other planets have names derived from the ancient mythology, as Mercury, Venus, Mars, Jupiter and Saturn, it seemed to the leading astronomers of the day, most accordant with sound analogy and good taste, to give it a corresponding appellation; and they, therefore, after proposing a number of mythological names, fixed upon that of Uranus, (the most ancient of the gods,) and this name has generally prevailed. But Arago, for the sake of securing the desired honor to Le Verrier, proposes to restore the same to Herschel, and that the planet Pallas also shall be named from its discoverer *Olbers*. The names *Janus*, *Nep-tune* and *Oceanus*, have also been proposed by others; and time only can decide which of the names will finally prevail.

For a time the contest for the honor of this achievement, seemed likely to awaken the ancient national rivalries of France and Great Britain. The English astronomers claimed that a young mathematician of Cambridge, Mr. Adams, had, without the least knowledge of what M. Le Verrier was doing, arrived at the same great result. But having failed to publish his paper until the world was made acquainted with the

facts through the other medium, he has lost much of the honor which priority of discovery would have gained for him, although great admiration may ever be felt for his genius and capacity. In the history of great discoveries and of great inventions, it is a remarkable fact, that the same idea has frequently occurred to two individuals nearly at the same time. Thus it is still a question, whether Newton or Leibnitz first devised the method of Fluxions; and the greatest single discovery in Chemistry, that of oxygen gas, was made almost simultaneously by Priestly in England, Scheele in Sweden, and Lavoisier in France. The explanation is easy. The secret rests in the Eternal Mind, and is withheld from the view of man, until, in the progress of society, all things are ready; then the curtain is withdrawn, and Truth darts her heavenly rays upon the few, who are at the moment gazing towards her with the clearest vision. Would we give the due meed of praise to all who have contributed to bring about this grand triumph of the human mind, our honors must be widely distributed. In the noble array of intellects which would stand before us, Newton, who furnished the mighty key that turns the secret wards of creation, must undoubtedly occupy the highest place. But Kepler, who first traced the existence of laws in the planetary system; Flamsteed, Lemonnier and Bradley, who noted the places of the planet Uranus, at different periods, mistaking it for a fixed star; Herschel, who brought it to light and established its planetary character; Leibnitz, La Grange, and La Place, who invented and perfected that wonderful instrument of research into the arcana of nature, the fluxionary calculus: these all, and many more, are entitled to share with Le Verrier the glory of this discovery.

It is characteristic of great truths, that have been attained by long and



laborious processes, to draw after them many other great truths, which they serve to establish. If their discovery has brought into requisition the profoundest principles of science, it follows that those principles, leading as they have done to a correct result, a result which nature owns, are themselves true, and receive, in the discovery, a confirmation the more signal as that result is the more hidden from ordinary view. Seldom has this point received so beautiful an illustration as in the discovery of *Le Verrier's planet*. Let us glance at the several great truths which this discovery confirms and illustrates.

In the first place, it affords a triumphant proof of the truth of *the law of universal gravitation*. It was the knowledge of this law, which first suggested the existence of such an undiscovered planet, since it was only on the supposition of the universal prevalence of of this law, that the unexplained irregularities in the motions of *Uranus*, were referred to such a hidden body. It was also by the application of the law of universal gravitation, in its exact expression, namely, that it acts in proportion to the quantity of matter, and inversely as the square of the distance, that the invisible cords which bound the stranger to the planet *Uranus* were followed back through the depths of space, until they revealed in the wide expanse of heaven, the very spot where it lay concealed. The law of gravitation, therefore, answers completely to the test proposed by Lord Bacon, that before a new discovery can be considered good, nature should respond to it through all her works. *Non canimus surdis, respondent omnia silvæ*. It is said that Newton, when engaged in his first computation on the motions of the moon, instituted for the purpose of verifying his theory of gravitation, seeing, as he approached the end of the solution,

that all was coming out in exact numerical conformity to the doctrine, was so overwhelmed with the great consequences of the discovery, that he grew nervous and was unable to complete the computation, but was obliged to hand it over to a friend to finish it. These consequences were, indeed, well fitted to overpower even the mind of Newton, since the simple truth which was beginning to shine out with perfect clearness in his mathematical expressions, unveiled nothing less than the hidden mechanism of the Universe, and would give to the astronomer the power, almost divine, of looking through all time, present, past, and future. But, probably, Newton himself did not at once comprehend in his mighty grasp all the great consequences of the truth he was approaching. The astonishing reach of the principle of universal gravitation can scarcely now, after the lapse of one hundred and fifty years, be fully comprehended, since almost daily, like a sounding line sent out into the depths of creation, it is disclosing to us new wonders respecting the worlds hidden in the abyss of space. How vast and unexpected are the results it has afforded in our knowledge of the grand machinery of nature! It accounts for all the celestial motions, whether of planets, comets, or stars; it teaches how to weigh the sun and planets as in a balance; it assigns the exact figure of the earth, and of every body in the solar system, independently of any measurement or observation; it explains not only the ordinary motions of the heavenly bodies, but all their irregularities, of which the moon alone has no less than sixty, and assigns the exact numerical value of each; it accounts for the tides, and, with the aid of a few observations, computes the height for every time and place; it teaches how, by means of the pendulum, to fix an invariable standard of

weights and measures ; it suggests new fields to the eye of observation, directing attention to objects which have eluded the keenest vision, aided by the highest powers of the telescope, and corrects the last refinements of instrumental measurement ; it has led to the grand result of the stability of the Universe, amid all the apparent causes of disorder and ruin ; it follows the comet through all the planetary realms, almost to the region of the stars, and brings it back again on the day which itself appoints for its return ; and, finally, it tells us of new planets still lurking in the solar system, points out their hiding places, assigns their exact weight and the period of their revolution, and directs the practical astronomer precisely where to point his telescope, to bring them down to earth. If any thing more could be wanting to establish the truth of the doctrine of universal gravitation, we surely have it in this last and most wonderful of all its revelations.

In the second place, the discovery under review proclaims the unerring certainty of the *method of Fluxions*, or the *Infinitesimal Calculus*. The fundamental principles of the Calculus are difficult to be expressed in an elementary form. So refined and almost spiritual are some of them, that it is only after having made some proficiency in the use of this method, that the learner feels fully assured that it rests on a foundation as immutable as pure geometry itself. But the discovery before us was attained by the calculus, applied, in its most refined and subtle forms, to the law of universal gravitation. The hidden truth was caught in its magic folds, but was so deeply involved within them, that to develop it and bring it, in its simple unity, to the light of day, required a labor and a skill which may well be compared to the task of finding a grain of gold when hidden among the sands of the sea shore, and as exceeding all that the

ancients conceived of the difficulty of threading one's way through the mazes of the Cretan labyrinth. Moreover, if we resolve the Calculus itself into the elementary principles of mathematics which it employs, in one or other of its processes, we shall find that the whole of this science is inwoven in its fabric ; and it follows, that a confirmation of the truth of the method of Fluxions, is, at the same time, a confirmation of the exact, eternal truth of the entire science of mathematics.

In the third place, the discovery of Le Verrier's planet, proves that *the other planets of our system are correctly weighed*. It is only on the supposition that the quantity of matter in Jupiter and Saturn is exactly determined, that it could be inferred that their united actions upon Uranus, to disturb his motions, was insufficient to account for his irregularities ; nor, had there been any essential error in the estimates of the masses of those planets, could it have been determined what amount of error remained to be accounted for by the hidden body.

In the fourth place, we derive from this discovery new confidence in the *uniformity of the laws of nature*. This doctrine, now so generally taken as an axiom, is by no means self-evident, nor has it always been actually believed. The ancient schools of philosophy taught just the opposite doctrine, averring that we could never know from what takes place in our world, what laws prevail in distant worlds ; that motion itself was one thing on earth, and perhaps quite another thing in the skies. Such a belief was the natural fruit of their mythology—a religion which distributed the several parts of the natural world to different divinities, Jupiter being lord of the air, Neptune of the sea, and Pluto of the realms below ; while various subordinate deities controlled particular kingdoms in the great empire of nature, Eolus presiding



over the winds, and Urania over the starry sphere. In these distant and independent realms, therefore, it was natural to believe that different laws prevail as different monarchs rule; but the religion of the Bible, teaching as it does the doctrine of one God, leads us to anticipate the grand result, proclaimed by all the discoveries of astronomy, of a perfect uniformity in the laws of nature, throughout her boundless realms, in earth, in air, in ocean, and in the remotest planets and stars.

Finally, the *harmonies of truth*, and the attribute by which she remains forever one and indivisible, are strikingly illustrated in the example before us. The astronomer in his closet constructs a series of mathematical formulæ, complicated perhaps, but all rising upon the immutable basis of mathematical demonstration. These he transforms, in a thousand ways, spreading them over reams of paper. All the while the truth, for which he is seeking, lies concealed deeply hidden beneath

massive piles, with which it is encumbered. These one by one, often to the surprise of the operator himself, melt away, until, at length, the truth, so long and so laboriously sought, divested of every disguise and incumbrance, shines out in its own native simplicity and beauty. But if it is true in theory, it is true also in fact; and the astronomer now sallies forth from his closet, and looks upward with his telescope, and there sees the confirmation of all his labors written on the skies. Not only do we find here new cause to admire the harmonies of truth, but its *fertility*, or the power of truth to beget truth, urges itself upon our consideration with new force, when we think how the discovery of the planet Uranus has furnished the key to the discovery of another planet nearly twice as far removed into the depths of space, which, again, in its turn, has perhaps an equal chance of guiding us on the way to still more distant worlds.

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# A D D R E S S

DELIVERED AT

THE SOUTHAMPTON MEETING

OF

THE BRITISH ASSOCIATION FOR THE  
ADVANCEMENT OF SCIENCE,

SEPTEMBER 10, 1846,

BY

SIR RODERICK IMPEY MURCHISON, G.C.S.T.S.,

F.R.S., V.P.G.S. & R. Geogr. S.,

MEM. IMP. ACAD. SC. OF ST. PETERSBURGH, CORR. MEM. ROY. INST. SC. PARIS, ETC.,

*P R E S I D E N T.*

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1846.





# A D D R E S S

BY

SIR RODERICK IMPEY MURCHISON, G.C.St.S., F.R.S., V.P.G.S.  
&c. &c.

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GENTLEMEN,

AFTER fifteen years of migration to various important cities and towns in the United Kingdom, you are for the first time assembled in the South-Eastern districts of England, at the solicitation of the authorities and inhabitants of Southampton. Easily accessible on all sides to the cultivators of science, this beautiful and flourishing sea-port is situated in a district so richly adorned by nature, so full of objects for scientific contemplation, that, supported as we are by new friends in England, and by old friends from the farthest regions of Europe, we shall indeed be wanting to ourselves, if our proceedings on this occasion should not sustain the high character which the British Association has hitherto maintained.

For my own part, though deeply conscious of my inferiority to my eminent predecessor in the higher branches of science, I still venture to hope that the devotion I have manifested to this Association from its origin to the present day, may be viewed by you as a guarantee for the zealous execution of my duties. Permit me then, Gentlemen, to offer you my warmest acknowledgements for having placed me in this honourable position; and to assure you, that I value the approbation which it implies as the highest honour which could have been bestowed on me—an honour the more esteemed from its being conferred in a county endeared to me by family connexions, and in which I rejoice to have made my first essay as a geologist.

The origin, progress and objects of this our "Parliament of Science" have been so thoroughly explained on former occasions by your successive Presidents, particularly in reference to that portion of our body which cultivates the mathematical, chemical and mechanical sciences, that after briefly alluding to some of the chief results of bygone years, with a view of impressing upon our new members the general advances we have made, I shall in this discourse dwell more particularly on the recent progress and present state of natural history, the department of knowledge with which my own pursuits have been most connected, whilst I shall also incidentally advert to some of the proceedings which are likely to occupy our attention during this Meeting.

No sooner, Gentlemen, had this Association fully established its character as a legitimate representative of the science of the United Kingdom, and by

the Reports which it had published, the researches which it had instituted, and the other substantial services which it had rendered to science, had secured public respect, than it proceeded towards the fulfilment of the last of the great objects which a Brewster and a Harcourt contemplated at its foundation, by inviting the attention of the Government to important national points of scientific interest. At the fourth Meeting held in Edinburgh, the Association memorialized the Government to increase the forces of the Ordnance Geographical Survey of Britain, and to extend speedily to Scotland the benefits which had been already applied by that admirable establishment to the South of England, Wales and Ireland. From that time to the present it has not scrupled to call the notice of the Ministers of the day to every great scientific measure which seemed, after due consideration, likely to promote the interests or raise the character of the British nation. Guided in the choice of these applications by a committee selected from among its members, it has sedulously avoided the presentation of any request which did not rest on a rational basis, and our rulers, far from resisting such appeals, have uniformly and cordially acquiesced in them. Thus it was when, after paying large sums from our own funds for the reduction of large masses of astronomical observations, we represented to the Government the necessity of enabling the Astronomer Royal to perform the same work on the observations of his predecessors which had accumulated in the archives of Greenwich, our appeal was answered by arrangements for completing so important a public object at the public expense. Thus it was, when contemplating the vast accession to pure science as well as to useful maritime knowledge, to be gained by the exploration of the South Polar regions, that we gave the first impulse to that project of the great Antarctic expedition, which, supported by the influence of the Royal Society and its noble President, obtained the full assent of the Government, and led to results which, through the merits of Sir James Ross and his companions, have shed a bright lustre on our country, by copious additions to geography and natural history, and by affording numerous data for the development of the laws that regulate the magnetism of the earth.

The mention of terrestrial magnetism brings with it a crowd of recollections creditable to the British Association, from the perspicuous manner in which every portion of fresh knowledge on this important subject has been stored up in our volumes, with a view to generalization, by Colonel Sabine and others; whilst a wide field for its diffusion and combination has been secured by the congress held at our last meeting, at which some of the most distinguished foreign and British magneticians were assembled under the presidency of Sir John Herschel.

It is indeed most satisfactory for us to know, that not only did all the

recommendations of the Association on this subject which were presented to our Government met with a most favourable reception, but that in consequence of the representations made by Her Majesty's Secretary of State for Foreign Affairs to the public authorities of other countries which had previously taken part in the system of cooperative observation, the Governments of Russia, Austria, Prussia and Belgium have notified their intention of continuing their respective magnetical and meteorological observations for another term of three years.

In passing by other instances in which public liberality has been directed to channels of knowledge which required opening out, I must not omit to notice the grant obtained from our gracious Sovereign, of the Royal Observatory at Kew, which, previously dismantled of its astronomical instruments, has been converted by us into a station for observations purely physical, and especially for those details of atmospheric phænomena which are so minute and numerous, and require such unremitting attention, that they imperiously call for separate establishments. In realizing this principle, we can now refer British and foreign philosophers to the observatory of the British Association at Kew, where I have the authority of most adequate judges for saying they will find that a great amount of electrical and meteorological observation has been made, and a systematic inquiry into the intricate subject of atmospheric electricity carried out, by Mr. Ronalds, under the suggestions of Professor Wheatstone, to which no higher praise can be given than that it has, in fact, furnished the model of the processes conducted at the Royal Observatory of Greenwich. This establishment is besides so useful through the facilities which it offers for researches into the working of self-registering instruments which are there constructed, that I earnestly hope it may be sustained as heretofore by annual grants from our funds, particularly as it is accomplishing considerable results at very small cost.

Our volume for the last year contains several communications on physical subjects from eminent foreign cultivators of science, whom we have the pleasure of reckoning amongst our corresponding members, and whose communications, according to the usage of the Association, have been printed entire amongst the reports. In a discussion of the peculiarities by which the great comet of 1843 was distinguished, Dr. Von Boguslawski of Breslau has taken the occasion to announce the probability, resting on calculations which will be published in Schumacher's '*Astronomische Nachrichten*,' of the identity of this comet with several of a similar remarkable character recorded in history, commencing with the one described by Aristotle, which appeared in the year 371 before our æra: should his calculations be considered to establish this fact, Dr. Von Boguslawski proposes that the comet should hereafter be



distinguished by the name of "Aristotle's Comet." This communication contains also some highly ingenious and important considerations relating to the physical causes of the phenomena of the tails of comets.

Dr. Paul Erman of Berlin, father of the adventurous geographical explorer and magnetician who was one of the active members of the magnetic congress at Cambridge, has communicated through his son some interesting experiments on the electro-dynamic effects of the friction of conducting substances, and has pointed out the differences between these and normal thermo-electric effects. Baron von Senftenberg (who is an admirable example of how much may be done by a liberal zeal for science combined with an independent fortune) has published an account of the success with which self-registering meteorological instruments have been established at his observatory at Seutenburg, as well as at the national observatory at Prague.

Of our own members, Mr. Birt has contributed a second report on Atmospheric Waves, in continuation of the investigation which originated in the discussion by Sir John Herschel, of the meteorological observations which, at his suggestion, were made in various parts of the globe, at the periods of the equinoxes and solstices, commencing with the year 1834.

In a communication to the Meeting of the Association at York, Colonel Sabine traced with great clearness (from the hourly observations at Toronto) the effect of the single diurnal and single annual progressions of temperature, in producing on the mixed vapours and gaseous elements of the atmosphere, the well-known progressions of daily and yearly barometrical pressure. To the conclusions which he then presented, and which apply, perhaps generally, to situations not greatly elevated in the interior of large tracts of land, the same author has added, in the last volume, a valuable explanation of the more complicated phenomena which happen at points where land and sea breezes, flowing with regularity, modify periodically and locally the constitution and pressure of the atmosphere. Taking for his data the two-hourly observations executed at the observatory of Bombay by Dr. Buist, Colonel Sabine has succeeded in demonstrating for this locality a *double daily progression of gaseous pressure*, in accordance with the flow and re-flow of the air from surfaces of land and water which are unequally affected by heat. And thus the diurnal variation of the daily pressure at a point within the tropics, and on the margin of the sea, is explained by the same reasoning which was suggested by facts observed in the interior of the vast continent of North America.

Among the many useful national objects which have been promoted by the physical researches of the British Association, there is one which calls for marked notice at this time, in the proposal of Mr. Robert Stephenson to carry an iron tube or suspended tunnel over the Menai Straits to sustain

the great railway to Holyhead. This bold proposal could never have been realized, if that eminent engineer had not been acquainted with the great progress recently made in the knowledge of the strength of materials, and specially of iron; such knowledge being in great measure due to investigations in which the Association has taken and is still taking a conspicuous share, by the devotion of its friends and the employment of its influence—investigations which have been prosecuted with great zeal and success by its valued members Mr. Hodgkinson and Mr. Fairbairn.

I may further state, that in the recent improvements in railways the aid of scientific investigations was called for by the civil engineer, to assist him in determining with accuracy the power to be provided for attaining the high velocities of fifty and sixty miles an hour; and it was found and admitted by the most eminent engineers, that the very best data for this purpose, and indeed the only experiments of any practical value, were those which had been provided for some years ago by a Committee of the British Association, and published in our Transactions. The Institution of Civil Engineers thus gave testimony to the practical value of our researches by adopting their results.

However imperfect my knowledge of such subjects may be, I must now notice that the last volume of our Reports contains two contributions to experimental philosophy, in which subjects of the deepest theoretical and practical interest have been elucidated, at the request of the Association, by the labours of its foreign coadjutors.

That some substance of a peculiar kind everywhere exists, or is formed in the atmosphere by electrical agency, both natural and artificial, had long been suspected, especially from the persistency of the odour developed by such agency, and its transference by contact to other matter. Professor Schönbein, to whom I shall hereafter advert as the author of a new practical discovery, is, however, the first philosopher who undertook to investigate the nature of that substance; and though the investigation is not yet complete, he has been enabled to report no inconsiderable progress in this difficult and refined subject of research.

A request from the Association to Professor Bunsen of Marburg, and our countryman Dr. Lyon Playfair, coupled with a contribution of small amount towards the expenses involved in the undertaking, has produced a report on the conditions and products of iron-furnaces which is of the greatest value in a commercial view to one of the most important of our manufactures, and possesses at the same time a very high interest to chemical science in some of the views which it develops. On the one hand it exhibits an entirely new theory of the reduction, by cyanogen gas as the chief agent, of iron from the ore; on the other it shows, that in addition to a vast saving of fuel,

about 2 cwt. of sal-ammoniac may daily be collected at the single establishment of Alfreton, where the experiments were made; thus leading us to infer that in the iron-furnaces of Britain there may be obtained from vapour which now passes away, an enormous quantity of this valuable substance, which would materially lessen the dependence of our agriculturists on foreign *guano*. It is indeed most gratifying to observe, that in pursuing this inquiry into the gaseous contents of a blazing furnace of great height, our associates traced out, foot by foot, the most recondite chemical processes, and described the fiery products with the same accuracy as if their researches had been made on the table of a laboratory.

Weighed however only in the scales of absolute and immediate utility, the remarkable results of these skilful and elaborate experiments give them a character of national importance, and justly entitle the authors and the body which has aided them to the public thanks.

After this glance at the subjects of purely physical science treated of in the last volume of our Transactions, let us now consider the domains of natural history; and as one of the cultivators of a science which has derived its main support and most of its new and enlarged views from naturalists, let me express the obligation which geologists are under to this Association, for having aided so effectively in bringing forth the zoological researches of Owen, Agassiz, and Edward Forbes. These three distinguished men have themselves announced, that in default of its countenance and assistance, they would not have undertaken, and never could have completed, some of their most important inquiries. Agassiz, for example, had not otherwise the means of comparing the ichthyolites of the British Isles with those of the continent of Europe. Without this impulse, Owen would not have applied his profound knowledge of comparative anatomy to British fossil saurians; and Edward Forbes might never have been the explorer of the depths of the *Ægean*, nor have revealed many hitherto unknown laws of submarine life, if his wishes and suggestions had not met with the warm support of this body, and been supported by its strongest recommendations to the Naval authorities.

These allusions to naturalists, whose works have afforded the firmest supports to geology, might lead me to dilate at length on the recent progress of this science; but as the subject has been copiously treated at successive anniversaries of the Geological Society of London, and has had its recent advances so clearly enunciated by the actual President of that body who now presides over our Geological Section, I shall restrain my "*esprit de corps*" whilst I briefly advert to some of the prominent advances which geologists have made. When our associate Conybeare reported to us at our second meeting, on the actual state and ulterior prospects of what he well termed the



"archæology of the globe," he dwelt with justice on the numerous researches in different countries which had clearly established the history of a descent as it were into the bowels of the earth—which led us, in a word, downwards through those newer deposits that connect high antiquity with our own period, into those strata which support our great British coal-fields. Beyond this however the perspective was dark and doubtful—

*"Res altâ terrâ et caligine mersas."*

Now, however, we have dispersed this gloom, and by researches first carried out to a distinct classification in the British Isles, and thence extended to Russia and America, geologists have shown that the records of succession, as indicated by the entombment of fossil animals, are as well developed in these very ancient or palæozoic strata as in any of the overlying or more recently formed deposits. After toiling many years in this department of the science, in conjunction with Sedgwick, Lonsdale, De Verneuil, Keyserling, and others of my fellow-labourers, I have arrived at the conclusion, that we have reached the very genesis of animal life upon the globe, and that no further "*vestigia retrorsum*" will be found, beneath that protozoic or Lower Silurian group in the great inferior mass of which no vertebrated animal has yet been detected, amid the countless profusion of the lower orders of marine animals entombed in it. But however this may be, it is certain that in the last few years all Central and Eastern Europe and even parts of Siberia have been brought into accordance with British strata. France has been accurately classified and illustrated by the splendid map of Elie de Beaumont and Dufrenoy; and whilst, by the labours of Deshayes and others, its tertiary fossils have been copiously described, the organic remains of its secondary strata are now undergoing a complete analysis in the beautiful work of M. Alcide d'Orbigny. Belgium, whose mineral structure and geological outlines have been delineated by D'Omalius d'Halloy and Dumont, has produced very perfect monographs of its palæozoic and tertiary fossils; the first in the work of M. de Koningk, the second in the recently published monograph of M. Nyst. Germany, led on by Von Buch, has shown that she can now as materially strengthen the zoological and botanical groundworks of the science, as in the days of Werner she was eminent in laying those mineralogical foundations which have been brought so near to perfection by the labours of several living men. So numerous in fact have been the contributions of German geologists, that I cannot permit myself to specify the names of individuals in a country which boasts so many who are treading closely in the steps of an Ehrenberg and a Rosè. As distinctly connected, however, with the objects of this Meeting, I must be permitted to state that the eminent botanist Goeppert, whose works, in combination with those of Adolphe Brongniart in France, have shed so much light on fossil plants, has

just sent to me, for communication to our Geological Section, the results of his latest inquiries into the formation of the coal of Silesia—results which will be the more interesting to Dr. Buckland and the geologists of England, because they are founded on data equally new and original. Italy has also to a great extent been presented to us in its true general geological facies, through the labours of Sismonda, Marmora, Pareto and others; whilst our kinsmen of the far West have so ably developed the structure of their respective States, that our countryman Lyell has informed us, that the excellent map which accompanies his work upon North America is simply the grouping together of data prepared by native State geologists, which he has paralleled with our well-known British types.

If then the astronomer has, to a vast extent, expounded the mechanism of the heavens; if lately, through the great telescope of our associate the Earl of Rosse, he has assigned a fixity and order to bodies which were previously viewed as mere *nebulae* floating in space, and has also inferred that the surface-cavities in our nearest neighbour of the planetary system are analogous to the volcanic apertures and depressions of the earth; the geologist, contributing data of another order to the great storehouse of natural knowledge, has determined, by absolute and tangible proofs, the precise manner in which our planet has been successively enveloped in divers cerements, each teeming with peculiar forms of distinct life, and has marked the revolutions which have interfered with these successive creations, from the earliest dawn of living things to the limits of the historic æra. In short, the fundamental steps gained in geology, since the early days of the British Association, are so remarkable and so numerous, that the time has now come for a second report upon the progress of this science, which may I trust be prepared for an approaching, if not for the next Meeting.

Intimately connected with these broad views of the progress of geology is the appearance of the first volume of a national work by Sir Henry De la Beche and his associates in the Geological Survey of Great Britain. Following, as it does, upon the issue of numerous detailed coloured maps and sections, which for beauty of execution and exactness of detail are unrivalled, I would specially direct your attention to this new volume as affording the clearest evidence that geology is now strictly brought within the pale of the fixed sciences. In it are found graphic descriptions of the strata in the South-West of England and South Wales, whose breadth and length are accurately measured, whose mineral changes are chemically analysed, and whose imbedded remains are compared and determined by competent palæontologists. The very statistics of the science are thus laid open, theory is made rigorously to depend on facts, and the processes and produce of foreign mines are compared with those of Britain.

When we know how intimately the Director-General of this Survey and his associates have been connected with the meetings of the British Association, and how they have freely discussed with us many parts of their researches—when we recollect that the geologist of Yorkshire, our invaluable Assistant General Secretary, around whom all our arrangements since our origin have turned, and to whom so much of our success is due, occupies his fitting place among these worthies—that Edward Forbes, who passed as it were from this Association to the *Ægean*, is the palæontologist of this Survey; and again when we reflect, that if this Association had not repaired to Glasgow, and there discovered the merits of the Survey of the Isle of Arran by Mr. Ramsay, that young geologist would never have become a valuable contributor to the volume under consideration—it is obvious from these statements alone, that the annual visits of our body to different parts of the Empire, by bringing together kindred spirits, and in testing the natural capacity of individuals, do most effectually advance science and benefit the British community.

Whilst considering these labours of the Government geologists, I shall now specially speak of those of Professor E. Forbes in the same volume, because he here makes himself doubly welcome, by bringing to us as it were upon the spot the living specimens of submarine creatures, which through the praiseworthy enthusiasm of Mr. McAndrew, one of our members, who fitted out a large yacht for natural-history researches, have been dredged up this summer by these naturalists from the southern coast, between the Land's End and Southampton. As a favourite yachting port like Southampton may, it is hoped, afford imitators, I point out with pleasure the liberal example of Mr. McAndrew, who not professing to describe the specimens he collects, has on this, as on former occasions, placed them in the hands of the members best qualified to do them justice, and is thus a substantial promoter of science.

The memoir of Edward Forbes in the Government Geological Survey to which however I would allude, is, in truth, an extension of his views respecting the causes of the present distribution of plants and animals in the British Isles, first made known at the last meeting of the British Association. As this author has not only shown the application of these ideas to the researches of the British Geological Survey, but also to the distribution of animals and plants over the whole earth, it is evident that these views, in great part original, will introduce a new class of inquiries into natural history, which will link it on more closely than ever to geology and geography. In short, this paper may be viewed as the first attempt to explain the *causes* of the zoological and botanical features of any region anciently in connexion. Among the new points which it contains, I will now only mention, that it



very ingeniously (and I think most satisfactorily) explains the origin of the peculiar features of the botany of Britain—the theory of the origin of Alpine Floras distributed far apart—the peculiarity of the zoology of Ireland as compared with that of England—the presence of the same species of marine animals on the coasts of America and Europe—the specialities of the marine zoology of the British seas called for by this Association—the past and present distribution of the great Mediterranean Flora ;—and lastly, it applies the knowledge we possess of the distribution of plants to the elucidation of the history of the superficial detritus, termed by geologists the “ Northern Drift.”

Amid the numerous subjects for reflection which the perusal of this memoir occasions, I must now restrict myself to two brief comments. First, to express my belief that even Humboldt himself, who has written so much and so admirably on Alpine floras, will admit that our associate's explanation of the origin of identity removes a great stumbling-block from the path of botanical geographers. Secondly, having myself for some years endeavoured to show, that the Alpine glacialists had erroneously applied their views, as founded on terrestrial phænomena, to large regions of Northern Europe, which must have been under the sea during the distribution of erratic blocks, gravel and boulders, I cannot but consider it a strong confirmation of that opinion when I find so sound a naturalist as Edward Forbes sustaining the same view by perfectly independent inferences concerning the migration of plants to isolated centres, and by a studious examination and comparison of all the sea shells associated with these transported materials. And if I mistake not, my friend Mr. Lyell will find in both the above points, strong evidences in support of his ingenious climatal theories.

Recent as the blocks and boulders to which I have alluded may seem to be, they were however accumulated under a glacial sea, whose bottom was first raised to produce that connexion between the continent and Britain, by which the land animals migrated from their parent East to our western climes ; a connexion that was afterwards broken through by the separation of our islands, and by the isolation in each of them of those terrestrial races which had been propagated to it. This latter inference was also, indeed, thoroughly sustained by the researches of Professor Owen, communicated to this Association ; first, in the generalization by which his report on the Extinct Mammals of Australia is terminated, and still more in detailed reference to our islands in his recently published work ‘ On the Extinct Fossil British Mammalia,’—a work which he has stated in his dedication originated at the call of the British Association. Professor Owen adds, indeed, greatly to the strength of our present Meeting, by acting as the President of one of our Sections, which having in its origin been exclusively occupied in

the study of Medicine, is now more peculiarly devoted to the cultivation of Physiology. Under such a leader I have a right to anticipate that this remodelled Section will exhibit evidences of fresh vigour, and will clearly define the vast progress that has been made in general and comparative anatomy since the days of Hunter and of Cuvier, for so large a part of which we are indebted to our eminent associate.

Assembled in a county which has the good fortune to have been illustrated by the attractive and pleasing history of the naturalist of Selborne, I am confident that our Fourth Section, to whose labours I would now specially advert, will yield a rich harvest, the more so as it is presided over by that great zoologist who has enriched the adjacent Museum of the Naval Hospital at Haslar with so many animals from various parts of the world, and has so arranged them as to render them objects well worthy of your notice. The report of Sir John Richardson in the last volume, on the Fishes of China, Japan and New Zealand, when coupled with his account in former volumes of the Fauna of North America, may be regarded as having completely remodelled our knowledge of the geographical distribution of fishes; first by affording the data, and next by explaining the causes through which a community of ichthyological characters is in some regions widely spread, and in others restricted to limited areas. We now know, that just as the lofty mountain is the barrier which separates different animals and plants, as well as peculiar varieties of man, so the deepest seas are limits which peremptorily check the wide diffusion of certain genera and species of fishes; whilst the interspersions of numerous islands, and still more the continuance of lands throughout an ocean, ensures the distribution of similar forms over many degrees of latitude and longitude.

The general study, indeed, both of zoology and botany has been singularly advanced by the labours of the Section of Natural History. I cannot have acted for many years as your General Secretary without observing, that by the spirit in which this Section has of late years been conducted, British naturalists have annually become more philosophical, and have given to their inquiries a more physiological character, and have more and more studied the higher questions of structure, laws and distribution. This cheering result has mainly arisen from the personal intimacy brought about among various individuals, who, living at great distances from each other, were previously never congregated; and from the mutual encouragement imparted by their interchange of views and their comparisons of specimens. Many active British naturalists have in fact risen up since these Meetings commenced, and many (in addition to the examples already alluded to) have pursued their science directly under the encouragement we have given them. The combination of the enthusiastic and philosophic spirit thus engendered

among the naturalists has given popularity to their department of science, and this Section, assuming an importance to which during our earliest Meetings it could show comparatively slender claims, has vigorously revived the study of natural history, and among other proofs of it, has given rise to that excellent publishing body the Ray Society, which holds its anniversary during our sittings. Any analysis of the numerous original and valuable reports and memoirs on botanical and zoological subjects which have enriched our volumes is forbidden by the limits of this address, but I cannot omit to advert to the extensive success of Mr. H. Strickland's report on Zoological Nomenclature, which has been adopted and circulated by the naturalists of France, Germany and America, and also by those of Italy headed by the Prince of Canino. In each of these countries the code drawn up by the Association has been warmly welcomed, and through it we may look forward to the great advantage being gained, of the ultimate adoption of an uniform zoological nomenclature all over the globe.

Whilst investigations into the geographical distribution of animals and plants have occupied a large share of the attention of our Browns and our Darwins, it is pleasing to see that some of our members, chiefly connected with physical researches, are now bringing these data of natural history to bear upon climatology and physical geography. A committee of our naturalists, to whom the subject was referred, has published in our last volume an excellent series of instructions for the observation of the periodical phænomena of animals and plants, prepared by our foreign associate M. Quetelet, the Astronomer Royal of Belgium. Naturalists have long been collecting observations on the effects produced by the annual return of the seasons, but their various natural-history calendars being local, required comparison and concentration, as originally suggested by Linnæus. This has now for the first time been executed by the Belgian Astronomer, who following out a plan suggested by himself at our Plymouth Meeting, has brought together the contributions and suggestions of the naturalists of his own country. When M. Quetelet remarks, "that the phases of the smallest insect are bound up with the phases of the plant that nourishes it; that plant itself being in its gradual development the product, in some sort, of all anterior modifications of the soil and atmosphere," he compels the admission, that the study which should embrace all periodical phænomena, both diurnal and annual, would of itself form a science as extended as instructive.

Referring you to M. Quetelet's report for an explanation of the dependence of the vegetable and animal kingdoms on the meteorology and physics of the globe, and hoping that the simultaneous observations he inculcates will be followed up in Britain, I am glad to be able to announce, that the outline of a memoir on physical geography was some months ago put into my hands



by Mr. Cooley, which in a great degree coinciding with the system of M. Quetelet, has ultimately a very different object. M. Quetelet chiefly aims at investigating the dependence of organized bodies on inorganized matter, by observing the periodical phænomena of the former. Mr. Cooley seeks to obtain an acquaintance with the same phænomena for the sake of learning and registering comparative climate as an element of scientific agriculture. Speaking to you in a county which is so mainly dependent on the produce of the soil, I cannot have a more favourable opportunity for inculcating the value of the suggestions of this British geographer. The complete establishment of all the data of physical geography throughout the British Islands ; *i. e.* the registration of the mean and extremes of the temperature of the air and of the earth ; the amount of conduction, radiation, moisture and magnetism ; the succession of various phases of vegetation, &c. (with their several local corrections for elevation and aspect), must certainly prove conducive to the interests of science, and are likely to promote some material interests of our country.

A minute knowledge of all the circumstances of climate cannot but be of importance to those whose industry only succeeds through the cooperation of nature, and it may therefore be inferred that such a report as that with which I trust Mr. Cooley will favour us, if followed up by full and complete tables, will prove to be a most useful public document. Imbibing the ardour of that author, I might almost hope that such researches in physical geography may enable us to define, in the language of the poet,

*"Et quid quæque ferat regio, et quid quæque recuset."*

At all events, such a report will tend to raise physical geography in Britain towards the level it has attained in Prussia under the ægis of Humboldt and Ritter, and by the beautiful maps of Berghaus.

Though our countryman, Mr. Keith Johnston, is reproducing, in attractive forms, the comparative maps of the last-mentioned Prussian author, much indeed still remains to be done in Britain, to place the study of physical geography on a basis worthy of this great exploring and colonizing nation ; and as one of the highly useful elementary aids to the training of the youthful mind to acquire a right perception of the science. I commend the spirited project of a French geographer, M. Guérin, to establish in London a georama of vast size which shall teach by strong external relief, the objects and details of which he will in the course of this week explain to the geographers present.

Reverting to æconomical views and the improvement of lands, I would remind our agricultural members, that as their great practical Society was founded on the model of the British Association, we hope they will always come to our Sections for the solution of any questions relating to their pursuits to which can be given a purely scientific answer. If they ask for the

explanation of the dependence of vegetation upon subsoil or soil, our geologists and botanists are ready to reply to them. Is it a query on the comparison of the relative value of instruments destined to economize labour, the mechanicians now present are capable of answering it. And if, above all, they ask us to solve their doubts respecting the qualities of soils and the results of their mixtures, or the effects of various manures upon them, our chemists are at hand. One department of our Institution is in fact styled the Section of Chemistry and Mineralogy, with their applications to Agriculture and the Arts, and is officered in part by the very men, Johnston, Daubeny and Playfair, to whom the agriculturists have in nearly all cases appealed. The first-mentioned of these was one of our earliest friends and founders; the second had the merit of standing by the British Association at its first meeting, and there inviting us to repair to that great University where he is so much respected, and where he is now steadily determining, by elaborate experiments, the dependence of many species of plants on soil, air and stimulus; whilst the third has already been alluded to as one of our best contributors.

If in reviewing our previous labours I have endeavoured to gain your attention by some incidental allusions to our present proceedings, I have yet to assure you, that the memoirs communicated to our Secretaries are sufficiently numerous to occupy our Sections during the ensuing week with all the vigour which has marked our opening day. Among the topics to which our assembling at Southampton gives peculiar interest, I may still say that if foreign and English geologists should find much to interest them in the Isle of Wight, the same island contains a field for a very curious joint discussion between the mathematicians and the geologists, with which I became acquainted in a previous visit to this place. It is a discovery by Colonel Colby, the Director of the Trigonometrical Survey, of the existence of a considerable attraction of the plumb-line to the south, at the trigonometrical station called Dunnose, on Shanklin Down. The details of this singular phænomenon, which has been verified by numerous observations with the best zenith sectors, will be laid before the Sections. In the meantime, we may well wonder that this low chalk range in the Isle of Wight should attract, in one parallel at least, with more than half the intensity of the high and crystalline mountain of Schiehallion in the Highlands of Scotland. Can those of our associates, who like Mr. Hopkins have entered the rich field of geological dynamics, explain this remarkable fact, either by the peculiar structure and distribution of the ridge of upheaved strata which runs as a back-bone from east to west through the island, or by referring it to dense plutonic masses of rock ranging beneath the surface along the line of displacement of the deposits?

Another local subject—one indeed of positive practical interest—that stands before us for discussion is, whether, by persevering in deepening the large shaft which they have sunk so deep into the chalk near this town, the inhabitants of Southampton may expect to be eventually repaid, like those of Paris, by a full supply of subterranean water, which shall rise to the surface of the low plateau on which the work has been undertaken? On no occasion, I must observe, could this town be furnished with a greater number of willing counsellors of divers nations, whose opinions will, it is hoped, be adequately valued by the city authorities. The question whether this work ought to be proceeded with or not, will however, I apprehend, be most effectively answered by those geologists who are best acquainted with the sections in the interior of this county, and with the levels at which the upper greensand and subcretaceous strata there crop out and receive the waters, which thence flow southwards beneath the whole body of chalk of the hills in the south of Hampshire.

Considering that we are now assembled in the neighbourhood of our great naval arsenal—that some of its functionaries, including the Admiral on the station, have honoured us with their support, and that, further, I am now speaking in a town whose magnificent new docks may compete with any for bold and successful engineering, I must say a few words on our naval architecture, the more so as we have here a very strong Mechanical Section, presided over by that eminent mechanician Professor Willis, assisted by that great dynamical mathematician Dr. Robinson, and that sound engineer George Rennie. Duly impressed with the vast national importance of this subject, and at the same time of its necessary dependence on mathematical principles, the British Association endeavoured in its earliest days to rouse attention to the state of ship-building in England, and to the history of its progress in France and other countries, through a memoir by the late Mr. G. Harvey. It was then contended, that notwithstanding the extreme perfection to which the internal mechanism of vessels had been brought, their external forms or lines, on which their sailing so much depends, were deficient as to adjustment by mathematical theory. Our associate Mr. Scott Russell has, as you know, ably developed this view. Experimenting upon the resistance of water, and ascertaining with precision the forms of vessels which would pass through it with the least resistance, and consequently with the greatest velocity, he has contributed a most valuable series of memoirs, accompanied by a great number of diagrams, to illustrate his opinions and to show the dependence of naval architecture on certain mathematical lines. Employed in the meantime by merchants on their own account, to plan the construction of sailing ships and steamers, Mr. Scott Russell has been so successful in combining theory with practice, that we must feel satisfied in having at different meetings helped him onwards by several money grants; our only



regret being, that our means should not have permitted us to publish the whole number of diagrams of the lines prepared by this ingenious author.

But however desirous to promote knowledge on this point, the men of science are far from wishing not to pay every deference to the skilful artificers of our wooden bulwarks, on account of their experience and practical acquaintance with subjects they have so long and so successfully handled. We are indeed fully aware, that the naval architects of the Government, who construct vessels carrying a great weight of metal and requiring much solidity and capacious stowage, have to solve many problems with which the owners of trading vessels or packets have little concern. All that we can wish for is, that our naval arsenals should contain schools or public boards of ship-building, in which there might be collected all the "constants of the art," in reference to capacity, displacement, stowage, velocity, pitching and rolling, masting, the effect of sails and the resistance of fluids. Having ourselves expended contributions to an extent which testify, at all events, our zeal in this matter, we are, I think, entitled to express a hope, that the data derived from practice by our eminent navigators may be effectively combined with the indications of sound theory prepared by approved cultivators of mathematical and mechanical science.

I cannot thus touch upon such useful subjects without saying, that our Statistical Section has been so well conducted by its former presidents, that its subjects, liable at all times to be diverted into moral considerations and thence into politics, have been invariably restricted to the branch of the science which deals in facts and numbers; and as no one individual has contributed more to the storehouse of such valuable knowledge than Mr. George Porter (as evidenced even by his report in our last volume), so may we believe that in this town, with which he is intimately connected, he will contribute to raise still higher the claims of the Section over which he is so well qualified to preside.

If in this discourse I have referred more largely to those branches of science which pertain to the general division of natural history, in which alone I can venture to judge of the progress which others are making, let me however say, that no member of this body can appreciate more highly than I do, the claims of the mathematical and experimental parts of philosophy, in which my friend Professor Baden Powell of Oxford, who supports me on this occasion as a Vice-President, has taken so distinguished a part. No one has witnessed with greater satisfaction the attendance at our former meetings of men from all parts of Europe the most eminent in these high pursuits. No one can more glory in having been an officer of this Association when it was honoured with the presence of its illustrious correspondent Bessel, than whom the world has never produced a more profound astronomer. If among his numerous splendid discoveries he furnished astronomers with

what they had so long and so ardently desired—a fixed and ascertained point in the immensity of space, beyond the limits of our own sidercal system, it is to Bessel, as I am assured by a contemporary worthy of him, that Englishmen owe a debt of gratitude for his elaborate discussion of the observations of their immortal Bradley, which, in his hands, became the base of modern astronomy.

Passing from this recollection, so proud yet so mournful to us all as friends and admirers of the deceased Prussian astronomer, can any one see with more delight than myself the brilliant concurrence at our present Meeting of naturalists, geologists, physiologists, ethnologists and statist, with mathematicians, astronomers, mechanicians, and experimental philosophers in physics and in chemistry? Surely then I may be allowed to signalize a particular ground of gratification among so many, in the presence at this Meeting of two individuals in our Experimental Sections, to one of whom, our eminent foreign associate Oersted, we owe the first great link between electric and magnetic phenomena, by showing the magnetic properties of the galvanic current; whilst the other, our own Faraday, among other new and great truths which have raised the character of English science throughout the world, obtained the converse proof by evoking electricity out of magnets. And if it be not given to the geologist whom you have honoured with this chair, to explain how such arcana have been revealed, still, as a worshiper in the outer portico of the temple of physical science, he may be permitted to picture to himself the delight which the Danish philosopher must have felt when on returning to our shores, after an absence of a quarter of a century, he found that the grand train of discovery of which he is the progenitor had just received its crowning accession in England from his former disciple, who, through a long and brilliant series of investigations peculiarly his own, has shown that magnetic or dia-magnetic forces are distributed throughout all nature.

And thus shall we continue to be a true British Association, with cosmopolite connexions, so long as we have among us eminent men to attract such foreign contemporaries to our shores. If then at the last assembly we experienced the good effects which flowed from a concentration of profound mathematicians and magneticians, drawn together from different European kingdoms—if then also the man\* of solid learning, who then represented the United States of America, and who is now worthily presiding over the Cambridge University of his native soil, spoke to us with chastened eloquence of the benefits our Institution was conferring on mankind; let us rejoice that this Meeting is honoured by the presence of foreign philosophers as distinguished as those of any former year.

Let us rejoice that we have now among us men of science from Denmark,

\* Mr. Everett.

Sweden, Russia, Prussia, Switzerland, Belgium, Italy and France. The King of Denmark, himself personally distinguished for his acquaintance with several branches of natural history, and a warm patron of science, has honoured us by sending hither, not only the great discoverer Oersted, who evincing fresh vigour in his mature age brings with him new communications on physical science, but also my valued friend, the able geologist and chemist Forchhammer, who has produced the first geological map of Denmark, and who has presented to us a lucid memoir on the influence exercised by marine plants on the formation of ancient crystalline rocks, on the present sea and on agriculture.

As these eminent men of the North received me as the General Secretary of the British Association with their wonted cordiality at the last Scandinavian Assembly, I trust we may convince them that the sentiment is reciprocal, and that Englishmen are nearly akin to them in the virtues of friendship and hospitality which so distinguish the dwellers within the circle of Odin.

Still adverting to Scandinavia, we see here a deputy from the country of Linnaeus in the person of Professor Svanberg, a successful young experimenter in physics, who represents his great master Berzelius—that profound chemist and leader of the science of the North of Europe, who established on a firm basis the laws of atomic weights and definite proportions, and who has personally assured me, that if our Meeting had not been fixed in the month of September, when the agriculturists of Sweden assemble at Stockholm, he would assuredly have repaired to us. And if the same cause has prevented Nilsson from coming hither, and has abstracted Retzius from us (who was till within these few days in England), I cannot mention these distinguished men, who earnestly desired to be present, without expressing the hope, that the memoirs they communicate to us may give such additional support to our British ethnologists as will enable this new branch of science, which investigates the origin of races and languages, to take the prominent place in our assemblies to which it is justly entitled.

The Royal Academy of Berlin, whose deputies on former occasions have been an Ehrenberg, a Buch, and an Erman, has honoured us by sending hither M. Heinrich Rosé, whose work on chemical analysis is a text-book even for the most learned chemists in every country; and whilst his researches on the constitution of minerals, like those of his eminent brother Gustave on their form, have obtained for him so high a reputation, he now brings to us the description of a new metal which he has discovered in the Tantalite of Bavaria.

Switzerland has again given to us that great master in palæontology, Agassiz, who has put arms into the hands of British geologists with which they have conquered vast regions, and who now on his road to new glories in America,



brings to us his report on the fossil fishes of the basin of London, which will, he assures me, exceed in size all that he has ever written on ichthyolites. From the same country we have our friend Prof. Schönbein, who, in addition to his report on Ozone, to which I have already referred, has now brought to us a discovery of vast practical importance. The "gun-cotton" of Schönbein, the powers of which he will exhibit to his colleagues, is an explosive substance, which, exercising a stronger projectile force than gunpowder, is stated to possess the great advantages over it of producing little or no smoke or noise, and of scarcely soiling fire-arms; whilst no amount of wet injures this new substance, which is as serviceable after being dried as in its first condition. The mere mention of these properties, to which our associate lays claim for his new material, is sufficient to show its extraordinary value in all warlike affairs, as also in every sort of subterranean blasting.

Professor Matteucci of Modena, who joined us at the York meeting, and then explained his various new and delicate investigations in electro-physiology, again favours us with a visit, as the representative of the Italian Philosophical Society of Modena and of the University of Pisa. This ingenious philosopher, who has measured the effect of galvanic currents in exciting through the nerves mechanical force in the muscles, doubtless brings with him such interesting contribution as will add great additional interest to the proceedings of the Physiological Section.

Having already spoken of the rapid progress which the sciences are making in Belgium, through the labours of our associate Quetelet and others, it is with pleasure I announce that M. de Koningk, the palæontologist, who has mainly contributed to this advance and to the solid foundation of the geology of his country by his excellent work on palæozoic fossils, has been sent to us by his own government.

Among these sources of just pride and gratification, no one has afforded me sincerer pleasure than to welcome hither the undaunted Siberian explorer, Professor von Middendorf. Deeply impressed as I am with the estimation in which science is held by the illustrious ruler of the empire of Russia, I cannot but hope that the presence of this traveller, so singularly distinguished for his enterprising exploits, may meet with a friend in every Englishman who is acquainted with the arduous nature of his travels. To traverse Siberia from south to north and from west to east; to reach by land the extreme northern headland of Taimyr; to teach us, for the first time, that even to the latitude of  $72^{\circ}$  north, trees with stems extend themselves in that meridian; that crops of rye, more abundant than in his native Livonia, grow beyond Yakutsk, on the surface of that frozen subsoil, the intensity and measure of cold in which he has determined by thermometric experiments; to explain, through their language and physical form, the origin of tribes now far removed from their parent stock; to explore the far eastern regions of

the Sea of Okhotsk and of the Shantar Isles; to define the remotest north-eastern boundary between China and Russia; and finally to enrich St. Petersburg with the natural productions, both fossil and recent, of all these wild and untrodden lands, are the exploits for which the Royal Geographical Society of London has, at its last meeting, conferred its Gold Victoria Medal on this most successful explorer. Professor Middendorf now visits us to converse with our naturalists most able to assist him, and to inspect our museums, in which, by comparison, he can best determine the value of specific characters before he completes the description of his copious accumulations; and I trust that during his stay in England he will be treated with as much true hospitality as I have myself received at the hands of his kind countrymen.

It is impossible for me to make this allusion to the Russian empire, without assuring you that our allies in science on the Neva, who have previously sent to us a Jacobi and a Kupffer, are warmly desirous of continuing their good connexion with us. It was indeed a source of great pleasure to me to have recently had personal intercourse in this very town with that eminent scientific navigator Admiral Lütke, in whose squadron His Imperial Highness the Grand Duke Constantine was acquiring a knowledge of his maritime duties. Besides the narrative of his former voyages, Lütke has since published an account of the periodical tides in the Great Northern Ocean and in the Glacial Sea, which I have reason to think is little known in this country. Having since established a *hypsalographie* in the White Sea, and being also occupied from time to time in observations in Behring's Straits, the Russians will soon be able to provide us with other important additions to our knowledge of this subject. Separated so widely as Admiral Lütke and Dr. Whewell are from each other, it is pleasing to see, that the very recommendation which the last-mentioned distinguished philosopher of the tides has recently suggested to me, as a subject to be encouraged by this Association, has been zealously advocated by the former. Let us hope then that this Meeting will not pass away without powerfully recommending to our own Government, as well as to that of His Imperial Majesty, that a systematic and simultaneous investigation of the tides in the Great Ocean, particularly in the Northern Pacific, be the object of special expeditions,—a subject (as Admiral Lütke well observes) which is not less worthy of the attention of great scientific bodies than the present inquiries into terrestrial magnetism; and one which, I may add, this Association will doubtless warmly espouse, since it has such strong grounds for being satisfied with the results which it has already contributed to obtain through its own grants, and by the researches of several of its associates.

Lastly, in alluding to our foreign attendants, let us hope that our nearest neighbours may respond to our call, and, imitating the example of th

lightened monarch, may prove by their affluence to Southampton, that in the realms of science as in public affairs there is that "entente cordiale" between their great nation and our own, of which, at a former meeting, we were personally assured by the profound Arago himself.

No sooner was it made known that the Chair of Chemistry at this Meeting was to be filled by Michael Faraday, than a compeer worthy of him in the Academy of Sciences of Paris was announced in the person of M. Dumas. To this sound philosopher it is well known that we owe, not only the discovery of that law of substitution of types, which has so powerfully aided the progress of organic chemistry, but also the successful application of his science to the arts and useful purposes of life; his great work on that subject, 'La Chinnie appliquée aux Arts,' being as familiar in every manufactory in England as it is upon the Continent.

Nor, if we turn from chemistry to geology, can such of us as work among the rocks be backward in welcoming the French geologists who have come to examine, in our own natural sections of the Isle of Wight, the peculiar development of their Paris basin, the identity of their chalk and our own, the fine sections of our greensand and of the Wealden formation of Mantell, and to determine with us *in situ* the strict relations of their Neocomian rocks with those peculiar strata which at Atherfield, in the Isle of Wight, have been so admirably illustrated by Dr. Fitton and other native geologists, and of which such beautiful and accurate diagrams have been prepared by Captain Ibbetson.

It is utterly impossible that such gatherings together of foreign philosophers with our own should not be productive of much advantage; for he must indeed be a bad statist in science who knows not that numerous are the works of merit which are published in periodicals, or in the volumes of societies of one country, which remain altogether unknown in another; and still less can he be acquainted with the present accelerated march of science, who is not aware that the germs of discovery which are lying ready in the minds of distant contemporaries must often be brought into action by such an interchange of thought. The collision of such thoughts may indeed be compared to the agency of the electric telegraph of our Wheatstone, which concentrates knowledge from afar, and at once unites the extremities of kingdoms in a common circle of intelligence.

But although the distinguished foreigners to whom I have adverted, and others, including our welcome associate M. Wartmann, the Founder of the Vaudois Society, and M. Prevost of Geneva, on whose merits I would willingly dilate if time permitted it, are now collected around us; many, among whom I must name M. de Caumont, the President of the French Society for the Advancement of Science, have been prevented from ho-



nouring us with their presence, because the national meetings in their several countries also occur in the month of September. To remedy this inconvenience, I ventured, when addressing you six years ago at the Glasgow meeting, to express the hope, that each of the national European societies might be led to abstain during one year from assembling in its own country, for the purpose of repairing by its own deputies to a general congress, to be held at Frankfort or other central city under the presidency of the universal Humboldt. Had the preparation of the 'Cosmos' and other avocations of that renowned individual permitted him to accept this proposition, which I have every reason to believe the British Association would have supported, I am convinced that many benefits to science would have resulted, and that each national body, on re-assembling the following year in its native land, would have more vigorously resumed its researches.

Adhering still to my project, I beg my countrymen and their foreign friends now present, to sustain this proposition for centralizing in a future year the representatives of the various branches of science of different countries, when they may at once learn the national progresses respectively made, and when, at all events, they can so appoint the periods of their national assemblies as to prevent those simultaneous meetings in France, Germany, Scandinavia, Italy, Switzerland and England, which are so much to be deprecated as interfering with a mutual intercourse.

Finally, my fellow-labourers in science, if by our united exertions we have done and are doing good public service, let me revert once more to the place in which we are assembled, and express on your part the gratification I know you experience in being on this occasion as well supported by the noblemen, clergymen, and landed proprietors around Southampton, as by its inhabitants themselves—an union which thus testifies that the British Association embraces all parties and all classes of men.

Seeing around me Her Majesty's Secretary of State for Foreign Affairs, the Speaker of the House of Commons, and several persons of high station and great influence, who willingly indicate by their presence the sense they entertain of the value of our conferences and researches, let us welcome these distinguished individuals, as living evidences of that good opinion of our countrymen, the possession of which will cheer us onward in our career. And above all, let us cherish the recollection of this Southampton Meeting, which will be rendered memorable in our annals by the presence of the illustrious Consort of our beloved Sovereign, who participating in our pursuits, in many of which His Royal Highness is so well versed, thus demonstrates that our Association is truly national, and enjoys the most general and effectual support throughout British society, from the humblest cultivators of science to the highest personages in the realm.

*From the "Christian Examiner" for March, 1851.*

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THE  
ASTRONOMICAL OBSERVATORY  
OF  
H A R V A R D U N I V E R S I T Y.

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BY WILLIAM MITCHELL.

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IN all ages of the world, and in all countries with which we are conversant, the firmament has been contemplated with awe. "Day unto day uttereth speech, and night unto night showeth knowledge; there is no speech nor language where their voice is not heard." The handiwork of Omnipotence is recognized by the savage and the sage, — the shepherds of Asia and the *savans* of Europe.

The vast accessions to our stock of astronomical knowledge which have distinguished the last half-century, and the deep and abiding interest which these acquirements have inspired on this side of the Atlantic, have necessarily given to institutions, strictly astronomical, a position and a value, to which a just view of their importance, in most of the great concerns of life, has at all times entitled them. In tracing the progress of American discoveries, we can scarcely persuade ourselves that only twenty-five years have elapsed since President John Quincy Adams, in his first annual message to Congress, urged upon that most unscientific body the establishment of a national observatory. At that period, as was stated by the President, while on the comparatively small ter-

ritorial surface of Europe there were one hundred and thirty observatories, there was not one in our whole country; important as was the science to the growing commerce of the country, "year after year the earth rolled on in perpetual darkness to the unsearching eyes of one half of the globe." Still more difficult it is to believe, that this proposition, fraught as it was with the characteristic ardor of that illustrious patron of science, met only with the ridicule of the country. Length of days, however, enabled the veteran to sweep the heavens with his own eye, in his own State, by means vying with those of all Europe.

The establishment of an astronomical observatory connected with Harvard University was an early proposition of the late Dr. Bowditch; and a committee, consisting of himself and Professor Farrar, was appointed in the year 1816, to procure instruments in view of the immediate erection of a suitable building. The present Director of the Observatory, being about to visit England, was requested by the Corporation of the College to examine some observatories in that country, and to obtain plans and estimates adapted to the wants of the institution. This was done, and the result was reported to the College government; but, it being impossible to secure the services of the first workmen in Europe, the whole matter was permitted to rest. In 1823, and again in 1825, President Adams, then Secretary of State, urged upon the Corporation the erection of a building, even though instruments could not be immediately procured; and, to promote this end, he proposed at both these periods to subscribe one thousand dollars himself, provided the requisite sum could be raised;\* but, the attempt proving ineffectual, no further action was had till the autumn of 1839. To astronomers, William C. Bond, Esq. of Dorchester, had been long favorably known as a skilful and diligent observer. With a few instruments, in a retired but beautiful position, "the world forgetting," he spent much of the night in observing and collecting celestial phenomena; and, though

\* President Quincy's History of Harvard University, vol. ii. p. 567.



closely occupied during the day in an arduous calling in the city of Boston, no eclipse or occultation escaped his attention. The authorities at Washington had secured his services in a series of astronomical and magnetic observations, corresponding with those which were to be made by the Exploring Expedition. These, with only a trifling remuneration, aided by a much-lamented son, he was prosecuting with unparalleled zeal, when the Corporation of the College, at the suggestion of President Quincy, proposed to him, with the consent of the United States government, to transfer all his instruments and apparatus to Cambridge. He yielded to this plan without prospect of pecuniary reward; and, though a mere fraction of the observations made here and at Dorchester has been published, the volumes of the Transactions of the American Academy of Arts and Sciences bear testimony to an unexampled amount of labor. His hopes, however, rested upon the prospect, which he has since happily realized, of having at some day the management of such instruments as his gifts as an observer and skill in the adjustment of instruments justly entitled him to enjoy, and which would render him useful to science. A convenient house was procured near the Colleges, and smaller buildings were erected on the surrounding grounds for the transit and magnetic observations. The instruments conveyed from Dorchester, the property of the observer, consisted of an excellent four-foot transit, still in good condition, an altitude and azimuth instrument, one or two achromatics, a Gauss declination magnetometer, a magnetic dip circle, a sidereal clock, besides chronometers, and the usual meteorological instruments. In addition to these, the American Academy had purchased a set of Lloyd's magnetic apparatus, and placed it at the disposal of W. C. Bond. Thus equipped, the course of observations consisted of the transit of stars for time and the correction of instrumental errors, moon culminations with moon-culminating stars carried through the entire lunations, occultations of stars by the moon, monthly term-day observations of Lloyd's declination instrument, and horizontal and vertical force magnetometer continued through the whole

twenty-four hours. The latter were made by the Gauss magnetometer, and sometimes in connection with Lloyd's. Metereological observations were constantly kept up, and a part of the time were made hourly. In the magnetic observations, the Messrs. Bond were occasionally assisted by Professors Peiree and Lovering; but the entire family of the Director were occasionally pressed into the service, though the chief aid was derived from the eldest son. This excellent youth devoted the brief period of his existence to the promotion of his father's wishes; and the amount of his labors and the accuracy of his results will be an enduring monument to his fidelity, as well as to his skill and application. Death deprived the father of the services of the son in November, 1842. His place has been admirably filled by his brother, George P. Bond, of whom we are soon to speak.

A part of the astronomical observations made at this establishment have been published monthly in the Proceedings of the American Academy. Copies of a greater number, however, are in the hands of Captain Wilkes, of the Exploring Expedition, and of Sears C. Walker, Esq. of the Coast Survey, to be used in the determination of the longitude of various stations on the Pacific and Atlantic coasts. Some of the magnetic observations are contained in the Memoirs of the Academy, and some have been published by Colonel Sabine; but much the larger portion remains unpublished.

Neither the Corporation of the College, nor the friends of astronomical science generally, were satisfied with the telescopes which had been placed at the disposal of Mr. Bond. Indeed, the College had hitherto furnished no instrument better than those removed from Dorechester, and even the contingent expenses of the establishment were paid from the personal resources of the Director. This was no less a source of uneasiness to the College than to the Observer, and at length it was concluded to erect a building suitable for the accommodation of an Equatorial Refractor of the largest class, and forthwith to order the instrument. This design was largely promoted by the appearance of the great comet of 1843. The limits of this article will not admit of a

detail of the steps that were taken to effect this desirable object. It is sufficient for our purpose, however, to say, what indeed is known to the friends of the Observatory, that the expense of the present institution was met by the united contributions of the College and of the American Academy, and by individuals, friends of science, citizens of Boston and its vicinity, desirous of securing the services of the present Director.

The building is beautifully situated on an eminence fifty feet above the level of the surrounding country, and distant three quarters of a mile in a north-west direction from the College buildings. The edifice, consisting hitherto of the centre building or tower surmounted by the dome which covers the Great Refractor, with one wing only, has presented an awkward appearance; but the other wing is now in progress, and, when completed, the whole will present an appropriate and imposing aspect. On entering the lower apartment of the Observatory proper, the stranger is surprised that his progress is arrested by a prodigious circular mass of masonry. This is the pier for the support of the Grand Equatorial, the great step-stone of the work. The reader will perceive the necessity of this solid base, when he calls to mind the fact, that the slightest tremor, when magnified by the power of the instrument, becomes of sufficient moment to vitiate entirely the delicate determinations of the observer. To obviate this difficulty, an excavation was first made twenty-six feet below the natural summit of the hill; and at the bottom of this was placed a coating of cement intermixed with coarse gravel ten feet in thickness, which, when hardened, formed an entire mass of great firmness. On this bed, the pier, composed of five hundred tons of large granite blocks, well fitted to each other and laid in cement, rises thirty-three feet to the upper surface of the floor of the dome. On the cap-stone of this rests, on three bearers, a solid granite tripod or pedestal, of eleven tons' weight, to the top of which is attached the Great Equatorial, which we hasten to describe, passing over most of the minor instruments of the Observatory. We have no hope, however, of doing any



thing like justice to this noble instrument, which, in connection with all the apparatus of the Observatory then in use, is described scientifically by Mr. W. C. Bond in a memoir communicated to the Academy in November, 1848.

The Great Refractor was made by Merz & Mahler, the successors of the celebrated Fraunhofer, at Munich. It is the largest refracting telescope in the world. When finished, it was thought by the makers to be the best; and, if we can judge of its merits by its performance, we must concur in their opinion. Its only possible rival is that at Pulkova, which it somewhat exceeds in effective aperture, — that at Cambridge having fourteen and ninety-five hundredths inches. Its focal length is twenty-two feet eight inches, and it is mounted equatorially on the German plan. It is furnished with eighteen eye-pieces. These consist of four annular micrometers, with powers as determined by the Director ranging from 103 to 373; five plain eye-pieces, with powers from 222 to 1,118; and nine spider-line micrometers, with powers from 141 to 2,004. The field-view of the latter is only a single minute of arc, somewhat less than a thirtieth of the moon's apparent diameter. The motion of the earth, which, with ordinary instruments, is constantly throwing the object out of the field, is here counteracted by clock-work, which communicates sidereal motion to the telescope. The defining excellence of this telescope is without example: with a power of two thousand, the *disks* of the satellites of Jupiter and that of Neptune have been well shown. With a power of eight hundred, stars have been separated, whose measured distance was only three tenths of a second. With this telescope the edge of Saturn's ring never disappears.

It reflects great credit upon the manufacturer, trifling as the fact may seem, that the packing of this instrument, with all its complicated and delicate machinery, was performed in such a manner as to secure its safety through the various modes of conveyance necessary in its journey from Munich. The granite pedestal, already alluded to, and to which the bed-plate of the equatorial mounting of the telescope is attached, was prepared previously to the arrival of the

telescope; and the mounting of the instrument, with all its equatorial and clock-work movements, attests the skill of the Director. Indeed, his mechanical gifts, and judgment in the adjustment of instruments of every description, have at all times given him an acknowledged advantage over most observers in this country and in Europe. His anxiety, during the long period of its manufacture, had impressed upon his mind's eye as perfect an image of every joint and screw and pinion as was subsequently impressed on his retina in the Observatory. The weight of the telescope, with its iron diaphragms and brass strengthening-rods, is upwards of three tons, and yet the friction is so successfully relieved by the judicious arrangement of wheels and counterpoises, that the finger of a child may change its direction. The improvements which the Director has made in the observing-chair must be gratifying to every one who has witnessed the awkward and painful twisting to which observers are usually subjected, especially in observations on zenith objects. It is ingeniously balanced by weights suspended by chains constructed in the manner of the *fusee* chain of a watch. It moves horizontally on rails of round inch-iron let into the floor of the dome; and the observer is enabled with perfect ease, at all times, without leaving his seat, or disturbing the chronometer which may lie beside him, to move the chair round on the railway, adjust his position in altitude, and change at pleasure the direction of the telescope.

Of the instruments of less magnitude, besides those brought from Dorchester, the most important are an excellent five-foot achromatic, mounted in a detached building; a comet-seeker by Merz, so fruitful in the hands of the younger Bond; and a transit-circle on the plan of that successfully used by Groombridge at Blackheath.

We come now to the results of the labor in the discoveries which have been made during the short period in which the instruments have been in working order. In discussing these, which we shall do briefly, it must be borne in mind, that the entire force employed in the Observatory, till within the last six months, has consisted of the Director, William C.

Bond, assisted by his son, George P. Bond. The latter, a graduate of the University, had already distinguished himself as a mathematician, and though young has communicated several learned memoirs to the American Academy and other institutions. Their first labors were necessarily directed to the minute determination of the latitude and the longitude of the Observatory. In doing this, the wide difference between the skill and labor requisite in the minute determination of these elements, and that employed by the navigator or the geographer, must be understood and appreciated. An approximate result is all that is ever obtained at sea, except it be by accident. Observations of this kind are deemed of no importance to the Observatory. The position of the Observatory is the starting-point in all future time, and to obtain it with sufficient accuracy is a work of magnitude. For their latitude, besides various other methods, they obtained three hundred prime-vertical observations; and for their longitude, the transit of six hundred moon-culminating stars, two hundred occultations of stars by the moon, and all the visible eclipses that have occurred in clear weather. Besides these, the Director has been engaged, the last two years, for the service and at the expense of the Coast Survey, in accumulating results from chronometers of his own and those belonging to the Cunard line of steamers for relative longitude; and, in order to obtain the best determination of local time at Liverpool, an arrangement was last year made with the Director of the new observatory of that city, who has obligingly taken charge of all the chronometers. The number of results hitherto made is one hundred and seventy-five by fifty chronometers in thirteen voyages. It may well be conceded that the data obtained by this variety of means, so multiplied, should entitle the Observatory to be considered the standard of longitude on this side of the Atlantic. Perhaps there is no spot on the whole face of the earth whose position is so accurately determined.

An immense labor has been involved in the examination of nebulae, as appears by the papers on this subject in the transactions of the American Academy. The well-known



REPORT  
ON A  
RAIL-WAY SUSPENSION BRIDGE  
ACROSS THE CONNECTICUT,  
AT MIDDLETOWN,  
WITH A PROPOSAL FOR ITS CONSTRUCTION,  
TO A  
COMMITTEE OF THE CITIZENS OF HARTFORD,  
BY  
CHARLES ELLET, JR.  
CIVIL ENGINEER.

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PHILADELPHIA :  
JOHN C. CLARK, PRINTER, 60 DOCK STREET.  
1848.



## PROPOSAL

*For the Construction of the Bridge described in  
the following Report.*

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THE bridge represented in the accompanying plans, and described in this report, is intended to possess strength sufficient for the passage of trains composed of twenty first class locomotive engines, and as many tenders, filling the bridge from end to end, and weighing in the aggregate 600 tons.  $= 2240 \times 600 = 1,344,000$

The flooring is to be placed 140 feet above the river, and the navigation left entirely unobstructed.

The bridge is to be upheld by 24 cables, composed each of 1000 strands of No. 10 iron wire.

The masonry is to be throughout well finished, and of a substantial and durable character.

The work is to be finished in two years from the time of its commencement, and to be proved, on its completion, by running over it trains of 600 tons, or greater weight.

I hereby offer to build such a bridge, in conformity with this report and the detailed plans by which it is illustrated—stipulating only for the privilege of introducing such modifications as will improve its character



—for the sum of \$300,000; the amount at which the cost is herein estimated.

The contract may prescribe the usual guards for the protection of the Company, if these are deemed sufficient; but if not, it may provide that no advances shall be made to me until the work is in a condition to permit locomotive engines of 15 tons weight to run safely over the bridge, at the highest attainable velocity; at which stage of its progress I shall be paid one-half of the whole cost, or \$150,000; the remainder to be held by the Company until the final completion of the edifice, in conformity with the plan herein set forth, and until the structure is proved to possess the promised capability.

I will also agree to build a bridge of 850 feet span on the same site, for the same gross sum, or \$300,000.

In the event that the first of these proposals is accepted, I will make a reasonable deposit in the hands of the Company, as an earnest of my intention to begin, and press the work forward, as rapidly as they can wish, to its completion.

CHARLES ELLET, Jr.

# REPORT

ON THE

## Proposed Suspension Bridge across the Connecticut,

AT MIDDLETOWN.

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THE question referred to me by a Committee of the citizens of Hartford, for a professional opinion and report, is—

The practicability and probable cost of constructing a Suspension Bridge competent for the passage of locomotive engines and trains over the Connecticut river at the “Narrows,” below Middletown; the flooring of the bridge being elevated high enough above the river to permit the passage of the tallest masts beneath it, and the whole structure to be of “a durable and substantial character, equal to the present and future wants of a great rail-way thoroughfare.”

The proposition to build such a bridge has originated in an Act of the Legislature of the State of Connecticut, authorizing the construction of a “draw-bridge,” on a low level, at a point a short distance above the wharves of Middletown, and some twenty miles below those of Hartford, from the effect of

which the citizens of Hartford anticipate great injury to their property and commerce.

The facts of the case are these:—

A bridge of some sort, across the Connecticut, is desirable for the accommodation of a rail-road intended to connect Boston and New York, which it is the wish of the projectors and proprietors to lay upon the best line that can be found between the two cities, so that the through-trips may be made in the least possible time. This best line would cross the Connecticut on a high level below Middletown; and one of the original plans of the road seems to have contemplated such a location. But, subsequently, this location was abandoned, and the right was asked for and obtained from the Legislature, to construct a bridge above Middletown, on a low level, and on a plan which completely stops off the navigation, excepting at a single point where provision is to be made for a draw.

In this state of things, a committee of the citizens of Hartford, actuated by a common and deep desire to protect the navigation of the stream, to which their city owes its original existence, and in a great degree its subsequent prosperity, have asked a professional opinion and estimate of the practicability and cost of constructing a SUSPENSION BRIDGE, of which the flooring shall be elevated high enough to permit the largest vessels to pass under it at full sail, and which shall so span the river as to leave its trade and current uninjured.

If this can be done, they represent that their pros-



perity will be unimpaired by the enterprise of their neighbours; the great public, for whose benefit this rail-way is intended, will be relieved of the necessity of making an ascent and descent, amounting together to 100 feet, in every trip over the line; and that by reducing the grades, a saving of time will be effected, and the public convenience and welfare, so far as they depend on the construction of the Air Line Road, will be promoted, without injury to them or complaint from any.

My views on a bridge of this character, and my estimate of its cost, are set forth in the following

## REPORT.

In presenting a design for a rail-way bridge over the Connecticut, I am aware of the difficulty which is almost always encountered in attempting to show the practicability of constructing a great work on a plan which has not yet been submitted, in all its features, to the test of practical experience.

In past times such an attempt could scarcely have hoped for success; for men then approached cautiously and by very slow steps, to the most important truths.

But it is our privilege to live in a period, and just here in an atmosphere, where all claim the right to judge of the feasibility of every useful project; and, fortunately for the present purpose, in a portion of this country where most persons are fitted by educa-

tion and intelligence to decide upon the facts and the testimony.

I approach my subject, therefore, with confidence; intending to draw no conclusion that does not rest on established facts, and is not legitimately deduced from premises that are apparent to the common sense of men. And I shall endeavour to use no argument more difficult of comprehension than that which is deduced from the assumption, that, if one wire will bear a weight of 1500 pounds, ten thousand such wires will sustain a weight of ten thousand times fifteen hundred pounds; that if one pound will produce a given depression in a single strand, it will require ten thousand pounds to produce an equal depression in ten thousand similar strands.

With this explanation, we may proceed to the examination of the plan of the work proposed.

#### GENERAL DESCRIPTION OF THE BRIDGE.

The plan which I have presented for this structure, represents a wire suspension bridge, of which the span, measured from centre to centre of the abutments or supporting towers, is 1050 feet.

A work equally safe, and, it is believed, in all respects as economical and advantageous, might be erected with a span of 850 feet; but it would involve the necessity of raising an abutment 140 feet high, and consume more time in its execution than the plan now presented.

*5.1.20 9/1/10*

nebula of Andromeda and that of Orion were fields of special labor. Both of these nebulae have interested astronomers from early times; that in Andromeda, long before the invention of the telescope. In September, 1847, very soon after the adjustment of the Great Refractor' was completed, an examination of this interesting object was commenced, when it was found to have an immense number of stars scattered over its surface, and seeming to have no connection with it. Fifteen hundred were found to be within its limits; but the most remarkable features, now for the first time presented to the human eye, were two narrow, dark bands, in which no deviation from perfect straightness could be detected, and scarcely any deviation from parallelism. These bands stretched quite across the field of vision and through the entire nebula in the direction of the longer axis. In view of the distance of this nebula assigned to it by Sir W. Herschel, the younger Bond has estimated the length of these bands to be twenty times the distance of Sirius from the solar system. These phenomena have been since observed by Lord Rosse, and made the subject of an address to the British Association.

With regard to the great nebula of Orion, the public are already aware that the observers at Cambridge discovered, very early after mounting the great telescope, that, in common with most nebulae, this also was composed of the blended light of an infinite number of stars clustering in obedience to some law, or in accidental juxtaposition. The Director has more recently subjected this beautiful nebula to rigorous scrutiny, and communicated his results in a learned memoir to the Academy, with a catalogue of the stars embraced in it and having no connection with its nebulosity. By means of this catalogue, and the maps and telescopic views which both observers have drawn, they have detected three new stars near the trapezium, and ascertained the curious fact, that one star in its neighborhood of the sixteenth magnitude is variable in its light; at its minimum entirely disappearing. Probably this is the only variable telescopic star known, and extends this curious property to a very distant region. Other



nebulæ and clusters have engaged the attention of the observers at Cambridge, and among their diagrams they have completed a map of every star steadily visible by the Great Refractor in the cluster in Hercules, with a view of ascertaining, at a distant day, their relative motions and configuration.

At different periods through the years 1847 and 1848, laborious observations were made upon the satellite of Neptune near the time of its greatest elongation, for the determination of its mean distance from the primary, in view of ascertaining the mass of Neptune as well as the orbit of the satellite. And although the results of Professor Peirce, as derived from these observations when compared with those of Professor Struve, have been the subject of criticism, their close agreement, when we consider the delicacy of these measures upon these exceedingly minute and immensely distant objects, may be placed, as it has been by Professor Pierce, among the wonders of modern astronomical observations.

On the 16th day of September, 1848, the younger Bond discovered a point of light, resembling a star of the seventeenth magnitude, in the plane of Saturn's ring, between two of the well-known satellites of that planet. He entered this upon his diagram of stars and satellites at the time in that region. On the 18th it was seen by both the observers, and by both recorded with expressions of doubt as to its true character. On the 19th their micrometrical determinations indicated that it partook of the retrograde motion of Saturn, and no doubt remained that this object was a satellite of Saturn hitherto unknown to the world. It is very remarkable that this discovery should also have been made by Mr. Lassell of Liverpool only two days later; and we deem it quite as remarkable, and a matter of surprise, that the English astronomers claim the honor of this discovery. If the question be asked, Who saw it first? the answer from all parties must be, George P. Bond; and if it be asked, Who saw it next? the answer must be, William C. Bond; but the clew on which the British astronomers rest their claim is, that Lassell made a map of its position relative to the other

satellites on the 18th. But the Bonds made "careful measurements" on the same day. Who ever thought of withholding from Sir William Herschel the credit of the discovery of Uranus, because he at first supposed it to be a comet, and because it was reserved for another observer to detect its true character many months afterwards? The editor of the London Athenæum, who never forgets the claims of England, maintains that there was no priority in either observer as to the first suspicion that the object was a satellite, and plausibly recommends that the English say it was discovered by Bond and Lassell; the Americans, by Lassell and Bond. We do not assent to this. Bond saw the object and mapped it on the 16th; and both observers detected its true character on the 19th.

A multitude of observations have been made upon the changes in the belts of the planet Jupiter. These were taken during the years 1848 and 1849, when the planet was favorably seen in high northern declination. Changes in these and in the relative brightness of the satellites have been mapped out by the observers, with explanatory notes, exhibiting very interesting phenomena. In the prosecution of this inquiry, the elder Bond on one occasion saw an eclipse of the first satellite in the shadow of the third, both satellites being off the planet, and both shadows on; a circumstance necessarily of rare occurrence, and probably never before seen.

Drawings of the solar spots, which were observed on every clear day through the apparent annual revolution of the sun, have been made at the Observatory, and, when collected with the notes and explanations which accompany them, will furnish new data for the determination of the period of his rotation, and will contribute also to an explanation of those mysterious appearances.

Very valuable observations for the determination of the sun's parallax were made on the planet Mars in November and December, 1849, and January, 1850, during the opposition of the planet. Its position relative to the best situated fixed stars within the range of the micrometer of the Great Refractor was carefully measured every morning and even-

ing. By allowing for the motion of the planet in the interval between the morning and evening measurement, they obtain the sum of its parallaxes, east and west, a quantity two or three times larger than the sun's parallax, which they propose to obtain from it. This method has been aforetime practised for the determination of the parallax of a comet while circumpolar; but never, we believe, for that of the sun. It is plain that, by taking advantage of the earth's rotation to carry them from one extremity to the other of a chord of about five thousand miles, they obtain the parallax of Mars as effectually as by the removal of the telescope to an equally distant point of the earth. For this class of observations, nothing can be more opportune than the electric clock, aided by the spring governor, a late invention of the Director, of which we have yet to speak. By means of this, they can reckon on two or three thousand measurements for a night and morning's work of two hours each. Such a set of determinations, thus multiplied, will afford them as accurate a determination of the sun's parallax as can be obtained by a transit of Venus, and may be repeated as often as desired.

Eleven comets had been discovered by the Assistant Observer, George P. Bond, before receiving any intelligence of their having been seen elsewhere. Nine of these were strictly telescopic,—a greater number, it is believed, than has ever been discovered by any unassisted individual, except the celebrated Messier. With some of these, as with the satellite of Saturn, the European observations were nearly simultaneous; indeed, the comet of June 3, 1845, and that of April 11, 1849, were both discovered here and in Europe on the same day and at the same hour of local time, the priority being only equivalent to the difference of longitude. The first of these, which has been claimed by Professor Colla of Parma, and distinguished by his name, is another instance of European injustice. Both observers saw it on the morning of June 3d, civil reckoning. Colla obtained no observation of its place, merely stating it was in Perseus, and no European observations were made earlier than on the 7th; but the Bonds had good places on the 2d, 4th, and



6th, astronomical time, and it was subsequently proved that a Southern gentleman of this country saw it on the last day of the previous month. And yet this comet is called Colla's comet throughout Europe, and the Professor has claimed, and it is supposed obtained, the medal of the king of Denmark, although beyond question this was an American discovery. Stricter justice, however, is done in reference to the discovery of the comet of the 29th of August of last year. The priority of George P. Bond is acknowledged in Europe, and this comet is distinguished by his name.

Besides the elements of the comets discovered by himself, this young man has calculated those of twenty other comets, as well as the orbit of Neptune and that of the new satellite of Saturn. Those only who have performed this operation can be sensible either of its labor or intricacy; and to those who are entirely familiar with the methods, the great liability to errors which are fatal to the results renders it at least a very perplexing problem.

We come now to the discovery of the new ring of Saturn, one of the greatest discoveries of the present age, and the highest proof of the excellence of the Great Refractor. During the last autumn, Saturn being favorably situated, the observers were perplexed with an appearance connected with this planet which was entirely new. This was a dark line bordering the inner edge of the ring projected with the shadow of the ring upon the body of the planet. At first they supposed this phenomenon had some connection with the shadow; but it could be traced on some occasions throughout the entire circumference of the ring, and on the inner anser of the old ring presented an edging of faint light. Suspecting its true character, the question remained unsettled till the beautiful night of November 15th. It was quite calm, the sky being just hazed over with thin cirrus. Saturn was on the meridian, and was probably never before so well seen. All doubt of the existence of a ring interior to any hitherto known was at once removed. The younger Bond has prepared a faithful drawing of its appearance on that occasion, it being exceedingly rare that an opportunity so favorable

occurs. It has been intimated that the Cambridge observers have been anticipated in this discovery by several astronomers; but it is not so. The mistake originates in confounding a plurality of divisions of the old ring (which is all they profess to have seen) with the new ring. Encke's article in the *Astronomische Nachrichten*, No. 338, has been cited as anticipating the Cambridge discovery; but it contains not a word about a new ring inside of the old one. It simply intimates, what has been several times done, that there are glimpses of divisions in the old rings. He puts the inner diameter of the old ring at  $26''.76$  at Saturn's mean distance, agreeing precisely with the Cambridge measurement, and also with that of Professor Struve. Now the diameter of the inside of the new ring is only  $23''.3$ . To suppose an error of this magnitude is absurd. The breadth of the new ring is somewhat less than that of the outer of the two old rings. Its light is very much fainter, an interesting peculiarity; and hence it is that in crossing the bright planet it is distinctly visible as an exceedingly narrow dark line.

It may be asked why these discoveries were not before made. To satisfy this inquiry, it is sufficient to say, that at no time, since the mounting of the Great Equatorial, had the earth been so favorably situated, in reference to the plane of Saturn's ring, as when these discoveries were made.

We have spoken of the application of the "Spring Governor," an invention of the Director, and for which he has received the gold medal of the Massachusetts Mechanic Association. It was made in the Observatory under the eye of the Director, and owes much of its mechanical excellence to the skill of his son, Richard Bond. The importance of this instrument, in faithfully recording observations communicated by electro-magnetism, cannot be spoken of in exaggerated terms.

Magnetic wires, connected with the telegraph lines, and corresponding with the principal cities of the United States, had been brought into the transit building at the request of the Superintendent of the Coast Survey; but the confusion in the second marks made it nearly hopeless to expect any

thing from the method of observing by electro-magnetism beyond a few experiments, as the difficulty of reading off the observations was more than the labor of obtaining them. This difficulty the Spring Governor has entirely overcome. It is a system of clock-work, regulating the rotary motion of a cylinder in such a manner that its revolutions shall be performed in a given time. The cylinder is of wood, with paper drawn smoothly over it. An hour's observations are recorded on a single sheet; and, when removed from the cylinder, the minutes and seconds appear entered in regular horizontal and vertical columns, and may be read off by the eye without the slightest danger of confusion or inaccuracy. After the necessary preparation in the Director's office or elsewhere, the observer repairs to the dome, unattended if he please, adjusts the telescope to the position of a star night or day; in a moment, the object, by the motion of the earth, enters the field of view, and approaches the vertical wires in the focus of the telescope. At the instant the star passes the wire (his finger being previously placed upon the *break-circuit key*, attached to the observing-chair), he suddenly presses downward; and, this simple movement being repeated at the transit of each wire, he returns to the office, and the minute, second, and part of a second of each event are there recorded as by magic. It is not important that the recording cylinder should be near, so long as the connection is perfect. Theoretically, it may be carried around the world, and practically to places quite remote.

In making this hasty sketch of the condition of this prosperous institution, we have passed over a multitude of observations and labors of less importance to science than those we have enumerated, yet not less necessary in the daily routine of duty; but we trust enough has been said to satisfy the generous contributors to this Observatory, that the best ends have been accomplished by their means; and we doubt not they will concur with us in the opinion, that, if we except the discovery of the planet Neptune, — which, as a mere discovery by the telescope, claims but little credit, — if we except this, the original discoveries made at this Observatory



since its establishment are scarcely excelled by those of the whole world beside in the same period of time.

No one who is not familiar with the duties of an observatory can be sensible of the labor, the intense anxiety, the continual disappointments, watchings, and privations, to which the practical astronomer is subjected; and we know of no living man who has done so much drudgery for science, with so slight a reward, as William C. Bond. But a better day is dawning upon the father and the son. Edward Bromfield Phillips, a young man of fortune, a graduate of the University, a classmate and a friend of the younger Bond, died a few years since, leaving a bequest to the Observatory of one hundred thousand dollars, as a perpetual capital fund, the interest to be applied annually for the payment of the salary of the observers, or for instruments, or a library for the use of the Observatory, at the *discretion* of the Corporation of the College, who are made the trustees of the fund. It was an act of great discretion in this young man to place the funds in the control of persons who would be likely to be faithful in the execution of his wishes. With this provision, and with that countenance and sympathy of the officers of the College which they have always enjoyed, the observers at Cambridge can scarcely fail to enlarge the bounds of science, and render themselves useful to the world.

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The height of the upper side of the flooring, or grade line of the road, is 140 feet above the low water surface of the Connecticut,—leaving space sufficient between the water and the platform, for the passage of the tallest masts in all conditions of the river.

The width of the platform between the parapets is 20 feet,—sufficient to admit the future introduction of a double track rail-way, although the plan contemplates for the present but a single track.

The height of the eastern abutment will average about 65 feet; the rock on which it is founded rising at this point, 75 feet above low water.

The western abutment is about 10 feet in height, and rests on a substantial foundation of earth and gravel, at a height of 130 feet above low water.

The towers which support the cables rise 65 feet above the road-way, measuring from the grade line to the point of suspension; but the masonry is carried up 10 feet above the bearing point, for the sake of architectural effect.

The thickness of the towers at the base is 30 feet, in the direction of the axis of the bridge, and 70 feet transversely thereto. In the former direction, they are pierced with an arch-way 20 feet wide and 34 feet high, through which the engine and train enter upon the bridge.

The style of the architecture of the towers is Egyptian, and all the work is of massive proportions.

The flooring will be upheld by 24 wire cables, each

of which will contain 1000 strands of No. 10 iron wire.

These cables rest on cast iron saddles at the summits of the towers, which move on rollers calculated to yield with the contractions and expansions of the wire, due to atmospheric changes.

The deflection of the cables below the bearing points is 60 feet. Their length will be 1300 feet.

The weight of each lineal foot of the bridge between the bearing points of the cables is 4000 lbs.

I propose now to demonstrate that a bridge of these proportions, will possess strength and stability sufficient for the safe passage of locomotive engines and heavy freight trains, and be, in all respects, fully competent for the severest rail-road duties.

#### OF THE STRENGTH OF THE CABLES.

To the mind that has not reflected upon this subject, and is not familiar with works of this description, the proposal to suspend a bridge which is to be traversed by locomotive engines and heavy trains, on strands of wire, may appear to be exceedingly rash and hazardous.

The evidences of its practicability must therefore be set forth in more than the usual detail, and all the points on which doubts can be supposed to rest will need to be separately and distinctly examined.

In pursuing this course, it is to be proved—

1. That the cables will be strong enough to bear



the weight of the bridge itself, and the additional weight of the transitory loads that may pass over it:

2. That the flooring will be firm enough to resist the pressure of passing weights without injurious bending:

3. That there will be no dangerous or inconvenient horizontal oscillation, due either to the action of the wind or to any other probable cause:

4. That the cost will not exceed some reasonable limit, justified by the object for which the bridge is intended.

The first, and obviously the most important of these questions, after admitting the importance of the work, is that which determines the strength of the cables that uphold the bridge.

As already stated, these cables are composed each of 1000 strands of No. 10 iron wire, and there will be twenty-four of them, containing in the aggregate 24,000 strands.

A strand of No. 10 wire weighs the 1-20th of a pound for each lineal foot, and if sound and of good quality will bear about 1500 pounds avoirdupois before breaking. In the bridge it will sustain 500 pounds permanently and safely.

The absolute strength of one thousand strands—the number composing one of the cables—will therefore be 1,500,000 pounds, or 750 tons.

The aggregate strength of the twenty-four cables which uphold the bridge, will be 36,000,000 pounds, or 18,000 tons.

Experience teaches how we may multiply fibres of flax, or threads of the silkworm, until we obtain strands sufficient to form a cable that will bear the heaving of a ship of war; how we may multiply particles of clay or sand until we form a dyke that will sustain the pressure of the ocean; and it is by the same process of accumulating material and strength, that we can bring together strands of wire, until we are capable of supporting any load of matter that, in human affairs, can need to be resisted.

But it is not the intention, in the present case, to deal in mere generalities. The subject before us is a beautiful scientific problem, and must be treated with all the severity which is due to such questions.

It is stated as a fact, proved by my own repeated trials, and which it is in the power of every reader to test and confirm, that such a strand of wire as is here specified, will break with a load of about 1500 pounds. But to exhibit this tenacity, the wire must be formed from iron of good quality and properly drawn.

These cables of 1000 strands each will be about five inches in diameter, and will weigh respectively 65,000 pounds, or  $32\frac{1}{2}$  tons.

To compare the strength of one of these cables with that of more familiar objects, it may be observed that 200 pounds is considered to be beyond the force exerted by a draught horse in his daily work.

The strength of a single cable is, therefore, equal to the tug of 7500 horses, when drawing with united power; and the aggregate strength of the twenty-four

cables will be equal to that of about 180,000 horses, or a team extending over a line of some 500 miles.

But still such computations do not prove the sufficiency of the cables; they show only that they are strong in comparison with certain other things. We have yet to compare their power with that which will be needed to sustain the heavy timbers of this bridge, and superadded thereto, the action of a column of freight cars filling the platform of the bridge from end to end.

#### OF THE LOAD TO BE SUPPORTED BY THE CABLES.

The load which the cables of this bridge will be required to sustain, will consist of three distinct items, viz: their own proper weight, the weight of the flooring which they uphold, and that of the transitory loads which may come upon the flooring.

The weight of the cables is immediately deduced from the dimensions already given,  
and amounts to                    -                    - 1200 lbs. per lin. foot.

The weight of the timber in the flooring is determined from the plan, and is                    -                    - 2200                    „                    „

The weight of the suspenders and other iron work supported by the cables, is                    -                    - 150                    „                    „

To which is to be added for flagging or ballast,                    -                    - 450                    „                    „

Making for the total permanent load,                    -                    - 4000 lbs per lin. foot,



measured along the horizontal line connecting the points of suspension.

The length of the flooring is 1000 feet, and the total permanent weight of the suspended portion of the bridge is, in round numbers, 2000 tons.

In determining the weight of the transitory loads which may come upon the platform, a prudent economy would suggest the propriety of examining the character of the transportation likely to pass over the bridge.

It is estimated, in the official report of the Company, that the present tonnage along the line of the road which will cross the bridge, will  
amount to                -                -                -                7050 tons.

To which is added, in the report, 80  
per cent. for the conjectural increase,    -    5640    „

Producing for the probable future annual tonnage,                -                -                -                12,690 tons.  
exclusive of certain agricultural productions, live stock and lumber prepared for market.

This is given as the official estimate; but in preparing for the accommodation of the trade of any extensive district of country, it would be prudent to look forward to a much greater business than this, where the estimated amount is intended to control the proportions of any important structure.

If we assume 30,000 tons of freight per annum for the business that will pass over this bridge, in addition to the usual complement of passenger trains to be found on other successful lines, we will approximate

more nearly to what ought to be the future trade, to justify the construction of the rail-road.

If this amount of freight is carried over the bridge in 300 days, there will be an average daily movement of 50 tons in each direction, or more than quadruple the quantity estimated by the Company.

For the conveyance of this freight, we may allow 16 tons for the engine, 9 tons for the tender, 30 tons for the cars, and 50 tons for the lading, and we will obtain aggregate trains of 105 tons.

If the present purpose were to prepare a plan simply compatible with good economy, such is the calculation which it would seem advisable to make, in obtaining a business basis on which to compute the strength and cost of this structure.

But, in the case before us, it is deemed expedient to adopt a plan fully equal to all the present and probable future wants of a great rail-way; and we must therefore treat this part of the subject on a much broader ground.

In adjusting the dimensions of the bridge discussed in this report, it is assumed not merely that the flooring may be loaded with gross trains of 105 tons, but that it may be covered any number of times daily with a column, 1000 feet long, of loaded freight cars, filling the track from one abutment to the other, and moving at the ordinary speed of such trains.

Indeed, in computing the strength of the cables, a still more liberal view of the subject has been taken, and

the load upon the bridge has been assumed to consist of *a column of the first class locomotive engines*, each of 20 tons weight, and followed by their respective tenders, each tender being of 10 tons weight.

The flooring of the bridge would contain 20 such locomotive engines and 20 tenders, or gross loads of 600 tons.

Now it will be observed, that the present actual freight along the line of the proposed road, which will cross the Connecticut, is 7050 tons per annum, with the exclusion of certain commodities of little moment; and that it is only by assuming, in the first place, that all the trade is accommodated by a single daily train, and that the tonnage will be more than quadruple the estimated amount, that we can obtain average gross loads of 105 tons. And after this, for the sake of giving ample room, it is assumed, for the basis of the computation upon which the proportions of this bridge are adjusted, that the work must sustain gross loads six times as great as this, which is itself quadruple the official estimate of the actual traffic.

By assuming that the flooring is covered by a weight equal to one train of locomotive engines and tenders daily, in each direction, we prepare for accommodating a trade twenty-four times as great as the estimated traffic : and by running only two such trains daily, the bridge will be adequate to the passage of about fifty times the estimated tonnage.



If, now, to the permanent weight of the  
bridge, - - - - - 2000 tons,

We add the weight of the transitory loads, say 20 locomotives of 20 tons each, or - - - 400 tons,

And 20 tenders of 10 tons			
each, or	-	-	200 „
			<hr/> 600 „

We shall have for the total weight, 2600 tons, or 5200 pounds per lineal foot, to represent the load upon the cables.

Now, the cables must not only be strong enough to support this load, but they should be adequate to its support for centuries, with every reasonable chance of being unimpaired by the strain.

The load which these cables are to uphold, is, as above stated, 2600 tons; but this weight, drawing across the line of the curve, acts with a certain mechanical advantage, and produces a tension much greater than the absolute weight.

The methods of calculating this strain are entirely perfect, and may be relied on as practically and theoretically accurate.

The rule here laid down for computing its value may be submitted directly to experiment, and verified in the most conclusive way.

The tension is composed at each point of two parts—that which is vertical, and therefore just equal to the weight upheld at that point; and that which is

horizontal, and depending on the form of the curve assumed by the cables.

The vertical component at each point of suspension is half the weight of the bridge and load, or 1300 tons.

The horizontal component is equal to the vertical component multiplied by half the span in feet, and divided by twice the deflection of the cables in feet.

To obtain this value in tons, we will observe that half the span is 525 feet, and twice the deflection is 120 feet. The vertical component is 1300 tons. The horizontal component is, therefore,

$$\frac{1300 \times 525}{120} = 5687 \text{ tons.}$$

The resultant of the horizontal tension, 5687 tons, and the vertical weight, 1300 tons, is—

$$(5687^2 + 1300^2)^{\frac{1}{2}} = 5834 \text{ tons.}$$

This is the strain against which provision is to be made—no less than 5834 tons, or 11,668,000 pounds.

The strain produced by the weight of the bridge acting across the line of the cables, is, therefore, more than double the actual weight supported.

But it has been stated that each strand of sound No. 10 wire is capable of sustaining any tension less than 1500 pounds. There will, therefore, be needed to balance the draught on the cables produced by the weight of the bridge thus loaded,

$$\frac{11,668,000}{1500} = 7779 \text{ strands.}$$

But the proposed cables consist of no less than 24,000 strands, or more than three times the number really necessary to support the tension to which they will be subjected.

In other words, it is assumed that the bridge is loaded from end to end with 20 tons locomotive engines, and 10 tons tenders; and *the cables are then made strong enough to bear more than three times their own weight, three times the weight of the bridge, and three times the weight of twenty first class locomotive engines and twenty tenders.*

Again, let it be observed that these are the facts which control the estimate of the cost of this bridge: That estimate is made for a work intended to bear safely and permanently, freight trains of 600 tons; while it is very obvious that the road is destined almost exclusively for passengers and valuable merchandise.

An engine of twenty tons, with its tender and eight or ten passenger cars, or a gross load of 100 tons, will probably be regarded on this line, for a long time to come, as an extraordinary train; and indeed all that a sound view of judicious economy could well prescribe as the ground-work of the plans intended for its accommodation.

But it is the wish of the Committee to present a plan for a bridge of ample power for any rail-way duty in this country. This plan has been controlled by that wish. It will remain for the rail-way company, in carrying out the details, to reduce the work and the



cost down to what is really necessary for their purposes, leaving the extravagant excess to be provided for by posterity.

#### OF THE STIFFNESS OF THE BRIDGE.

It will hardly be doubted now, that the cables proposed for this bridge will possess ample strength to afford every reasonable assurance of their perfect safety. Nor will it be doubted, either, that such cables can be manufactured, since *they have been made* both of larger and smaller size. There is, in fact, no more difficulty in making and adjusting one of 5000 strands, than one of 500 strands.

But the fitness of the structure for the purposes in view does not rest solely on the strength of the supporting wire; for the bridge must not only be so strong that it shall not be broken down under the weight of the trains, but it must also be so firm that it will not be dangerously or injuriously bent under the engines which are to pass over it.

This division of the subject is fortunately susceptible of the same rigid analysis as that which has already been considered.

A suspended chain is flexible, and yields more or less to every weight that is applied to it; but it yields in accordance with certain laws, and the amount of its flexure may be calculated in ordinary cases with any necessary degree of precision.

These calculations are not speculative or conjectu-

ral, but absolute and certain. They depend on those established principles of mechanics which have been confirmed by the experience of centuries, and upon which all the computations of art equally rest.

There is no need of proving their accuracy here by any mathematical process, for the results can be stated in advance, and verified by experiment.

It is true that the observation and the calculation will not always agree precisely, with our uncertain means of measurement. The calculation is precise and positive, while the observation is only an approximation.

In a suspension bridge nicely constructed, and of which all the elements are known, the change of figure or depression of the central point, due to the weight of a single individual, or the variation of one degree in the temperature of the air, can be computed within a fraction of a hair's breadth.

The eye will not detect quantities so small, and they can only be rendered perceptible by a nicely adjusted instrument. But these motions take place, although unobserved, and are just as sensible to the computation as if they were produced by the weight of a locomotive engine.

In determining the effect of given forces in changing the figure of the flooring, there are three causes of movement which must be separately considered.

1. The depression caused by the alteration of the figure of the curve, the drawing up of the flanks, and the simultaneous sinking of the centre.

2. That due to the extensibility of the material—the elongation of the wire consequent on the additional tension produced by the additional weight.

3. The drawing up, or closer approximation to a straight line, of the stays or guys.

In ordinary cases, embracing the structure under consideration, the first of these three quantities is by far the greatest; so much the greatest, indeed, that the others are hardly worthy of consideration in comparison with this one.

It is also to be observed in these investigations, that there are two classes into which such questions may be divided, viz: those in which the concentrated weights producing the movement are small in comparison with that of the body put in motion, and consequently, in which the depression of the central point is also small in comparison with the primitive depression of the curve; and those in which these weights and movements are comparatively large.

In bridges of great span, as the proposed bridge over the Connecticut, the weight of an engine and tender is but some 30 tons, and the flexure of the curve, or the droop of the cables, in the ordinary condition of the structure, is many feet; while the weight of the bridge itself is 2000 tons, and the movements produced by the action of the heaviest engines will be usually but a few inches.

The questions which arise in this investigation belong, therefore, to the first of these classes, and the rules which will be laid down in this report, for de-

termining the value of these movements, are to be taken as applicable only to this class of such questions.

Further—but without meaning to confine the application of these rules strictly within the limits here given—it may be stated generally, that where the sagitta of the curve is not less than the twentieth part of the span, and where the weight placed in the centre of the arch, is not greater than the twentieth part of the weight of the suspended portion of the bridge, these rules may be applied with entire confidence. But if we transcend these limits, by making the moving weight materially greater, or the original flexure of the curve materially less than the proportions designated, the depressions which actually have place will vary appreciably from those indicated by the calculation.

One of the most important problems that will occur to an engineer, in arranging the plan of a bridge of this character, is to ascertain the bending of the flooring which will be produced by placing a given weight, such as a locomotive engine, in the centre of the arch.

In computing this depression, he will assume that the flooring possesses no rigidity whatever beyond that which is due to the simple weight of the material of which it is composed. In other words, that the framing offers no resistance, and that the timbers are as flexible as a chain of the same length and possessing the same weight.

The amount of this depression, under these condi-



tions, may be computed by the following simple process:—

Multiply the sagitta, or deflection of the curve of the cables in feet, by the weight in tons placed in the centre of the flooring, and divide the product by twice the weight of the bridge, in tons.

The result obtained by this process, will express the deflection in feet, under the supposition that the timber possesses no stiffness, and adds nothing at all to the strength or rigidity of the platform.

For an application of this rule, we may assume that a locomotive engine, of 20 tons weight, is placed in the centre of the flooring of the Connecticut river bridge.

The calculated deflection would then be

$$\frac{60 \times 20}{2 \times 2000} = \frac{1}{30} \text{ of a foot,}$$

or the flooring would be depressed three inches and six-tenths.

But the bridge is planned with a view to preserve a camber or arch in the flooring, of two feet, at midsummer, when unloaded; and it would therefore require about seven such engines to bring the centre down to the horizontal line.

It is unnecessary to attempt to prove, that a depression of four or five inches in the flooring of a flexible bridge, one thousand feet long, would produce no injurious effect—for that fact is demonstrated practically by every suspension bridge in existence. The Fair-

mount bridge sinks three inches and a fourth under a load of two tons, and is depressed by heavier weights six or eight inches many times daily, not only without injury, but without exhibiting any movement that would be in the least degree inconvenient under a railway train.

On the completion of the Freiburg bridge, it was frequently loaded, by concentrating people, horses and artillery on the flooring, until the central point was depressed *more than three feet*, without causing any injury to any part of the structure.

A wooden bridge, or any other bridge supported by a rigid framing, can rarely bend over one or two inches without suffering injury that will ultimately cause its destruction. But it is the peculiar merit of the suspension bridge, to yield slightly to the pressure of a passing weight, and return at once to its true position on the removal of the disturbing force. And this process may be continued thousands of times, with the certainty, known in advance, that the movement will occur, and that the structure will recover its place immediately on the withdrawal of the weight. It is no more injured by being depressed a few inches, than a flexible chain, stretched between two fixed points, is injured by a force which draws it slightly into another position.

But such is not the case with any other description of bridges. The bending of an ordinary wooden bridge proves the working of the joints, and shows

that it will sooner or later be racked to pieces. The integrity of such a bridge depends on the stiffness of the framing, and that stiffness must be preserved or the bridge will fall. [See Note A.]

But we have seen that a 20 tons engine will depress the flooring of this bridge  $3\frac{1}{2}$  inches, allowing nothing for the rigidity of its framing. And this amount of bending, it is concluded, could do no shade of damage, because it takes place daily on other similar bridges of much shorter span and does them no injury.

The flexure of this bridge, produced by the passage of a 20 tons engine, is less than that to which an ordinary steamboat is constantly subjected, without starting a timber or opening a seam. On the western waters of this country, boats 150 feet in length are frequently bent *ten or twelve inches* in crossing over shoals, without apparent detriment to their hulls or machinery. Yet this is a case in which a slight motion would be comparatively dangerous—for if the bending exceed what is due to the elasticity of the material, the joints must open, and the boat will sink.

But in this bridge there are no seams to open, no frames to become disjointed, or other danger to be apprehended; and the greatest possible movement of the flooring, due to the action of a single locomotive engine is less than that which actually takes place in the hulls of many approved boats, at almost every change in the position of the cargo.

The depression of the flooring of this bridge, under the weight of a 20 tons locomotive engine, is too small

to be detected by any but a practised eye, carefully placed to observe it. It is much less important than the oscillations and heavings of the cars in passing from one sill to another on any line of rail-road in this country.

The apprehension, therefore, that the bending of the bridge will present an impediment to the progress of the engine and train is entirely groundless. There is no sudden angle formed in the flooring, but the slight depression that really has place is diffused, in all cases, over at least half the length of the span. [See Note B.]

It has been already stated that the Freibourg bridge, a very slight structure, was repeatedly bent three feet by concentrating heavy weights on the platform. But on comparing the proportions of the Freibourg bridge with those of the work before us, it will be observed that the weight which would depress the former three feet, and which the calculation shows to be more than thirty tons, would produce a depression in the latter of only six inches.

It must surely be clear, then, that if a depression of more than 36 inches produced no injury in the weaker bridge, a depression of six, or any less number of inches, could not be hurtful to the stronger work.

The Freibourg bridge was bent more than three feet without injury, and this one might also be bent an equal or a greater amount, without the slightest cause for apprehension. But to bend the flooring of this bridge three feet—assuming still that the timber framing would present no resistance—would require



the concentration of a load of 200 tons in the centre of the arch, or ten locomotive engines of twenty tons each, to be piled one upon the other at that point.

This is a trial that few, if any, wooden bridges would bear: they would not only bend, but be broken in two; while on the bridge proposed, the only effect of the concentration of such a weight would be to increase the strain supported by each strand of wire about 50 pounds. But these strands would each bear an increase of nearly 1000 pounds, or 20 times this amount, before approaching the breaking point.

It thus appears that the effect of a weight which would crush any timber bridge that has yet been built, would be only to depress the flooring of this work an inconsiderable amount; and the moment the weight was removed, the flooring would return uninjured to its place.

When speaking of a depression of three feet in the centre of the arch, we are assuming an unheard of load—a weight of ten of the first class locomotives piled up on the centre of the platform. But no such load can be collected there. In practice the cars are stretched out upon the track, and a train of 200 tons would spread over a space varying from 500 to 800 feet.

Now there is a remarkable difference to be observed between the effect of a weight occupying the centre of the bridge, and that which would be produced by the same weight stretched along a considerable portion of the platform, in the manner of a rail-way train.

As an example of this difference, we may allude to the test weight placed on the flooring of the Fairmount bridge—a work which was slightly built and intended only for common travel, and on which no extraneous means are applied to add to that stiffness which it derives from its own inertia.

This bridge, as is shown by computation and experiment, is depressed  $3\frac{1}{4}$  inches by a load of two tons; or, by the rule laid down and verified by observation, it is about eight times as flexible as that in the plan before us. It would be bent just about as much under the weight of a loaded cart and one horse, as the heavy rail-road bridge, proposed for the Connecticut, would be under that of a 20 tons engine.

Yet, notwithstanding this flexibility under the action of small weights placed in the centre of the arch, that structure was publicly proved by placing on the platform *two columns* of loaded carts, each containing as much stone as the horses could pull upon the bridge, and drawn up as near together as they could stand, while the foot-ways were at the same time filled with people. With this weight, due to 39 carts, horses and loads, and probably reaching 120 tons, the depression of the flooring was not visibly greater than is frequently produced by placing three or four such teams in the centre of the arch.

After standing some time on the bridge, this train was started off simultaneously. No injury resulted to the structure, no motion was produced which would have been hurtful, if inconvenient, had the double train

of carts been a rail-way train; and the flooring immediately assumed its proper camber when the weight was withdrawn.

Let it not be supposed that it is the intention to represent this light and comparatively feeble bridge over the Schuylkill, as at all proper to be used, in its present condition, for modern rail-way purposes. It is wholly unfit for any such application. The example which it furnishes is intended only to illustrate the single fact, that although a small weight placed in the centre will produce a perceptible movement, a very great weight may be stretched along the flooring without causing any considerable increase of flexure.

The bridge which we are discussing will be five times as heavy as that at Fairmount, for each lineal foot, or fifteen times as heavy in the aggregate; and will require, as will be hereafter shown, eight times the weight to disturb it; and will be no more shaken by a twenty tons engine, than the other by a two and a half tons cart.

The rules upon which these computations are made, have been stated approximatively; but it may well be asked upon what authority they are laid down, and how their accuracy is to be verified.

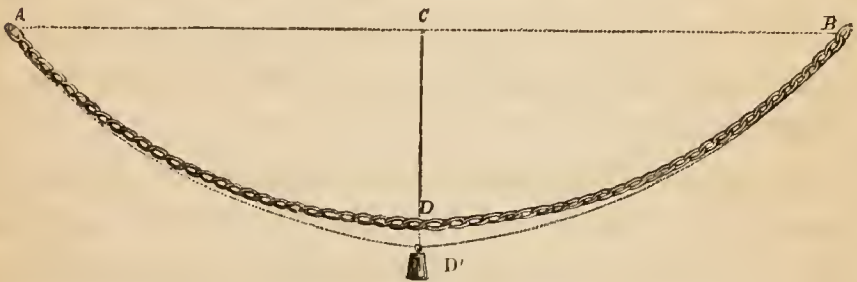
The scientific reader is referred, for a complete and beautiful analysis of this whole subject, to the "*Mémoire sur les Ponts Suspendus*," par M. Navier, late Chief Engineer in the *Corps Royal des Ponts et Chaussées*, and a distinguished member of the French Academy of Sciences.

But the complete investigation involves some very difficult and complicated mathematical formula, and will therefore engage the attention only of professional readers.

It is, however, fortunate, that the principal laws may be presented in a very simple dress, and in a shape which will permit their accuracy to be conveniently tested by every one.

Let any person, curious in these matters, take a chain of any length or size, and suspend it between two fixed points, A, B.

Let the number of links in the chain, as suspended from A to B, be counted. Let the distance, C D, showing the deflection, be also measured, by ascertaining the number of links which represents that distance.



Then let any number of links of the same chain be suspended to the point D, and that point will be found to have been depressed thereby the distance D D'.

Now, to calculate this depression, D D',—

Multiply the number of links in C D, by the number suspended at D, and divide by twice the number



of links in the whole chain from A to B. The quotient will be the distance,  $D D'$ , expressed in links.

The length of a link is the distance from the centre of one link to the centre of the next one to it.

This rule may be applied to all cases within the limits assigned, and approximatively to many cases beyond those limits; and it will always exhibit results which coincide very closely with experiment, if the experiment be accurately made.

It matters not what the size of the chain may be; from the finest watch chain up to the heaviest cable, or cable bridge, this rule will hold good, and be accurate enough for almost all practical purposes, until the bridge is stiffened by timber or other trussing, when the actual movements will become less than those exhibited by the calculation. Indeed, in nearly every case, within the limits specified, the errors which exist in the computation are on the safe side—since the calculated depression is generally a fraction greater than that which really occurs.

Now, let it be observed, that according to this rule, the depression produced by a given weight will be less and less as the weight of the chain becomes greater and greater.

Consequently, *if we double the span*, or put two chains in place of one, or double the weight of the links in the same chain, leaving all other things the same, the depression will be reduced one-half. In other words, the stability of a suspension bridge of great span depends mainly on its weight.

We must bear this fact in mind—that it is essentially *weight* which gives solidity and firmness to the structure. It resists by its inertia, and its inertia is in proportion to its weight.

To apply this principle to bridges, we have the case of a small foot bridge formerly stretched across the Schuylkill at the Falls, and sustained by six strands of wire. This work was exceedingly light, and it was also exceedingly flexible; so flexible, it is said, that few persons could cross it without fear.

The Fairmount bridge, of about the same span, and over the same stream, does not bend injuriously under the heaviest teams and droves of cattle, and has sustained safely more than 100 tons.

Now, why did the foot-bridge bend fearfully under the weight of a man, while the Fairmount bridge sustains safely the weight of two thousand men?

It is simply because the Fairmount bridge is heavier than the other—heavier, indeed, when thus loaded, as compared with the weight of a four-horse team, than the slight foot-bridge, as compared with that of a man.

If this is the reason, and this the principle, why may we not extend the application to cases of higher importance; and by making a bridge ten times as heavy as that at Fairmount, make it capable of bearing a twenty tons engine as lightly as the other bears a two tons cart, and capable of bearing the action of a train of 1000 tons as easily as the other bore a train of 100 tons?

There is no difficulty in this. All we require is more material, judiciously applied.

But have we provided that material in this plan?

The cables of the Fairmount bridge were composed of about 2500 strands of No. 10 iron wire, and those of the bridge before us will contain *twenty-four thousand strands of the same wire*.

To move the Fairmount bridge, or send a vibration through it, 135 tons must be put in motion. To disturb the bridge which it is proposed to build at the "Narrows," 2000 tons must be displaced.

But the relative stability of the two works is greater even than their relative weights, per lineal foot, would indicate; for the cables hang much more loosely on the Fairmount than is proposed for the Middletown bridge—another element in such computations.

#### OF THE STIFFNESS OF SUSPENSION BRIDGES AS DEPENDENT ON THE FORM OF THE CURVE OF THE CABLES.

The stiffness of the bridge depends, as already observed, mainly on the weight, when the proportions are constant; but when the weight is the invariable quantity, the stiffness depends mainly on the form of the curve which the cables are permitted to assume.

For example:—If we have a bridge of any given weight and span, in which the deflection of the cables is any given amount, and place a given load in the centre of the arch, and note the depression; then draw the cables more tightly, so that the deflection is reduced to one-half its previous value, and apply the

same weight, we will find the depression also reduced about one-half.

The tighter the cables are drawn the more stable will be the structure, until we approach the point where the increased tension due to the load causes an elongation in the material itself, of which the cable is composed, sufficient to compensate for what is otherwise gained in stability.

The total deflection, due both to the change of figure and the elongation of the material, is a problem which only comes up with any interest, when the proportions of the bridge transcend the limits assigned for the application of the rule already given.

In these cases, when they occur, the results may be obtained with all desirable exactness, and over a wider range, by the application of a not very complicated formula. (See Note C.)

But, apart from these questions, which involve the extensibility of the material, and keeping still within our limits, it will be observed that the depression produced by a given force is directly as the original deflection. Hence, when we compare the movement which takes place under the action of a given weight on the Fairmount bridge, with that which will result from the same weight on the Middletown bridge, we must take into consideration this difference in their respective proportions.

Each foot in length of the Middletown bridge is five times heavier than a foot in length of the other. From this cause, therefore, the movement of the Fair-



mount bridge, consequent on the application of a given force, will be five times the greater.

But the deflection of the cables in the Fairmount bridge is 60 per cent. greater, in proportion to their span, than the deflection of those in the Middletown to the span of the latter work. From this cause, also, the stiffness of the latter will again be 60 per cent. greater than that of the former.

The combination of these two elements shows that the Fairmount bridge will be bent *eight inches* by a weight, which, on the proposed bridge, would produce a movement of but *one inch*. And we have already seen, that a train of more than one hundred tons, drawn by horses over the rough planks of the former, produced no injurious effect. It is clear, then, that a bridge, of which the cables are nearly ten times as strong, and which is eight times more stable, cannot be injuriously affected by the same weight, which in fact exceeds that of the ordinary trains likely to traverse the proposed road.

We have now gone through this branch of the subject, and have given full evidence of the sufficiency of the bridge proposed, so far as the considerations of strength and stability are at issue. This evidence is presented in the most conclusive form, by laying down rules for computing all the essential strains and motions, to which the work will be subjected in rail-road service: by showing how the accuracy of the rules may be tested, and inviting the experiment to prove their correctness: by giving the calculated deflections

under given circumstances, and comparing them with the observation, and showing that the results fully confirm the calculations: by extending these computations from the finest guard chain up to the heaviest wire bridge in the country, and showing its confirmation throughout. (See Note D.)

#### OF THE EFFECT OF SPEED.

We have not yet made particular allusion to the effect of speed; and it may perhaps be supposed, that the oscillations produced by trains in motion are not reached by the methods of calculation presented, and that this bridge, however it may be adapted to the support of stationary weights, may not be applicable to the high velocities contemplated on the proposed "Air Line Road."

On this head, it might be sufficient to compare the loss of time incident to slackening up the speed of the engine while passing over a space of 1000 feet, with that which would be consumed by crossing on the low level, at a sacrifice of 100 feet total ascent and descent.

By reducing the speed on the bridge to the rate usual on long wooden bridges—or from 25 miles down to some 5 miles an hour—the loss of time in crossing at the Narrows, would be less than two minutes.

But the loss of time in merely crossing the draw-bridge above Middletown, due to the same reduction of speed, would be  $3\frac{1}{2}$  minutes, or as the bridge is nearly twice as long, the loss of time would be nearly

twice as great. But, in addition to this advantage on the score of time, the high level avoids the delay consequent on a total ascent and descent of 100 feet at every trip, besides the occasional delays incident to the simultaneous approach of vessels and trains at the same passing point. The advantage in time is, therefore, all in favor of the high level.

But there is another consideration. It is an error to suppose that the depression of the bridge is greater when a body of a given weight is moving along it, than when the same body is resting quietly on the platform. Careful experiments have been made on the Fairmount bridge, which seem fully to prove that the depression produced in the flooring by earts moving over the rough planks, is *considerably less* than that produced by the same carts when standing quietly in the centre of the arch.

I have caused a spirit level to be placed on the shore near the bridge, and the height of the flooring to be accurately ascertained; then, two earts, of which the total weight was four thousand pounds, to be brought on the bridge, and stood opposite the apex of the curve, and the corresponding depression accurately noted.

The same earts, horses and loads, were then driven over the bridge in the same relative position, and the depression again noted.

*The depression of the bridge was less when the carts were in motion, than when they were at rest on the flooring.*

Similar experiments have been tried on the flexure of rail-way bars, by placing an accurate instrument half way between the bearing points of ordinary T rails of different patterns, and observing the deflections produced by the drivers of locomotives at rest, or moving over the rails at moderate and high velocities.

In this case, the depression was not less when the engine was running, but, so long as the fixtures remained secure, appeared to be essentially the same as when the weight was standing quietly on the bar.

Similar experiments have been tried on a wire cable 1160 feet in length, stretched across Niagara river, and used as a ferry, by suspending a small ear to sheeves, which traversed along the top of the cord. The oscillations of this cord, at the point of suspension of the ear, were almost too small to appreciate, while no difference could be observed between the depression occasioned by the ear, when at rest and when in motion.

#### OF THE STIFFNESS OF THE TIMBER IN THE FLOORING.

In all that has preceded, the computations and conclusions have been based on the supposition, that the flooring of the bridge is entirely flexible, and without strength, acting as it would act if the girders were sawed through and through in numerous places along its length. And it has been shown, that in this hypothesis the bending would be so small under the hea-



viest weights, that the platform might be traversed safely by rail-way trains of extraordinary size.

But this flooring is not so divided, and so entirely feeble; it is on the contrary a very strong and rigid structure.

The arrangement of the timbers is shown in the annexed engraving.

Joist or cross beams, 12 inches wide by 24 inches deep, and  $28\frac{1}{2}$  feet long, are suspended from the cables at every 5 feet, measured along the platform.

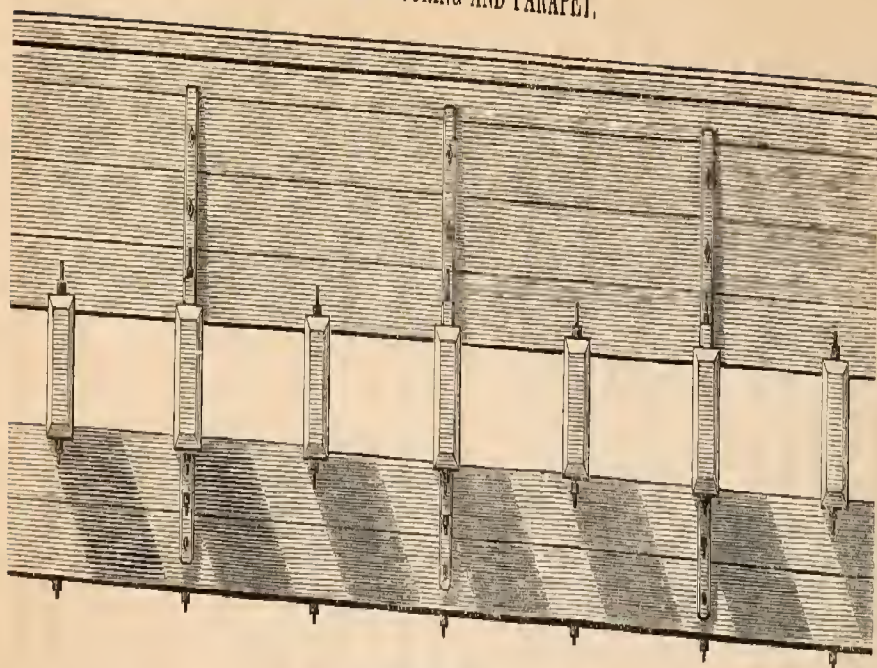
At the distance of ten feet from the centre, on each side of the bridge, is raised a heavy parapet, or wall of timber, four feet high and 12 inches thick. These parapets are built so as to constitute of themselves heavy girders, running from one end of the bridge to the other, and distributing the effect of every load over a considerable portion of the flooring.

They are each composed of a string of 12 inch square oak timber, laid on the cross joists, upon which they are fitted by a gain of two inches. They will be 40 feet long and properly scarphed at the ends.

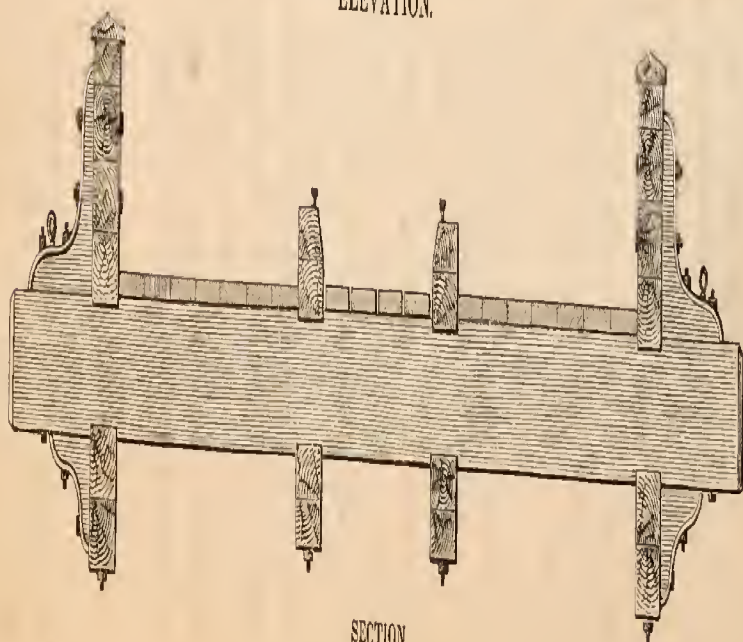
On top of these timbers, and parallel with them, is placed a second course of the same size, closely fitted to, and breaking joints with the former, with which they are united by tree-nails. On top of these last timbers is laid a third course of the same size, and secured in the same way to the lower courses.

On top of this third course is laid another string, 12 by 8 inches, still breaking joints with, and tree-nailed to the lower courses.

PLAN OF FLOORING AND PARAPET.



ELEVATION.



SECTION.

The whole is covered with a four inch eap or eoping, breaking joints as before and secured to the preceeding courses. To preserve this parapet in its vertieal posture, and guard against lateral movements, staunch wooden knees are bolted on the outside, both to the parapet by horizontal bolts, and to the cross joists by vertieal bolts.

Directly under each of the parapets is placed a longitudinal girder, formed in the same way, but composed of only two 12 inch square timbers, which break joints with each other, and are let into and fitted upon the under side of the joist by slight gains cut in the upper side of the girder.

Heavy iron serew bolts are finally driven through the parapet, cross joists, and lower girder, by means of which the whole are drawn and secured firmly together.

The rail-way track occupies the centre of the platform; the rails are supported by girders formed of two courses of 12 by 9 inches stuff, breaking joints with each other, and let down upon the joists, as already described for the parapets; and the platform is still further stiffened by similar girders corresponding with the line of the rails on the lower sides of the joists.

The platform is then covered with three inch oak plank. The rail-way track with flagging.

There will be 35 cubic feet of oak timber in each lineal foot of the flooring.

No calculation is presented here of the strength of this

platform, or the resistance which it will offer to the weight of the trains; but it is submitted to the practical reader to decide, whether a flooring bound together by such girders, firmly secured at the ends to the massive abutments by staunch iron ties, and thoroughly bolted in the manner here specified, will not offer *some* resistance.

It will be recollected that the centre of the arch cannot descend, or yield to a weight placed there, without drawing up the flanks; and the flanks cannot rise without bending this timber framing.

Still, no estimate is made of the value of this stiffness, or of the reduction in the movement which might be expected in consequence thereof. The bridge is strong enough, and stable enough, for all the purposes of this rail-way, without counting on any collateral aid.

#### OF THE EFFECTS OF THE WIND.

It is not difficult to show by direct computation, based on the measured force of wind during violent storms, that no danger is to be apprehended to this structure from any such cause. But we have examples enough to spare us the trouble of the investigation.

The effect of the wind is not, as might be supposed, to produce a horizontal oscillation in the flooring. This movement is prevented by the mode of suspension, which swings the bridge in the manner of a hammock, so that it is guyed or stayed by its own weight,



and by means of the very cables that uphold it. These cables do not swing in vertical planes, but are inclined from the summits of the towers towards the axis of the bridge, and act as constant and effectual lateral supports.

The only motion that is ordinarily perceptible in high winds—and which is manifested, perhaps, more remarkably in the Menai than in any other suspension bridge—is a vertical oscillation, or waving of the flooring, produced by the upward pressure of the currents of air passing at high velocity under the bridge.

The flooring of the Fairmount bridge is 30 per cent. wider than that proposed for the Air Line road; and the wind consequently acts on the former with 30 per cent. more power than on the latter, in producing this motion. But, on the other hand, the flooring of the Middletown bridge is four times as heavy as that of the Fairmount, and it resists, consequently, with four times as much power.

The effect of the wind is not at all hurtful on the Fairmount bridge, and it cannot therefore be injurious on a work which offers four times as much resistance, and on which the force applied is a great deal less.

The width of the Freibourg bridge, of which the flooring is more than 800 feet long, is almost precisely the same as that of the work proposed. The wind, therefore, acts upon both with nearly equal power. But the proposed work is nearly six times as heavy as the Freibourg, and consequently opposes the same force of wind with six times as much resistance.

In fact, all fears on this score for a well constructed bridge are entirely groundless, whether the span be long or short.

#### COMPARISON BETWEEN THIS AND OTHER SUSPENSION BRIDGES.

To be able to form a better judgment of the relative strength of the bridge here proposed, and that of other well known structures, it will be useful to bring their principal dimensions together.

The nearest approach to this work is perhaps the Freiburg bridge, a structure which measures 889 feet between the points of suspension. This remarkable edifice was designed exclusively for common road purposes; and though forming part of one of the highways leading into a city of eight or ten thousand inhabitants, it is very slightly and cheaply built.

Its weight is 740 lbs. per lineal foot, and no outlay has been permitted in any part of it for the purpose of adding to the stiffness due to its weight.

This bridge is upheld by four cables, each formed of 1056 strands of wire, and containing, altogether, 4224 strands.

In the rail-way bridge under discussion there are to be *twenty-four thousand strands*, or nearly six times as many.

But the Freiburg bridge was twice crossed on the day of its inauguration, by 300 soldiers in rank, marching to military music; a weight of about 20

tons concentrated in a point, and striking the platform with the momentum due to the height of the tread.

On the previous day the flooring of the same bridge was occupied by about 2000 people, also marching in procession, and keeping time to music.

The weight on the platform, at this time, must have been at least 120 tons; but it was not concentrated in a point, as before, and it produced no damage or inconvenience, although it occasioned a very sensible horizontal oscillation.

Now the load sustained on this occasion was considerably greater than that of the heaviest freight trains likely to pass over the Air Line road, while the bridge proposed for the Air Line road is nearly six times as strong, and seven times as stable as this. No doubt can therefore be entertained of the sufficiency of the proposed work.

The next most important of the existing structures of this description, is the Menai bridge, concerning the fitness of which, for rail-way purposes, there has been some discussion among gentlemen distinguished in England both by official eminence and well merited reputation.

It is not necessary to discuss that question here, though it cannot be denied that there are peculiarities in this work so evidently calculated to impair its applicability to rail-way service, for which it was never intended, that its rejection by the engineer of the Holyhead road should occasion no surprise. The weight

of this bridge is 2483 pounds per lineal foot, and the span about  $13\frac{1}{2}$  times the sagitta.

The Middletown bridge, as designed, will be 64 per cent. heavier, and for that reason 64 per cent. more stable than this; and the ratio of the span to the deflection of the cables 30 per cent. greater, and the structure from this cause also 30 per cent. stiffer.

In other words, by carrying out the computation, it is found that a weight which would depress the flooring of the Menai bridge *seven inches*, would depress that at Middletown but  $3\frac{1}{2}$  inches—making the calculation with reference only to the weight of the bridge and the form of the curve.

But there are other arrangements which influence the stability of the Menai bridge in an extraordinary degree, and produce a depression greatly exceeding that which this computation will exhibit. The chains which pass over the supporting towers of that work rest, as these do, on cast iron saddles, which move upon their summits and relieve the masonry of all appreciable horizontal strain. But the stays which pass from the saddles to the anchorage are of extraordinary length—considerably exceeding, in their total development, the whole length of the supporting chains.

The consequence of this feature of the plan is obviously a very great increase of deflection caused by the straightening out of these guys, as well as by the stretching of such great length of material, when extraneous loads are brought on the platform. When the weight comes on the flooring, the guys yield, the



saddles move, and the supporting chains of the bridge are necessarily lengthened. A movement here of only one inch on a side, produces a depression of *five inches* in the centre of the arch, in addition to the depression already considered.

The Menai bridge, although substantially built, and as the first great structure of the kind, a remarkable work, is by no means well adapted to conversion into a rail-way bridge—needing a considerable addition to its weight, heavy longitudinal girders for the purpose of distributing the pressure of concentrated loads over a greater length of platform, and a total change of the method of staying, to reduce the range of the movement of the saddles.

Another bridge, which has also acquired some little, but most undeserved notoriety, in consequence of the arguments which have been drawn from it adverse to the adoption of suspension bridges for rail-way purposes, is a small bridge over Tees, on the Stockton and Darlington rail-way.

This work was formerly used for the passage of rail-way trains, but was found to be utterly inadequate to the duty, and was replaced by another under the direction of Mr. Stevenson, an eminent English engineer.

In a report to the directors of the Hollyhead rail-way, Mr. Stevenson has attempted to justify the expenditure of some two millions of dollars in the erection of a tubular bridge over Menai, by an argument based on the failure of this insignificant structure.

The work which failed was an ordinary suspension bridge, possessing no more strength than was proper for ordinary road purposes, and built, or applied, apparently without any knowledge of the principles upon which this whole question turns.

But what is curious in this matter is the fact, that the bridge by which this imperfect structure was replaced is itself a suspension bridge, but stronger, heavier, better secured, and throughout more judiciously planned for the locality.

The weak and slender bridge failed, because it had not material enough either in the flooring or chains, and because the chains hung too loosely to prevent hurtful oscillations.

The second work succeeded, because the structure is heavier, the chains drawn tighter, the iron girders well bolted down, and sensible measures taken to secure success.

The second bridge, though firm enough for security, also oscillates considerably under the heavy engines in use on that great road; but it might easily have been made still stronger, still heavier and still firmer.

Without meaning to justify the extravagant expenditure that is encountered in the tubular bridge over Menai—three-fourths of which could have been saved by a substantial suspension bridge—we must look with some indulgence upon the feeling that prompts an able man, conscious of his power, to strike out a new path to distinction and seek for argument to justify it. We

must judge of such things, too, with an allowance for the peculiar views which prevail in England, where engineers, directors, and even stockholders, are prone to encourage extravagant outlay, for the purpose of excelling in magnificence and originality, and where those practical views of economy and usefulness which here constitute the criterion of excellence, are less stringently enforced than on this utilitarian soil.

#### ESTIMATED COST OF THE BRIDGE.

We have now gone over every branch of this subject, which appears to be essential to the establishment of the entire feasibility of constructing a safe and sufficient rail-road bridge, across the Connecticut at the point in question. It remains only to ascertain the probable cost of the work, that we may be able to judge whether the advantages of the structure are sufficient to justify the expense of its erection.

On this head, also, we have ample experience, and there is no part of the edifice of which the cost may not be estimated with sufficient accuracy for the security of a contractor, or the necessities of a company.

The cost may be stated to be as follows:—

#### *Estimate.*

1,560,000 pounds of No. 10 iron wire, including the cost of manufacture, raising and adjusting, at $10\frac{1}{2}$ cents,	-	-	\$163,800
45,000 pounds of No. 12 wire in the suspenders, at 12 cents,	-	-	5,400

220,000 pounds of bar iron, in anchorage,	
at 7 cents, - - - - -	15,400
35,000 pounds of bar iron, in bolts, for	
flooring and parapets, at 7 cents, -	2,450
60,000 pounds iron castings, for saddles,	
rollers, plates, &c. &c. at 4 cents, -	2,400
35,000 cubic feet of timber, in the flooring	
and parapets, including framing and	
raising, at 30 cents, - - - - -	10,500
1,000 cubic yards of rock excavation, in le-	
velling foundations, at \$1.00, - -	1,000
1,500 do. in fastening chambers, at \$4.00,	6,000
5,600 perches of masonry in the Egyptian	
towers, at \$5.00, - - - - -	28,000
14,000 do. in abutments, and fastening	
walls, at \$3.00, - - - - -	42,000

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\$276,950

Or, making still further allowances for contingencies, the round sum of \$300,000. (See Note E.)

This estimate is sufficient to cover every expense that can reasonably be expected to occur, and leave ample margin for the protection of the contractor. It may be regarded as the proposal of the writer for the work, if a contract should be desired.

#### CONCLUSION.

It is to be borne in mind, that the structure to which this estimate applies is of extravagant proportions, and



far exceeding in strength the real wants of the Company. A bridge abundantly sufficient for all the duties of the Air Line Road, can be erected for little over \$200,000; but, as before stated, the Committee desired to have a plan prepared for a bridge of the first order, and equal to all the duties of a great rail-way.

But taking the structure as it is, with all its excess of strength and weight, the cost would appear to be small in comparison with the evils which will be entailed on the city of Hartford, and all the country bordering the Connecticut, from Middletown to the head of navigation, by the adoption of the plan proposed by the Rail-road Company, and resisted by the great interests which it threatens. These evils will consist—

1. In the injury to the navigation consequent on the construction of the bridge, and the effect of that injury upon the commerce and value of property, as far as this injury may reach.

2. The loss of time to all who may hereafter traverse the Air Line Road, by the necessity of a total ascent and descent of 100 feet at every trip of every locomotive passing over the line.

3. The injury to the public, consequent on delays at the draw, which might, of course, be avoided by the higher level.

These are evils which certainly ought only to be encountered under manifest and absolute necessity. No ordinary consideration could justify a company in violating the vested rights of a population of more than 100,000 inhabitants, or putting their common earnings

and inheritances in jeopardy. No ordinary considerations could justify a legislature in giving the sanction of LAW to such a violation.

But does that necessity exist in this case? Can it exist, when a structure which involves none of these risks, and none of these certain or contingent evils, can be built for a sum utterly insignificant in comparison with the value of the injury apprehended from the work proposed?

It is vain to urge the necessity of the measure on the ground of the impracticability of the alternative plan—a high bridge at the Narrows. Its practicability can be established by its achievement long before the rail-way will be in a condition to use the work.

It cannot be, then, on the basis of public necessity, or public convenience, that this new road will be permitted to impede the most important entrance into your prosperous city, or to stop up the principal avenue of your commerce and thrift.

The *necessity* does not exist, and the superior convenience is attained by adopting a high level, which leaves the navigation open. There is no motive here, founded on public utility, or defended by the public interest, which can sanction the placing of any impediment in the channel of this stream. And it is not to be supposed, therefore, that the Legislature, under such circumstances, will consent to relieve this Company, important as its intentions may be to the general welfare, from a trifling outlay, at the expense of Hartford—sacrificing existing vested rights of known and

real worth, interests that have been built up by years of industrious enterprise, to any speculative object, however laudable, of which the actual value is yet to be submitted to the test of experience.

The great highways that lead into a city are the arteries which flow from the fountains of its wealth; and it is the characteristic of modern legislation to open these channels through hills and over valleys, that the products of the widest possible areas may pour through them into the markets which the cities furnish. The generous emulation which grows up between neighbouring towns promotes that result, and quickens all the elements of prosperity and trade.

To stop up or impede these channels—to cut off the natural inlets of wealth, which have actually given existence to the cities and sustained their growth—are measures which might have been entertained in an earlier age and under more arbitrary governments. But in this country, almost every effort having in view the obstruction in any degree of important navigable waters, has been resisted strenuously and desperately by the towns above the site of the contemplated work. It is, of course, they who are to bear the burden that can best judge of its weight.

Every application of the city of Wheeling for a bridge over the Ohio has been contested by Pittsburg, and successfully contested, until the present Company adopted the bold plan of spanning the stream by an arch of more than one thousand feet, placed quite beyond the reach of steamboats.

The application for the right to build a bridge at Cincinnati, having a pier in the water, has been resisted successfully by the river interest; and will doubtless continue to be resisted, until the applicants decide on a stupendous arch of 1400 or 1500 feet span, and study to promote their own great interests and most laudable wishes, without invading the existing and superior rights of others.

Albany has long sought to place a bridge over the Hudson, and has been desperately and successfully resisted by Troy; and this resistance will certainly continue until a plan is adopted which will attain this most desirable object, and at the same time protect the vast interests created by remarkable enterprise, at the head of that great navigation. (See Note F.)

The Schuylkill *was* obstructed for the convenience of a rail-way, which has added little or nothing to the commerce of the city, while it has annoyed and irritated a large population whose rights were unnecessarily impaired, and whose property was heedlessly injured.

The Susquehanna has not yet been obstructed by a bridge at Havre de Grace; but the application has been made and a contest has of course commenced between the petitioners and the village of Port Deposit. The interests above this bridge are comparatively small; but they are of great value to their owners, and so far have received the protection of their Legislature.

There are circumstances, most undoubtedly, where compensation could be made, in which some sacrifice



of an existing interest may be justified for the obvious purpose of promoting a greater public good.

But yours is no such case as this. The land interest and the water interest—the interest of the public using the rail-way, and that of the greater public navigating the Connecticut—are here identical; for the plan that protects the navigation, and leaves the highway which Nature prepared without obstruction, is also that which offers the best line to the land travel; thus giving to the Legislature, the appointed guardian of their respective rights, the double motive for protecting both. Two such interests will certainly not be recklessly violated for any speculative object.

The navigation of the Connecticut above Middletown, is superior to that of the upper Mississippi or Missouri, or any of their branches. It is, in fact, superior to that of any river between the Alleghany and the Rocky Mountains, north of the Ohio. The water is deeper at its lowest stage, steamboats of greater draught traverse it, and the country through which it flows is more densely peopled, and the land more highly cultivated, than that which borders any of the western waters. And there is certainly no city or population which would seem to present a stronger claim upon the just protection of the Legislature and people of Connecticut, than the city and population of Hartford, the staunch opposer of this assault upon her prosperity.

But no party would now be so idle as to ask to place a draw-bridge across the Ohio or Mississippi; no law

could be obtained for such an obstruction, and nothing is hazarded by the assertion that such a nuisance would be immediately overthrown, if placed there under the colour of any law. The bridges that are established on those streams, must be placed high enough to clear the steamboats, and must leave the channel open.

These views are submitted with more than the interest which usually attaches an engineer to a professional subject. The wrong which has been threatened, the danger which is still pending over your prosperity, are so manifestly the result of inadequate reflection, and so easily and cheaply to be obviated, that it would be difficult to refrain from the expression of an opinion on that head.

Trusting that these evils may be averted, and your commerce and rights, and all the grave interests of the public protected,

I have the honour to be,

Gentlemen, your ob'dt serv't,

CHARLES ELLET, Jr.

*Civil Engineer.*

*Philadelphia, April 23d, 1848.*

## POSTSCRIPT.

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Since receiving the first proof of the foregoing report, I have seen a copy of a pamphlet, entitled, "A Reply to the Statement of the Citizens of Hartford, &c.," in which I find the following passage:—

"The Hartford statement, with great want of candour, endeavours to communicate the impression that a rail-road suspension bridge is feasible here, by asserting that such a bridge, for the same purpose, is in progress of erection across Niagara river, with a span of 800 feet. And also that a suspension bridge is being built across the Ohio, at Wheeling. As to the latter, it is sufficient to say, that it is not designed for rail-road use; and in respect to the former, the public ought to know that it is incomplete (a single cable only having as yet been stretched across the chasm, on which the architect is able to swing across in a basket without breaking it down), but further, that the entire project is not only looked upon as chimerical, *but the capital itself is not subscribed.*"

To so much of this extract as is merely intended to discredit a noble enterprise, in which I have embarked

my reputation, and, to a considerable extent, my property, I do not deem it essential to offer any reply.

There has been opposition to that work also, and there are parties who have supposed that their interests would be promoted by representing it as “chimerical;” and this misrepresentation has been a greater obstacle to the Company, than any they will ever find in the natural difficulties of the undertaking.

The assertion that “the capital itself is not subscribed,” is a very great error, and one which I regret to be compelled to say, rests on no foundation whatever. *It has all been subscribed*, and the Company have entered into a solemn contract with me for the execution of the work, by which they stipulate to furnish the funds as fast as I need them. The work is now in full progress.

Among the Directors and Stockholders are some of the wealthiest and most influential citizens in Canada West and Western New York.

C. E. Jr.



## NOTES.

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### NOTE A.

There are few wooden bridges of which the deflection is less than one inch under the weight of an ordinary locomotive engine and tender. I have caused various experiments to be made on this subject, and have been favoured by intelligent engineers with their observations—which prove that a deflection of one inch under heavy engines is almost universal in bridges of only 130 or 140 feet span.

A depression of  $1\frac{1}{2}$  inches is frequent, and from two to three inches occasionally occurs.

A wooden bridge of 100 feet span is ordinarily more strained by a weight which produces a depression of one inch, than a suspension bridge of the same span would be under a depression of a foot; or one of a thousand feet span by a depression of many feet.

A depression of one inch in a bridge of 100 feet span is just as important as one of 10 inches in a bridge of 1000 feet span.

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### NOTE B.

It can only be in extreme cases, where very heavy trains are conveyed, that the change of grade due to the flexure of the bridge could be an object of any moment; and never until the inclination surmounted by the engine, exceeds the maximum grade ascended by the same engine on other parts of the road.

In the centre of an arch of great span, the flooring offers, comparatively, little resistance; but near the ends, where secured to the abutments, its stiffening effect is greatly increased.

In a weak bridge, or even in a strong one, overloaded, the first dif-

feulty would be experienced in passing from the bridge to the abutment; but, even in very flexible structures, this trouble may be in great measure counteracted by the arched form of the flooring.

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### NOTE C.

The following is the formula alluded to in the report, which being an approximate result obtained by developing a series, and suppressing the higher powers, must be applied with caution, and not much beyond the limits assigned; and in the application care must be taken not to vitiate the results, by introducing the elasticity of the links in an ordinary chain. Represent by

$h$ , half the span of the arch;

$l$ , one-half the length of the cables, from the apex to the anchorage;

$f$ , the deflection;

$a$ , the cross section of the cables in square inches;

$p$ , the constant weight of the bridge per lineal foot;

$\pi$ , the weight placed in the centre of the arch and causing its depression;

$\delta$ , the value of this depression.

We shall then have this approximate formula,—

$$\delta = \frac{\pi}{4} \left( \frac{f}{p h} + \frac{l h^2}{900 a f^2} \right)$$

to express the depression of the central point, due at the same time to the change of the figure and the extension of the material of the cables.

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### NOTE D.

It was the intention to offer, in a note, some cases of this sort; but the experiment is so easily tried by every reader, that it is deemed unnecessary to swell the report for that purpose.

## NOTE E.

The quantity of masonry exhibited in this estimate is greater than would be really necessary for the execution of the work. The object not being to limit the cost to any very low figure, considerable latitude has been taken in adjusting the proportions, with a view to symmetry and effect.

The price affixed to the masonry of the abutments and wings may appear small; but as a considerable portion of the work consists of a massive wall which supports nothing but the rail-way track, it will, I doubt not, be found sufficient.

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 NOTE F.

There is, perhaps, no place in this country where the erection of a bridge across a navigable stream can be sustained by as great an interest, and as many important considerations, as at Albany. But the people of New York have hitherto respected the prior rights of Troy, and prohibited the obstruction of the navigation, to which that flourishing city owes its existence and prosperity. Nevertheless, the want of a bridge at Albany is becoming annually more imperative, and on the completion of the Hudson river rail-road, and the numerous great lines and tributaries progressing in the West, a bridge *will be made* across this stream, on some plan which will leave its navigation free.

It is perfectly practicable for all the rail-roads to maintain a high level at Greenbush, and crossing on a suspension bridge, raised entirely above the masts of all the shipping—leaving the whole channel clear—land their passengers in the heart of the city, and in a station worthy of the great interests that would be there concentrated.

In this case, as at Middletown, the same plan would obviate a considerable portion of the ascending grades, leading up from the river in each direction, and by promoting the common interests of the several roads and the city of Albany, justify a combination of all their strength to effect the object.

The cost of such a work would certainly be large, but nevertheless, it must, sooner or later, be encountered.





